

CHAPTER 11

- Reading 11.1 **Slater, E. and Roth, M. (1969). *Mayer-Gross, Slater and Roth: Clinical Psychiatry* (3rd edn.). London: Balliere Tindall and Cassell.**
- Reading 11.2 **American Psychiatric Association (1994). 'Introduction' in *Diagnostic and Statistical Manual* (4th edn) Washington, DC: APA, pages xv–xvi**
- Reading 11.3 **The opening paragraphs of Chapter 1, Signs and Symptoms of mental disorder, in Gelder, M.G., Gath, G., and Mayou, R.A.M. (1983). *The Oxford Textbook of Psychiatry*, (1st edn). Oxford: Oxford University Press, p 1**
- Reading 11.4 **The opening sections of Sigmund Freud's *Project for a Scientific Psychology*, (Standard Edition), Vol. I. London: Hogarth Press, pages 294–297.**
- Reading 11.5 **Freud, S. (1977). 'Dora' case study. In *Case Histories I*. The Pelican Freud Library, Vol. 8. London: Penguin.**
- Reading 11.6 **Freud, S. (1977). 'Dora' case study. In *Case Histories I*. The Pelican Freud Library, Vol. 8. London: Penguin, pp 60–63.**
- Reading 11.7 **Freud, S. (1977). 'Dora' case study. In *Case Histories I*. The Pelican Freud Library, Vol. 8. London: Penguin.**
- Reading 11.8 **Ricoeur, P. (1970). Energetics and hermeneutics in *The Interpretation of Dreams*. In *Freud and Philosophy* (trans. D. Savage). London: Yale University Press, Book II, chapter 2, pp 87–93**
- Reading 11.9 **Grunbaum, A. (1988). *Precis of The Foundations of Psychoanalysis*. In *Mind, Psychoanalysis and Science*, (ed. P. Clark and C. Wright). Oxford: Basil Blackwell, pp 10–13.**
- Reading 11.10 **Gardner, S. (1993). *Irrationality and the Philosophy of Psychoanalysis*, Cambridge: Cambridge University Press, pp 131–137**

Reading 11.1**EXERCISE 4**

2 Extracts from: Slater, E. and Roth, M. (1969). *Mayer-Gross, Slater and Roth: Clinical Psychiatry* (3rd edn.). London: Balliere Tindall and Cassell.

Extract 1 (from the end of the preface): pages xiv–xv

Of little less importance is the progress which has been made in physical methods of treatment, with which have gone advances in defining the physiological and biochemical bases of mental disorder. In retrospect, it can be seen that at the time of our first edition we already stood on the brink of a psychopharmacological era. A wide range of substances has been developed for the treatment of schizophrenia, paranoid, depressive and anxiety states. In many instances their efficacy has been established with repeated controlled trials. In addition to transforming therapeutics in psychiatry, and bringing an increasing range of psychiatric disorders under observation, the study of the new psychotropic drugs has stimulated the development of heuristically valuable hypotheses about the biochemical mechanisms underlying mental disorder. This has not been without setbacks. Until a few years ago it looked possible that the biochemical basis of schizophrenia would be clarified in the foreseeable future. These hopes have faded. However, in the last decade, a far more promising prospect has opened up in the depressive disorders; the evidence that the metabolism of catechol- and indol-amines is in some manner associated with depressive symptoms comes from a number of independent sources. It would be surprising if important further advances were not made in this field within the next decade; and if they came they would have farreaching effects on our thinking about the commonest forms of psychiatric illness.

Another new arrival is the methodological advance by which it is becoming possible to use for scientific purposes the great mass of information which is assembled by the psychiatrist about the individual patient. We are now in a position to apply advanced techniques such as multivariate and principal component analysis to large bodies of case material. The impact of these developments has so far been most marked in the fields of classification and diagnosis; but we are at the brink of a revolution that is likely to transform psychiatric research as a whole.

What then, one might ask, is the future of clinical psychiatry with all these vigorous newcomers in the field? The most significant result of all the self-criticism to which psychiatry has subjected itself, the scepticism of the value of the clinical interview, of psychiatric history-taking, of the validity and function of diagnosis and the clinical criteria on which diagnostic judgements are based, all these and the vast amount of scrupulously conscientious work which has been invested in them, have led to the validation of the basic clinical methods and clinical concepts. Effective support has not been found for the idea that mental illness is a myth, and the literature of clinical psychiatry a mythology. Attempts to find substitutes for diagnostic classifications in cluster

analysis of psychometric ratings have led back once again to the Kraepelinian groupings. On our last edition, one of our reviewers wrote: 'Those who believe that all human behaviour, whether normal or abnormal, is both meaningful and purposive, will have little sympathy with the authors' views'. We are content that this should be so, since the number of those who share that belief must be diminishing day by day.

It has been the task of the two revising authors to try to do justice to all the great changes to which we have referred, and to integrate them into a living framework. Perhaps psychiatry has now already progressed beyond the stage in which textbooks by only a few authors can be justified. However, the sciences do not advance solely by the accumulation of ever more facts; it is also necessary to arrive at statements of an increasing generality, to provide modes of interpretation, i.e. hypotheses, of an increasingly extensive explanatory and predictive power. A major task to be met in a textbook is the placing of facts and observations in perspective and to see them in relation to one another. The author who takes it upon himself to review a much wider field than that in which his own best work has been done, or that with which he is most familiar, will surely disclose to the informed reader errors of fact and emphasis and the biases which his own restricted experience necessarily involves. But what he should be able to give to the reader in compensation for these deficiencies is a picture of the entire scene as it is visualized by a single observer placed at an elevation over one part of the whole map. While the multi-author textbook will remain indispensable for many purposes, above all for detailed study of any part of the whole terrain, the general reader needs in addition the synoptic study in which parts are brought into relation with the whole.

Extract 2 (from start of chapter 1): pages 1–6

Chapter 1

Introduction

The Foundations of Psychiatry

This book is based on the conviction of the authors that the foundations of psychiatry have to be laid *on the ground of the natural sciences*. In it the attempt is made to apply the methods and the resources of a scientific approach to the problems of clinical psychiatry. It is obvious that in the present stage of development of our specialty such a plan can only be carried out in a partial way, and that much of our clinical knowledge today belongs more to medical art than to science. Nevertheless we have kept to this aim because we believe that it is only from an organic connexion between the natural sciences, biology, medicine and psychiatry, and from the arduous but reliable scientific methods of investigation and discussion that lasting advances can be made.

It seems necessary to make such a statement at the outset, because now, as so frequently during the hundred years of its existence, psychiatry is in danger of losing its connexion with the body of medicine. These recurrent crises can be traced to two main causes: to the influence of psychiatric practice on the knowledge and the attitude of psychiatrists, and to the peculiar position of psychiatry between medicine and neurology on the one side and philosophy and psychology on the other.

The primary concern of *the practising psychiatrist*, within the walls of an institution or at large, is to treat and to help his patients. He must try to do this even where knowledge is inadequate and the results of past research provide him with no or an insecure foundation. He will then turn to any tool to hand. When, for instance, the discoveries in cerebral anatomy and pathology of the eighties and nineties of the last century proved of little help in the understanding of mental patients, and when at the same time Kraepelin's nosological classification was of little use when it came to their treatment, it was natural for psychiatrists to be allured by the psychopathological theories of Freud, which promised an explanation of the whole before the details had been elaborated. Similar manifestations of impatience, attempted short cuts and deviations from the slow advance of science, have happened before and happen now. Whenever a new branch of scientific activity appears with some relevance to human behaviour, or whenever a new philosophical movement meets a popular response, psychiatrists seize upon it and try out its implications in their own field. This also is a way of breaking new ground, which must, however, if it is to bear fruit, be worked over by the methods of science.

This instability in the attitude of psychiatrists is made all the easier by the subjectivity and the *lack of precision of psychological data*. Mental events can only be described in words which are themselves often open to varied interpretations. Many terms used in psychiatry are taken from everyday language and are not clearly defined. Special terms, on the other hand, have been taken over from psychiatry into ordinary speech, so that their meaning has been watered down and become ambiguous. Much of the psychiatric literature of today owes its existence to the possibility of playing with words and concepts; and the scientific worker in psychiatry must constantly bear in mind the risks of vagueness and verbosity.

The harm that these tendencies have done and may yet do to the psychiatrist and to his reputation is not to be minimized; strict attention to scientific standards in clinical work is needed now more than ever. It is well to remember that as recently as 150 years ago the treatment of mental aberrations, excepting the care of the dangerous psychotic, was regarded as the province of the philosopher and the theologian. In this country before the First World War, whoever took up psychiatry was considered a failure, a man unable to make his way in medicine or surgery. Psychotherapy was regarded as identical with charlatanry, and the institutional psychiatrist as only fitted to act as society's custodian of its degenerate or dangerous members.

That this has now changed, and that the psychiatrist is recognized as a physician of standing as well as a counsellor in social problems of general interest, is due to the cautious industry

of responsible research workers and to the impartial collection of careful clinical observations. With this in mind the following critical review of present-day tendencies in psychiatry should be read.

Although practising psychiatrists of all schools share much in common, there is rather too much dissension for the present state of psychiatry to be regarded as entirely healthy. Wide differences about fundamental issues exist between what is thought and taught in different centres. These differences inspire attitudes of dogmatism, and there is not the open-mindedness there should be. Rapid advances are being made, but are judged or even ignored on the basis of preconceptions. The solid acquisitions of knowledge from the past, where they conflict with current modes of thought, are not being reformulated where necessary, but are being neglected and even forgotten. Psychiatry is not only being split into a number of schools, but is also, which is more regrettable, being *divorced from the parent science of medicine*. There are indeed psychiatrists who, so far from regretting the split, clear-sightedly do what they can to widen it. Growth in every field, in the number of practising psychiatrists, in the amount of time given to psychiatric teaching of undergraduate and postgraduate students, in the claims made by psychiatrists to be heard in their own and in related fields, in public esteem and support, has led to a corresponding decrease of self-criticism. The normal progress of scientific advance, by which facts are first accumulated and confirmed, and then have fitted to them a theory whose critical implications are subjected to test, has been interrupted by a flight into the air. Theoretical exposition follows theoretical exposition in ever-growing complexity, and the need constantly to check theory by seeking at every point for new facts is forgotten.

Some of the correctives to these tendencies suggested in the previous edition of this textbook may have reflected a too narrow conception of the scope of psychiatry as a discipline. It is desirable that psychiatrists should attempt to define the social and environmental conditions under which the individual thrives or breaks down. It is also to the good that they should round off their knowledge and understanding of disease by studying patients who manage to survive in the community at large, as also those suffering from mild and subclinical forms of psychiatric disorder. Estimates of incidence and prevalence of the main forms of psychiatric disorder have to be made if the mental health services are to be well-planned, and observations on the social setting of health and disease might ultimately enable the psychiatrist to make some contribution towards preventive programmes. The psychiatrist cannot, therefore, ignore the social sciences and must play his part in defining those 'environmental, domestic, occupational, economic, habitual and nutritional factors without which the intimate (or specific) causal factors cannot find their opportunity' (Ryle, 1948). But he must retain a sense of proportion and, in practical work as in research, direct his main energies to those tasks in the field where needs are most pressing, development most promising, and where his special skills and experience are most likely to make him effective. His special gifts and insights are derived from intimate familiarity with the

phenomena of mental disorder and, while he will not wish to define these too rigidly, they should, if he is wise, provide for him the focus of interest. For if he should disperse his activities over too wide a field, he will inevitably fall a victim to superficiality and error.

It therefore seems to us far-fetched to claim that sociology or cultural anthropology occupy as basic a position in relation to psychiatry as do the mental and biological sciences. Within their respective fields the quantitative relation between the known and the unknown is entirely different. Our knowledge of medicine and physiology is detailed, relatively precise and capable of clear definition. Our knowledge of sociology is scanty, imprecise and not easily capable of confirmation or refutation. Our knowledge of medicine gives us information about individuals; sociological knowledge gives us information only about groups, from which deductions about the individual are notoriously subject to error. Medicine teaches us much about the causes of ill-health, the mode of operation of those causes, and the reasons why they show their effects in particular symptoms. Sociology is largely a study of normal people, and has made so far little contribution to our knowledge of the causes of illness. There is no culture or society on record in which the major psychoses have been found to be absent. Nor are any free from neurotic disorders, which are generally regarded as having a much closer association with social and environmental factors.

The argument in favour of an intimate relationship between psychiatry and medicine might still be regarded as trifling if, in fact, *applicability of neurological and medical concepts in psychiatry* were every restricted. This is far from being the case. The organic psychoses, the psychiatric sequelae of cerebral trauma, the epilepsies, encephalitis, alcoholism and the intoxications, occupy a considerable part of the territory which it is the duty of psychiatry to cover. Furthermore, this field is constantly being enlarged. Entirely unsuspected relations, some of them of great theoretical importance, between physical and mental changes are constantly being discovered. The addition of cerebral surgery to the psychiatrist's weapons has opened a new path to the solution of problems as well as to treatment. Recent work with the electroencephalograph provides an instance of the transformation of ideas which may result from the application of neurophysiological concepts and methods to psychiatric problems. The organic connexion between psychiatry on the one side and medicine and physiology on the other is itself a region of rapid further growth.

Another *objection to the divorce of psychiatry from medicine* is that when it is so deprived of its natural foundations it can be swayed by every wind that blows. New doctrines arise and are propagated, and find in many minds a ready application. There is no longer a standard of reference by which they may be criticized. The newer schools which would elevate psychiatry to a loftier position make little or no use of experiment, clinical observation, painstaking follow-up study, even statistical analysis and argument. The psychiatrist is content if he can interpret his findings in terms which happen to be fashionable. Diagnosis becomes

unpopular, and is allowed only secondary importance; what is exalted in its place is interpretation and understanding.

The emphasis on the contribution of sociology and anthropology derives from the view that, since man is a social animal in intimate association with his cultural environment, the level of organization which should be studied to provide an appropriate framework for psychiatry is that at which individual human beings are integrated with communities. There would be something to be said for this view if psychiatry could be justifiably described as the 'science of behaviour' (Masserman). It would then have to aim at giving a complete account, not only of the clinical disorders that provide the daily work of most psychiatrists, but also of human motivation and behaviour as manifested in social and political life, in art and religion. But this would involve a grandiose and *unwarranted expansion of the scope of psychiatry*. A complete account of human behaviour in this sense would demand contributions not only from psychiatry, sociology, psychology and anthropology, but also from economics, history, literature and all the sciences—in fact, from all branches of human knowledge.

Much contemporary work in the field of psychopathology reflects a shift of interest from the characteristics of groups, such as 'schizophrenics', to actual persons, the differences between them, and their relations with one another. An *excessive preoccupation with individuals* is heuristically sterile. Physics would not have advanced very far if every natural phenomenon had been regarded as unique rather than as a member of a class of similar phenomena. Progress depends on recognizing similarities in phenomena which may, superficially, differ very greatly, for from these similarities we may deduce general causes.

We may take as an analogy the logic employed in a *statistically planned experiment*, such as is conducted by the experimental biologist. In interpreting his findings he will attempt first of all to separate out the effects of general causes—the difference between two breeds, or the presence or absence of a soil fertilizer. These are the 'main effects'. If there are more than one of them, he will also be able to measure the effect of 'interactions' between them. Finally he will come down to individual variation, which will be classifiable, statistically, as 'error'. If he were to start at the opposite end, and try to cover as much as possible of the total differences observed by individual variation, he would never discover anything else at all.

So it is in psychiatry. Individual schizophrenics differ from one another almost as much as normal individuals, but they do have similarities, and aspects in which they differ from normals as a class. Their risks of suicide, of tuberculosis and of early death are increased; their chances of successful social adjustment are diminished; their ways of thinking and of feeling show consistent and characteristic differences from those of normal people. Within the schizophrenic group further *classification is useful*; there are sex differences, age differences, differences in clinical type of illness, which are significant for prognosis and treatment. If these classifications are never made, the information they provide naturally never accrues, and it is gratuitous to assert that they are 'little guide'. Furthermore, if we are going to allow ourselves

only one method of treatment, e.g. psychotherapy, and apply it in every case, no information of a general kind can possibly be relevant to treatment; but such an approach takes us back to the days of universal purging and bleeding.

If we forgo the *making of a diagnosis*, we also forgo all application of the extensive knowledge which has been accumulated in the past. This would be sheer folly; we cannot wilfully ignore what is known, and if we wish to do so we are under the psychological necessity of proving (or believing) that the knowledge is false knowledge, or that it is irrelevant. If we refrain from diagnosis we shall be left in the individual case without the help of general concepts. The wise physician never neglects the individual peculiarities of his patient; but he will first see how far he can be fitted into general patterns, and he will not mistake a quality which is characteristic of the group, such as thought disorder or auditory hallucination, as either without significance or as something to be interpreted by the life-history of that one patient alone.

All-embracing explanations and concepts such as those often advanced by psychoanalysts are of dubious value in the light of recent advances in the philosophy of science. It has been argued by Popper (1963) that the greater the explanatory power of a hypothesis, the less is its scientific usefulness. No testable predictions can be based on theories which explain everything and are not capable of refutation. The most useful hypothesis is the one which has the greatest *a priori* improbability; and no sooner is it formulated than one should make the most strenuous efforts to refute it. Our confidence in it will increase with every well-designed effort at refutation which it survives. As has been emphasized by Perley and Guze (1962), an important function of a diagnosis is that of a prediction which can be confirmed or refuted. What are predicted are such significant features as the range of causes which may be shown to be responsible for the condition when further investigated, its course and outcome, etc.

Diagnosis is not a matter of merely naming and labelling. Ideally it implies judgment of causation; and even if this is possible in only the most tentative way, it always includes a plan of action, e.g. of treatment. In medicine we have to deal with causes of all kinds, and not only those that are both necessary and sufficient. In searching for a cause of some phenomenon, we are really *searching for a quantitative relationship*. If A is the necessary and sufficient cause of B, then there is a one-to-one relationship between A and B. If A is a necessary but not sufficient cause, then there is no B without A, but A may be combined with x or y instead of with B. If the variety of these xs and ys is very great, the causal relationship, though it still exists, is thereby weakened. If A is neither necessary nor sufficient, then there are As without Bs and Bs without As, and the strength of the causation will depend on the proportionate relationship between AB to A on the one hand and B on the other. From this one may deduce the reason why the remoter antecedents of an illness are of little consequence. We can, if we like, lose our way in a causal network; but we need not do so if we take into account the quantitative aspect of causation. It is at this point that so much psychiatric

thought loses cogency and direction. Quantitatively important causes are tangled in a knot of others whose quantitative relation with the effect we are interested in is slight or entirely unknown. It is, for instance, maintained by some psychoanalysts that during the birth-pangs of the mother the infant suffers 'anxiety', and that this is the cause of anxiety symptoms in later life. This doctrine appears to be meaningless, for an experience which is shared by all human beings cannot be the cause of a difference between some human beings and others.

In analysing the causes of a given state we are led to postulate factors which hold in this case and do not hold in others. We deal, in fact, in terms of *differences*. In considering the causes of an abnormal mental state it is relevant to inquire whether there is a family history of mental illness, for if that is the case the patient is differentiated from others. Recent studies have also adduced evidence that psychiatric patients differ from normal subjects in respect of the circumstances of their early childhood and upbringing. In a wide range of disorders, including psychoneuroses (Barry and Lindemann, 1960), depressive states (Brown, 1961), delinquency (Glueck and Glueck, 1950), the affective disorders of the aged (Kay, Beamish and Roth, 1964b) and psychiatric patients in general (Gregory, 1958), the frequency with which one or other parent had been lost in childhood has proved greater than in the general population. The specificity of the psychological effects of such deprivations is considered to be greater by some authors (Bowlby, 1960) than others (Lewis, 1954). The important thing is that a start has been made in putting to some sort of objective test hypotheses about the early origins of psychiatric disorder which owe something to psychoanalytic thinking. But that this constitutes no more than a beginning is illustrated by Wootton's review of twenty-one systematic studies of delinquents (Wootton, 1959); the evidence about the aetiological role of such factors as poverty, lower social status, poor employment record, broken home, mother's employment outside the home and low educational attainment proved to be uncertain and imprecise. There is as yet only an insecure foundation of hard fact about the early origins of psychiatric disorder. Few psychiatrists would doubt that deprivation and vicissitude in childhood contribute something to emotional instability and neurotic breakdown. But the precise extent and significance of this contribution remains uncertain.

The need in psychiatry for a scientific approach is overwhelming; by neglecting it we introduce every year a greater degree of confusion. Its requirements are simple. Where a particular quality is attributed to a sample of the population, e.g. those suffering from mental illness, it must be shown, unless it is self-evident, that the same quality is found in other samples to a lesser degree or in lower frequency. *Control studies*, gathered either by the investigator's own hand, or by other means, must be available. The formulation of hypotheses in a precise form should follow and be based on the facts. The major logical consequences of the hypotheses should then be drawn, and those selected for further investigation which might produce critical results, i.e. ones which could prove fatal to the hypothesis.

Reading 11.2**EXERCISE 6**

Introduction to: American Psychiatric Association (1994). 'Introduction' in *Diagnostic and Statistical Manual* (4th edn) Washington, DC: APA, pages xv–xvi

Introduction

This is the fourth edition of the American Psychiatric Association's *Diagnostic and Statistical Manual of Mental Disorders*, or DSM-IV. The utility and credibility of DSM-IV require that it focus on its clinical, research, and educational purposes and be supported by an extensive empirical foundation. Our highest priority has been to provide a helpful guide to clinical practice. We hoped to make DSM-IV practical and useful for clinicians by striving for brevity of criteria sets, clarity of language, and explicit statements of the constructs embodied in the diagnostic criteria. An additional goal was to facilitate research and improve communication among clinicians and researchers. We were also mindful of the use of DSM-IV for improving the collection of clinical information and as an educational tool for teaching psychopathology.

An official nomenclature must be applicable in a wide diversity of contexts. DSM-IV is used by clinicians and researchers of many different orientations (e.g., biological, psychodynamic, cognitive, behavioral, interpersonal, family systems). It is used by psychiatrists, other physicians, psychologists, social workers, nurses, occupational and rehabilitation therapists, counselors, and other health and mental health professionals. DSM-IV must be usable across settings—inpatient, outpatient, partial hospital, consultation-liaison, clinic, private practice, and primary care, and with community populations. It is also a necessary tool for collecting and communicating accurate public health statistics. Fortunately, all these many uses are compatible with one another.

DSM-IV was the product of 13 Work Groups (see Appendix J), each of which had primary responsibility for a section of the manual. This organization was designed to increase participation by experts in each of the respective fields. We took a number of precautions to ensure that the Work Group recommendations would reflect the breadth of available evidence and opinion and not just the views of the specific members. After extensive consultations with experts and clinicians in each field, we selected Work Group members who represented a wide range of perspectives and experiences. Work Group members were instructed that they were to participate as consensus scholars and not as advocates of previously held views. Furthermore, we established a formal evidence-based process for the Work Groups to follow.

The Work Groups reported to the Task Force on DSM-IV (see p. ix), which consisted of 27 members, many of whom also chaired a Work Group. Each of the 13 Work Groups was composed of 5 (or more) members whose reviews were critiqued by between 50 and 100 advisers, who were also chosen to represent diverse

clinical and research expertise, disciplines, backgrounds, and settings. The involvement of many international experts ensured that DSM-IV had available the widest pool of information and would be applicable across cultures. Conferences and workshops were held to provide conceptual and methodological guidance for the DSM-IV effort. These included a number of consultations between the developers of DSM-IV and the developers of ICD-10 conducted for the purpose of increasing compatibility between the two systems. Also held were methods conferences that focused on cultural factors in the diagnosis of mental disorder, on geriatric diagnosis, and on psychiatric diagnosis in primary care settings.

To maintain open and extensive lines of communication, the Task Force on DSM-IV established a liaison with many other components within the American Psychiatric Association and with more than 60 organizations and associations interested in the development of DSM-IV (e.g., American Health Information Management Association, American Nurses Association, American Occupational Therapy Association, American Psychoanalytic Association, American Psychological Association, American Psychological Society, Coalition for the Family, Group for the Advancement of Psychiatry, National Association of Social Workers, National Center for Health Statistics, World Health Organization). We attempted to air issues and empirical evidence early in the process in order to identify potential problems and differences in interpretation. Exchanges of information were also made possible through the distribution of a semiannual newsletter (the *DSM-IV Update*), the publication of a regular column on DSM-IV in *Hospital and Community Psychiatry*, frequent presentations at national and international conferences, and numerous journal articles.

Two years before the publication of DSM-IV, the Task Force published and widely distributed the *DSM-IV Options Book*. This volume presented a comprehensive summary of the alternative proposals that were being considered for inclusion in DSM-IV in order to solicit opinion and additional data for our deliberations. We received extensive correspondence from interested individuals who shared with us additional data and recommendations on the potential impact of the possible changes in DSM-IV on their clinical practice, teaching, research, and administrative work. This breadth of discussion helped us to anticipate problems and to attempt to find the best solution among the various options. One year before the publication of DSM-IV a near-final draft of the proposed criteria sets was distributed to allow for one last critique.

In arriving at final DSM-IV decisions, the Work Groups and the Task Force reviewed all of the extensive empirical evidence and correspondence that had been gathered. It is our belief that the major innovation of DSM-IV lies not in any of its specific content changes but rather in the systematic and explicit process by which it was constructed and documented. More than any other nomenclature of mental disorders, DSM-IV is grounded in empirical evidence.

Reading 11.3**EXERCISE 7**

Extract from: The opening paragraphs of Chapter 1, Signs and Symptoms of mental disorder, in Gelder, M.G., Gath, G., and Mayou, R.A.M. (1983). *The Oxford Textbook of Psychiatry*, (1st edn). Oxford: Oxford University Press, p 1.

Psychiatry can only be practised if the psychiatrist develops two distinct capacities. One is the capacity to collect clinical data objectively and accurately by history taking and examination of mental state, and to organize the data in a systematic and balanced way. The other is the capacity for intuitive understanding of each patient as an individual. When the psychiatrist exercises the first capacity, he draws on his clinical skills and knowledge of clinical phenomena; when he exercises the second capacity, he draws on his general understanding of human nature to gain insights into the feelings and behaviour of each individual patient.

Both capacities can be developed by accumulating experience of talking to patients, and by learning from the guidance and example of more experienced psychiatrists. From a textbook, however, it is inevitable that the reader can learn more about clinical

skills than about intuitive understanding. In this book the first four chapters are concerned with various aspects of clinical skills. This greater coverage of clinical skills in no way implies that intuitive understanding is regarded as unimportant but simply that it cannot be learnt from reading a textbook.

The psychiatrist can only acquire skill in examining patients if he has a sound knowledge of how each symptom and sign is defined. Without such knowledge, he is liable to misclassify phenomena and make inaccurate diagnoses. For this reason, questions of definition are considered in this first chapter, before the examination of patients is described in the next.

Once the psychiatrist has elicited a patient's symptoms and signs, he needs to decide how far these phenomena resemble or differ from those of other psychiatric patients. In other words, he must determine whether the clinical features form a syndrome, which is a group of symptoms and signs that identifies patients with common features. The purpose of identifying a syndrome is to be able to plan treatment and predict the likely outcome by reference to accumulated knowledge about the causes, treatment, and outcome of the same syndrome in other patients. The principles involved are discussed in Chapter 4, which is concerned with classification, and also in the chapters dealing with the different syndromes.

Reading 11.4

EXERCISE 9

Extract from: The opening sections of Sigmund Freud's *Project for a Scientific Psychology*, (Standard Edition), Vol. I. London: Hogarth Press, pages 294–297.

Key to abbreviations in the project

Q = Quantity (in general, or of the order of magnitude in the external world)—See p. 362

$Q\eta$ = Quantity (of the intercellular order of magnitude)—See p. 306

ϕ = system of permeable neurones

ψ = system of impermeable neurones

ω = system of perceptual neurones

W = perception (Wahrnehmung)

V = idea (Vorstellung)

M = motor image

Introduction

The intention is to furnish a psychology that shall be a natural science: that is, to represent psychical processes as quantitatively determinate states of specifiable material particles, thus making those processes perspicuous and free from contradiction. Two principal ideas are involved: [1] What distinguishes activity from rest is to be regarded as Q ,¹ subject to the general laws of motion. (2) The neurones² are to be taken as the material particles.

N and $Q\eta$ —Similar experiments are now frequent.³

[1] (a) First Principal Theorem

The Quantitative Conception

This is derived directly from pathological clinical observation especially where excessively intense ideas were concerned—in hysteria and obsessions, in which, as we shall see, the quantitative characteristic emerges more plainly than in the normal.⁴ Processes such as stimulus, substitution, conversion and discharge, which had to be described there [in connection with those disorders], directly suggested the conception of neuronal excitation as quantity in a state of flow. It seemed legitimate to

¹ [In a footnote to his contribution to *Studies on Hysteria* (1895d), *Standard Ed.*, 2, 195 n., Breuer remarks that 'the conception of the energy of the central nervous system as being a quantity distributed over the brain in a changing and fluctuating manner is an old one'. He goes on to quote from the early nineteenth century French physician, Georges Cabanis (1824, 3, 153). A discussion of Q will be found in Appendix C, p. 392 below.]

² [The term 'neurone', as a description of the ultimate unit of the nervous system, had been introduced by W. Waldeyer in 1891. Freud's own histological researches had led him towards the same finding. See especially Freud (1884f) and a note on this in *Standard Ed.*, 3, 230 n.]

³ [Cf., for instance, Exner (1894), with a similar title and a similar programme, very differently carried out.]

⁴ ['Excessively intense ideas' are discussed in Section 1 of Part II, p. 347 below.]

attempt to generalize what was recognized there. Starting from this consideration, it was possible to lay down a basic principle of neuronal activity in relation to Q, which promised to be highly enlightening, since it appeared to comprise the entire function. This is the principle of neuronal inertia: that neurones tend to divest themselves of Q. On this basis the structure and development as well as the functions [of neurones] are to be understood.⁵

In the first place, the principle of *inertia* explains the structural dichotomy [of neurones] into motor and sensory as a contrivance for neutralizing the reception of $Q\eta$ by giving it off. Reflex movement is now intelligible as an established form of this giving-off: the principle provides the motive for reflex movement. If we go further back from here, we can in the first instance link the nervous system,⁶ as inheritor of the general irritability of protoplasm, with the irritable external surface [of an organism], which is interrupted by considerable stretches of non-irritable surface. A primary nervous system makes use of this $Q\eta$ which it has thus acquired, by giving it off through a connecting path to the muscular mechanisms, and in that way keeps itself free from stimulus. This discharge represents the primary function of the nervous system. Here is room for the development of a secondary function. For among the paths of discharge those are preferred and retained which involve a cessation of the stimulus: *flight from the stimulus*. Here in general there is a proportion between the Q of excitation and the effort necessary for the flight from the stimulus, so that the principle of *inertia* is not upset by this.

The principle of inertia is, however, broken through from the first owing to another circumstance. With an [increasing] complexity of the interior [of the organism], the nervous system receives stimuli from the somatic element itself—endogenous stimuli—which have equally to be discharged. These have their origin in the cells of the body and give rise to the major needs: hunger, respiration, sexuality.⁷ From these the organism cannot withdraw as it does from external stimuli; it cannot employ their Q for flight from the stimulus. They only cease subject to particular conditions, which must be realized in the external world. (Cf., for instance, the need for nourishment.) In order to accomplish such an action (which deserves to be named 'specific'⁸), an effort is

⁵ [In the extended form described below (p. 297), this is what was later known as the 'principle of constancy' and attributed by Freud to Fechner. This is by no means Freud's first mention of it. A discussion of its significance and of its many occurrences throughout Freud's writings will be found in an Editor's Appendix to the first paper on the neuro-psychoses of defence (1894a), *Standard Ed.*, 3, 65. It has been suggested that the concept may be equated with that of homeostasis.]

⁶ [Here and elsewhere this stands for 'Nsy' in the MS. It seems on the whole probable that Freud was using this as an abbreviation for the ordinary 'Nervensystem' and not for 'Neuronensystem' (as expanded in Anf., passim). The former is in fact written out in full in the MS. on pp. 314 and 324 below.]

⁷ [These 'endogenous stimuli' are thus the precursors of the 'instincts'. Cf. the Editor's Note to 'Instincts and their Vicissitudes', *Standard Ed.*, 14, 114 ff. See also below, p. 316.]

⁸ [The 'specific' action reappears, under other names, in (for instance) 'Repression' (1915d) *ibid.*, 14, 147 and in *Civilization and its discontents* (1930a), *ibid.*, 21, 67. But it had been mentioned earlier than this in Part III of the first paper on anxiety neurosis (1895b), *ibid.*, 3, 108 (where it was termed 'the specific or adequate action') and earlier still in Draft E, p.192 above ('the specific reaction').]

required which is independent of endogenous $Q\eta'$ and in general greater, since the individual is being subjected to conditions which may be described as *the exigencies of life*.⁹ In consequence, the nervous system is obliged to abandon its original trend to inertia (that is, to bringing the level [of $Q\eta'$] to zero). It must put up with [maintaining] a store of $Q\eta'$ sufficient to meet the demand for a

specific action. Nevertheless, the manner in which it does this shows that the same trend persists, modified into an endeavour at least to keep the $Q\eta'$ as low as possible and to guard against any increase of it—that is, to keep it constant.¹⁰ All the functions of the nervous system can be comprised either under the aspect of the primary function or of the secondary one imposed by the exigencies of life.

⁹ [This phrase, too, occurs regularly in other works, e.g. in *The Interpretation of Dreams* (1990a), *ibid.*, 5, 565, though Freud later preferred the Greek word 'Ananke'. Cf. *Civilization and its Discontents* (1930a), *ibid.*, 21, 139.]

¹⁰ [See footnote 1, p. 296 above.]

Reading 11.5

EXERCISE 10

2 extracts from: Freud, S. (1977). 'Dora' case study. In *Case Histories I*. The Pelican Freud Library, Vol. 8. London: Penguin.

Extract 1: page 41

In face of the incompleteness of my analytic results, I had no choice but to follow the example of those discoverers whose good fortune it is to bring to the light of day after their long burial the priceless though mutilated relics of antiquity. I have restored what is missing, taking the best models known to me from other analyses; but, like a conscientious archaeologist, I have not omitted to mention in each case where the authentic parts end and my constructions begin.

There is another kind of incompleteness which I myself have intentionally introduced. I have as a rule not reproduced the process of interpretation to which the patient's associations and communications had to be subjected, but only the results of that process. Apart from the dreams, therefore, the technique of the analytic work has been revealed in only a very few places. My object in this case history was to demonstrate the intimate structure of a neurotic disorder and the determination of its symptoms; and it would have led to nothing but hopeless confusion if I had tried to complete the other task at the same time. Before the technical rules, most of which have been arrived at empirically, could be properly laid down, it would be necessary to collect material from the histories of a large number of treatments. Nevertheless, the degree of shortening produced by the omission of the technique is not to be exaggerated in this particular case.

Extract 2: pages 45–49

I begin the treatment, indeed, by asking the patient to give me the whole story of his life and illness, but even so the information I receive is never enough to let me see my way about the case. This first account may be compared to an unnavigable river whose stream is at one moment choked by masses of rock and at another divided and lost among shallows and sandbanks. I cannot help wondering how it is that the authorities can produce such smooth and precise histories in cases of hysteria. As a matter of fact the patients are incapable of giving such reports about themselves. They can, indeed, give the physician plenty of coherent information about this or that period of their lives; but it is sure to be followed by another period as to which their communications run dry, leaving gaps unfilled, and riddles unanswered; and then again will come yet another period which will remain totally obscure and unilluminated by even a single piece of serviceable information. The connections—even the ostensible ones—are for the most part incoherent, and the sequence of different events is uncertain. Even during the course of their story patients will repeatedly correct a particular or a date, and then perhaps, after wavering for some time, return to their first version. The patients'

1. [Not Art and Science serve, alone;
Patience must in the work be shown.
Goethe, *Faust*, Part I (Scene 6).
(Bayard Taylor's translation.)]

inability to give an ordered history of their life in so far as it coincides with the history of their illness is not merely characteristic of the neurosis. It also possesses great theoretical significance. For this inability has the following grounds. In the first place, patients consciously and intentionally keep back part of what they ought to tell—things that are perfectly well known to them—because they have not got over their feelings of timidity and shame (or discretion, where what they say concerns other people); this is the share taken by *conscious* disingenuousness. In the second place, part of the anamnestic knowledge, which the patients have at their disposal at other times, disappears while they are actually telling their story, but without their making any deliberate reservations: the share taken by *unconscious* disingenuousness. In the third place, there are invariably true amnesias—gaps in the memory into which not only old recollections but even quite recent ones have fallen—and paramnesias, formed secondarily so as to fill in those gaps. When the events themselves have been kept in mind, the purpose underlying the amnesias can be fulfilled just as surely by destroying a connection, and a connection is most surely broken by altering the chronological order of events. The latter always proves to be the most vulnerable element in the store of memory and the one which is most easily subject to repression. Again, we meet with many recollections that are in what might be described as the first stage of repression, and these we find surrounded with doubts. At a later period the doubts would be replaced by a loss or a falsification of memory.

That this state of affairs should exist in regard to the memories relating to the history of the illness is a *necessary correlate of the symptoms and one which is theoretically requisite*. In the further course of the treatment the patient supplies the facts which, though he had known them all along, had been kept back by him or had not occurred to his mind. The paramnesias prove untenable, and the gaps in his memory are filled in. It is only towards the end of the treatment that we have before us an intelligible, consistent, and unbroken case history. Whereas the practical aim of the treatment is to remove all possible symptoms and to replace them by conscious thoughts, we may regard it as a second and theoretical aim to repair all the damages to the patient's memory. These two aims are coincident. When one is reached, so is the other; and the same path leads to them both.

It follows from the nature of the facts which form the material of psychoanalysis that we are obliged to pay as much attention in our case histories to the purely human and social circumstances of our patients as to the somatic data and the symptoms of the disorder.

The family circle of the eighteen-year-old girl who is the subject of this paper included, besides herself, her two parents and a brother who was one and a half years her senior. Her father was the dominating figure in this circle, owing to his intelligence and

his character as much as to the circumstances of his life. It was those circumstances which provided the framework for the history of the patient's childhood and illness. At the time at which I began the girl's treatment her father was in his late forties, a man of rather unusual activity and talents, a large manufacturer in very comfortable circumstances. His daughter was most tenderly attached to him, and for that reason her critical powers, which developed early, took all the more offence at many of his actions and peculiarities.

Her affection for him was still further increased by the many severe illnesses which he had been through since her sixth year. At that time he had fallen ill with tuberculosis and the family had consequently moved to a small town in a good climate, situated in one of our southern provinces. There his lung trouble rapidly improved; but, on account of the precautions which were still considered necessary, both parents and children continued for the next ten years or so to reside chiefly in this spot, which I shall call B—. When her father's health was good, he used at times to be away, on visits to his factories. During the hottest part of the summer the family used to move to a health-resort in the hills.

When the girl was about ten years old, her father had to go through a course of treatment in a darkened room on account of a detached retina. As a result of this misfortune his vision was permanently impaired. His gravest illness occurred some two years later. It took the form of a confusional attack, followed by symptoms of paralysis and slight mental disturbances. A friend of his (who plays a part in the story with which we shall be concerned later on [see p. 60, *n.* 2]) persuaded him, while his condition had scarcely improved, to travel to Vienna with his physician and come to me for advice. I hesitated for some time as to whether I ought not to regard the case as one of taboparalysis, but I finally decided upon a diagnosis of a diffuse vascular affection; and since the patient admitted having had a specific infection before his marriage, I prescribed an energetic course of anti-luetic treatment, as a result of which all the remaining disturbances passed off. It is no doubt owing to this fortunate intervention of mine that four years later he brought his daughter, who had meanwhile grown unmistakably neurotic, and introduced her to me, and that after another two years he handed her over to me for psychotherapeutic treatment.

Reading 11.6

EXERCISE 12

Extract from: Freud, S. (1977). 'Dora' case study. In *Case Histories I*. The Pelican Freud Library, Vol. 8. London: Penguin, pp 60–63.

In order to particularize Dora's case it is not enough merely to draw attention to the reversal of affect; there has also been a *displacement* of sensation. Instead of the genital sensation which would certainly have been felt by a healthy girl in such circumstances,¹ Dora was overcome by the unpleasurable feeling which is proper to the tract of mucous membrane at the entrance to the alimentary canal—that is by disgust. The stimulation of her lips by the kiss was no doubt of importance in localizing the feeling at that particular place; but I think I can also recognize another factor in operation.²

The disgust which Dora felt on that occasion did not become a permanent symptom, and even at the time of the treatment it was only, as it were, potentially present. She was a poor eater and confessed to some disinclination for food. On the other hand, the scene had left another consequence behind it in the shape of a sensory hallucination which occurred from time to time and even made its appearance while she was telling me her story. She declared that she could still feel upon the upper part of her body the pressure of Herr K.'s embrace. In accordance with certain rules of symptom-formation which I have come to know, and at the same time taking into account certain other of the patient's peculiarities, which were otherwise inexplicable,—such as her unwillingness to walk past any man whom she saw engaged in eager or affectionate conversation with a lady,—I have formed in my own mind the following reconstruction of the scene. I believe that during the man's passionate embrace she felt not merely his kiss upon her lips but also the pressure of his erect member against her body. This perception was revolting to her; it was dismissed from her memory, repressed, and replaced by the innocent sensation of pressure upon her thorax, which in turn derived an excessive intensity from its repressed source. Once more, therefore, we find a displacement from the lower part of the body to the upper.³ On the other hand, the compulsive piece of behaviour which I have mentioned was formed as though it

¹ Our appreciation of these circumstances will be facilitated when more light has been thrown upon them. [Cf. pp. 121–2.]

² The causes of Dora's disgust at the kiss were certainly not adventitious, for in that case she could not have failed to remember and mention them. I happen to know Herr K., for he was the same person who had visited me with the patient's father [p. 48f.], and he was still quite young and of prepossessing appearance.

³ The occurrence of displacements of this kind has not been assumed for the purpose of this single explanation; the assumption has proved indispensable for the explanation of a large class of symptoms. [Cf. below, p. 120, *n.*] Since treating Dora I have come across another instance of an embrace (this time without a kiss) causing a fright. It was a case of a young woman who had previously been devotedly fond of the man she was engaged to, but had suddenly begun to feel a coldness towards him, accompanied by severe depression, and on that account came to me for treatment. There was no difficulty in tracing the fright back to an erection on the man's part, which she had perceived but had dismissed from her consciousness.

were derived from the undistorted recollection of the scene: she did not like walking past any man who she thought was in a state of sexual excitement, because she wanted to avoid seeing for a second time the somatic sign which accompanies it.

It is worth remarking that we have here three symptoms—the disgust, the sensation of pressure on the upper part of the body, and the avoidance of men engaged in affectionate conversation—all of them derived from a single experience, and that it is only by taking into account the interrelation of these three phenomena that we can understand the way in which the formation of the symptoms came about. The disgust is the symptom of repression in the erotogenic oral zone,⁴ which, as we shall hear [p. 85], had been over-indulged in Dora's infancy by the habit of sensual sucking. The pressure of the erect member probably led to an analogous change in the corresponding female organ, the clitoris; and the excitation of this second erotogenic zone was referred by a process of displacement to the simultaneous pressure against the thorax and became fixed there. Her avoidance of men who might possibly be in a state of sexual excitement follows the mechanism of a phobia, its purpose being to safeguard her against any revival of the repressed perception.

In order to show that such a supplement to the story was possible, I questioned the patient very cautiously as to whether she knew anything of the physical signs of excitement in a man's body. Her answer, as touching the present, was 'Yes', but, as touching the time of the episode, 'I think not'. From the very beginning I took the greatest pains with this patient not to introduce her to any fresh facts in the region of sexual knowledge; and I did this, not from any conscientious motives, but because I was anxious to subject my assumptions to a rigorous test in this case. Accordingly, I did not call a thing by its name until her allusions to it had become so unambiguous that there seemed very slight risk in translating them into direct speech. Her answer was always prompt and frank: she knew about it already. But the question of *where* her knowledge came from was a riddle which her memories were unable to solve. She had forgotten the source of all her information on this subject.⁵

If I may suppose that the scene of the kiss took place in this way, I can arrive at the following derivation for the feelings of disgust.⁶ Such feelings seem originally to be a reaction to the smell (and afterwards also to the sight) of excrement. But the genitals can act as a reminder of the excretory functions; and this applies especially to the male member, for that organ performs the function of micturition as well as the sexual function. Indeed, the function of micturition is the earlier known of

⁴ [See below, p. 86.]

⁵ See the second dream [p. 139.—Cf. also pp. 68 *n.*, 97f. and 162 *n.*].

⁶ Here, as in all similar cases, the reader must be prepared to be met not by one but by several causes—by *overdetermination*. [Freud had mentioned this characteristic of hysterical symptoms in Section III of his chapter on the psychotherapy of hysteria in Breuer and Freud's *Studies on Hysteria*, 1895. It was also discussed by Breuer (with an acknowledgment to Freud) in Section III of his theoretical contribution to the same work. Cf. *P.F.L.*, 3, 289–90 and 373–6.]

the two, and the *only* one known during the pre-sexual period. Thus it happens that disgust becomes one of the means of affective expression in the sphere of sexual life. The Early Christian Father's '*inter urinas et faeces nascimur*' clings to sexual life and cannot be detached from it in spite of every effort at idealization. I should like, however, expressly to emphasize my

opinion that the problem is not solved by the mere pointing out of this path of association. The fact that this association can be called up does not show that it actually *will* be called up. And indeed in normal circumstances it will not be. A knowledge of the paths does not render less necessary a knowledge of the forces which travel along them.

Reading 11.7**EXERCISE 14**

5 extracts from: Freud, S. (1977). 'Dora' case study. In *Case Histories I*. The Pelican Freud Library, Vol. 8. London: Penguin.

Extract 1: page 57

(The extract begins with Dora's father speaking.)

'Dora, who inherits my obstinacy, cannot be moved from her hatred of the K.s. She had her last attack after a conversation in which she had again pressed me to break with them. Please try and bring her to reason.'

Her father's words did not always quite tally with this pronouncement; for on other occasions he tried to put the chief blame for Dora's impossible behaviour on her mother—whose peculiarities made the house unbearable for every one. But I had resolved from the first to suspend my judgement of the true state of affairs till I had heard the other side as well.

The experience with Herr K.—his making love to her and the insult to her honour which was involved—seems to provide in Dora's case the psychological trauma which Breuer and I declared long ago¹ to be the indispensable prerequisite for the production of a hysterical disorder. But this new case also presents all the difficulties which have since led me to go beyond that theory,² besides an additional difficulty of a special kind.

Extract 2: page 69

The poor woman had thrown a most unwelcome light on a part of Dora's own behaviour. What the governess had from time to time been to Dora, Dora had been to Herr K.'s children. She had been a mother to them, she had taught them, she had gone for walks with them, she had offered them a complete substitute for the slight interest which their own mother showed in them. Herr K. and his wife had often talked of getting a divorce; but it never took place, because Herr K., who was an affectionate father, would not give up either of the two children. A common interest in the children had from the first been a bond between Herr K. and Dora. Her preoccupation with his children was evidently a cloak for something else that Dora was anxious to hide from herself and from other people.

The same inference was to be drawn both from her behaviour towards the children, regarded in the light of the governess's

¹ [In their 'Preliminary Communication' (see Freud, 1893a).]

² I have gone beyond that theory, but I have not abandoned it; that is to say, I do not today consider the theory incorrect, but incomplete. All that I have abandoned is the emphasis laid upon the so-called 'hypnotic state', which was supposed to be occasioned in the patient by the trauma, and to be the foundation for all the psychologically abnormal events which followed. If, where a piece of joint work is in question, it is legitimate to make a subsequent division of property, I should like to take this opportunity of stating that the hypothesis of 'hypnotic states'—which many reviewers were inclined to regard as the central portion of our work—sprang entirely from the initiative of Breuer. I regard the use of such a term as superfluous and misleading, because it interrupts the continuity of the problem as to the nature of the psychological process accompanying the formation of hysterical symptoms.

behaviour towards herself, and from her silent acquiescence in her father's relations with Frau K.—namely, that she had all these years been in love with Herr K. When I informed her of this conclusion she did not assent to it. It is true that she at once told me that other people besides (one of her cousins, for instance—a girl who had stopped with them for some time at B—) had said to her: 'Why you're simply wild about that man!' But she herself could not be got to recollect any feelings of the kind. Later on, when the quantity of material that had come up had made it difficult for her to persist in her denial, she admitted that she might have been in love with Herr K. at B—, but declared that, since the scene by the lake it had all been over.³

Extract 3: page 93

In this way I gained an insight into a conflict which was well calculated to unhinge the girl's mind. On the one hand she was filled with regret at having rejected the man's proposal, and with longing for his company and all the little signs of his affection; while on the other hand these feelings of tenderness and longing were combated by powerful forces, amongst which her pride was one of the most obvious. Thus she had succeeded in persuading herself that she had done with Herr K.—that was the advantage she derived from this typical process of repression; and yet she was obliged to summon up her infantile affection for her father and to exaggerate it, in order to protect herself against the feelings of love which were constantly pressing forward into consciousness. The further fact that she was almost incessantly a prey to the most embittered jealousy seemed to admit of still another determination.⁴

My expectations were by no means disappointed when this explanation of mine was met by Dora with a most emphatic negative. The 'No' uttered by a patient after a repressed thought has been presented to his conscious perception for the first time does no more than register the existence of a repression and its severity; it acts, as it were, as a gauge of the repression's strength. If this 'No', instead of being regarded as the expression of an impartial judgement (of which, indeed, the patient is incapable), is ignored, and if work is continued, the first evidence soon begins to appear that in such a case 'No' signifies the desired 'Yes'. Dora admitted that she found it impossible to be as angry with Herr K. as he had deserved. She told me that one day she had met Herr K. in the street while she was walking with a cousin of hers who did not know him. The other girl had exclaimed all at once: 'Why, Dora, what's wrong with you? You've gone as white as a sheet!' She herself had felt nothing of this change of colour; but I explained to her that the expression of emotion and the play of features obey the unconscious rather than the conscious . . .

³ Compare the second dream.

⁴ We shall come upon this [in a moment].

Extract 4: page 94

Another time Dora came to me in the worst of tempers after having been uniformly cheerful for several days. She could give no explanation of this. She felt so contrary today, she said; it was her uncle's birthday, and she could not bring herself to congratulate him, she did not know why. My powers of interpretation were at a low ebb that day; I let her go on talking, and she suddenly recollected that it was Herr K.'s birthday too—a fact which I did not fail to use against her. And it was then no longer hard to explain why the handsome presents she had had on her own birthday a few days before had given her no pleasure. One gift was missing, and that was Herr K.'s, the gift which had plainly once been the most prized of all.

Nevertheless Dora persisted in denying my contention for some time longer, until, towards the end of the analysis, the conclusive proof of its correctness came to light

Extract 5: pages 149–150

'The fact is, I am beginning to suspect that you took the affair with Herr K. much more seriously than you have been willing to admit so far. Had not the K.s often talked of getting a divorce?'

'Yes, certainly. At first she did not want to, on account of the children. And now she wants to, but he no longer does.'

'May you not have thought that he wanted to get divorced from his wife so as to marry you? And that now he no longer wants to because he has no one to replace her? It is true that two years ago you were very young. But you told me yourself that your mother was engaged at seventeen and then waited two years for her husband. A daughter usually takes her mother's love-story as her model. So you too wanted to wait for him, and you took it that he was only waiting till you were grown up enough to be his wife.⁵ I imagine that this was a perfectly serious plan for the future in your eyes. You have not even got the right to assert that it was out of the question for Herr K. to have had any such intention; you have told me enough about him that points directly

towards his having such an intention.⁶ Nor does his behaviour at L—contradict this view. After all, you did not let him finish his speech and do not know what he meant to say to you. Incidentally, the scheme would by no means have been so impracticable. Your father's relations with Frau K.—and it was probably only for this reason that you lent them your support for so long—made it certain that her consent to a divorce could be obtained; and you can get anything you like out of your father. Indeed, if your temptation at L—had had a different upshot, this would have been the only possible solution for all the parties concerned. And I think that is why you regretted the actual event so deeply and emended it in the phantasy which made its appearance in the shape of the appendicitis. So it must have been a bitter piece of disillusionment for you when the effect of your charges against Herr K. was not that he renewed his proposals but that he replied instead with denials and slanders. You will agree that nothing makes you so angry as having it thought that you merely fancied the scene by the lake. [Cf. p. 79.] I know now—and this is what you do not want to be reminded of—that you *did* fancy that Herr K.'s proposals were serious, and that he would not leave off until you had married him.'

Dora had listened to me without any of her usual contradictions. She seemed to be moved; she said good-bye to me very warmly, with the heartiest wishes for the New Year, and—came no more. Her father, who called on me two or three times afterwards, assured me that she would come back again, and said it was easy to see that she was eager for the treatment to continue. But it must be confessed that Dora's father was never entirely straightforward. He had given his support to the treatment so long as he could hope that I should 'talk' Dora out of her belief that there was something more than a friendship between him and Frau K. His interest faded when he observed that it was not my intention to bring about that result. I knew Dora would not come back again. Her breaking off so unexpectedly, just when my hopes of a successful termination of the treatment were at their highest, and her thus bringing those hopes to nothing—this was an unmistakable act of vengeance on her part.

⁵ The theme of waiting till the goal is reached occurs in the content of the first situation in the dream. I recognize in this phantasy of waiting for a fiancée a portion of the third component of that situation. I have already alluded [p. 140, n. 1] to the existence of this third component.

⁶ In particular there was a speech which he had made in presenting Dora with a letter-case for Christmas in the last year in which they lived together at B—.

Reading 11.8

EXERCISE 15

Extract from: Ricoeur, P. (1970). Energetics and hermeneutics in *The Interpretation of Dreams*. In *Freud and Philosophy* (trans. D. Savage). London: Yale University Press, Book II, chapter 2, pp 87–93

The difficult Chapter 7 of *The Interpretation of Dreams*¹ (*Traumdeutung*) is unquestionably the heir to the “Project” of 1895; left unpublished by Freud himself, the “Project” found an outlet in *The Interpretation of Dreams*.² However, at least two changes have supervened. The first is so great that no one could overlook it: the psychical apparatus of *The Interpretation of Dreams* functions without any anatomical reference; it is a *psychical* apparatus. From this point on, dreams impose a theme that may be called Herbartian: there are dream “thoughts”; a dream is the accomplishment or fulfillment (*Erfüllung*) of a desire or wish (*Wunsch*); that is to say, it is something “psychical” or “ideational.” Hence *The Interpretation of Dreams* no longer speaks of cathected neurons but of cathected ideas. This first change entails another one which, though less visible, is perhaps of greater importance for an epistemological reflection on “models”: the schema of the psychical apparatus oscillates between a *real* representation, as was the machine of the “Project,” and a *figurative* representation, as will be the later schemata of the topography. We shall try to understand this ambiguity and, if possible, justify it to a certain extent.

These two changes disclose a more radical transformation affecting the relationship between the topographic-economic *explanation* on the one hand and *interpretation* on the other. In the “Project” that relation was left unclear: the interpretation of symptoms, which arose from observations of transference in neurotic patients, guided the construction of the system without itself being thematized within the system. As a result the systematic explanation seemed to be independent of the concrete work of the analyst and of the patient’s own work on his neurosis. Such is not the case in *The Interpretation of Dreams*: here the systematic explanation is placed at the end of a process of work whose own rules have been elaborated; the express aim of the explanation is to present a schematic transcription of what goes on in the dream-work that is accessible only in and through the work of interpretation. The explanation, therefore, is explicitly subordinated

¹ I shall frequently cite passages from *The Interpretation of Dreams* so as to propose to French readers a more exact translation of the original text. The title itself, which directly concerns the theory of hermeneutics, should be translated literally: *Deutung* does not mean science but interpretation. The French translation to which I refer—with the reservation that I may correct it—is that of I. Meyerson, *La Science des rêves* (Paris, P. U. F., 1950), translated from the 7th German edition; I also indicate in parentheses the pagination of the edition of the *Club français du livre*, 1963. [Translator’s note: By the author’s directive, nearly all references to the French editions are omitted and all quotations are taken from James Strachey’s translation in the *Standard Edition*, Vols. 4 and 5.]

² The evolution of the theory may be followed in the letters to Fliess written after the “Project”; see particularly Letters 39 and 52, which are still close to the “Project.” It is important for our later discussion to note that the theory of the hallucinatory character of dreams—a theory already introduced in the “Project” (*The Origins of Psychoanalysis*, p. 401)—preceded the more general thesis that dreams are a wish-fulfillment: Letters 28, 45, and 62; cf. Anzieu, *L’Auto-analyse*, pp. 82–129.

to interpretation; it is not by accident that this book is called *Die Traumdeutung*, *The Interpretation of Dreams*.

The dream-work and the work of exegesis

The thesis that dreams have meaning is first of all a polemical thesis which Freud defends on two fronts. On the one hand, it is opposed to the notion that dreams are a chance play of representations, a waste product of mental life, and that the sole problem concerning them is their lack of meaning. From this first point of view, to say that dreams have meaning is to assert that they are an intelligible, and even intellectual, operation of man; to understand them is to experience their intelligibility. On the other hand, the thesis is opposed to any premature organic explanation of dreams; the thesis signifies that one can always substitute for the dream account another account, with a semantics and a syntax, and that these two accounts are comparable to one another as two texts. It sometimes happens that Freud compares, more or less appropriately, this relation of text to text to that of translating from one language to another; we will return to the exactitude of the analogy later. For the present let us take the analogy as unambiguous affirmation that interpretation moves from a less intelligible to a more intelligible meaning. The same may be said of the analogy of the picture puzzle or rebus, which is another example of the relation of obscure text to clear text.³

The comparison of meaning to a text enables one to eliminate what remains equivocal in the notion of symptom; a symptom, to be sure, is already an *effect-sign* and presents the mixed structure we wish to delimit in this study; but this mixed structure is more clearly revealed by dreams than by symptoms.⁴ Belonging

³ “The aim which I have set before myself is to show that dreams are capable of being interpreted [*einer Deutung fähig sind*] . . . My presumption that dreams can be interpreted at once puts me in opposition to the ruling theory of dreams and in fact to every theory of dreams with the exception of Schemer’s; for ‘interpreting’ a dream implies assigning a ‘meaning’ [*Sinn*] to it—that is, replacing [*ersetzen*] it by something which fits [*sich . . . einfügt*] into the chain of our mental acts as a link having a validity and importance equal to the rest” (*GW*, 2/3, 100; *SE*, 4, 96). Further on, at the beginning of Chapter 3, Freud compares the situation of the analyst who has surmounted the first difficulties of dream interpretation to that of an explorer who comes upon an open view after passing through a narrow defile: “We find ourselves in the full daylight of a sudden discovery” (*wir stehen in der Klarheit einer plötzlichen Erkenntnis*) (*GW*, 2/3, 127; *SE*, 4, 122; this phrase is omitted in the French translation, p. 94 [77]). Thus dreams are seen to be “psychical phenomena of complete validity [*vollgültiger*]—fulfillments of wishes [*Wunsch*]; they can be inserted [*einzureihen*] into the chain of intelligible [*uns verständlichen*] waking mental acts; they are constructed by a highly complicated activity of the mind [*geistige*]” (*ibid.*). On the comparison of interpretation to a translation from one language into another, or to the solution of a rebus, see *GW*, 2/3, 283–84; *SE*, 4, 277–78.

⁴ Chronologically the idea of symptoms, which is common to Breuer and Freud, is certainly first; methodologically, however, the reversal of priority is essential: “Though my own line of approach to the subject of dreams was determined by my previous work on the psychology of the neuroses, I had not intended to make use of the latter as a basis of reference in the present work. Nevertheless I am constantly being driven to do so, instead of proceeding, as I should have wished, in the contrary direction and using dreams as a means of approach to the psychology of the neuroses” (*GW*, 2/3, 593; *SE*, 5, 588). The structural identity of neurotic symptoms and dreams as both being “formations of compromise” will be established only at the end of the topography (“*Traumbildung und Symptombildung*,” *GW*, 2/3, 611–13; *SE*, 5, 605–08). But the interpretation of symptoms as *symbols*, in the *Studies on Hysteria*, is the main link; cf. below, p. 97, n. 15.

as they do to discourse, dreams reveal that symptoms have a meaning; thus dreams enable one to coordinate the normal and the pathological within what might be called a general semiology.

But is it possible to maintain interpretation on this unambiguous level where relations would be those of meaning to meaning? Interpretation cannot be developed without calling into play concepts of an entirely different order, energy concepts. It is impossible to achieve the first task of interpretation—viz. to discover the thoughts, ideas, or wishes that are “fulfilled” in a disguised way—without considering the “mechanisms” that constitute the dream-work and bring about the “transposition” or “distortion” (*Entstellung*) of the dream-thoughts into the manifest content. This study of the dream-work, according to one of the methodological texts of *The Interpretation of Dreams*, constitutes the second task.⁵ The distinction between the two tasks has only a pedagogical value, however: the discovery of the unconscious dream-thoughts shows that they are the same as the thoughts of waking life; all the strangeness of dreams is centered, rather, upon the dream-work. Transposition or distortion, in which the dream-work roughly consists, splits dreams off from the rest of psychological life, whereas the revealing of the dream-thoughts relates dreams to waking life.

Moreover, the first task, which in the course of the book is not clearly distinguished from the second, cannot be accomplished to any great extent without recourse to economic concepts. To find the dream-thoughts is, in fact, to follow out a certain regressive path which, beyond the present impressions and bodily excitations, the memories of waking life or the day's residues, or the actual wish for sleep, discloses the unconscious, that is to say, *the earliest wishes*. It is our childhood that rises to the surface, with its forgotten, checked, repressed impulses, and along with our childhood that of mankind, recapitulated in that of the individual. Dreams provide access to a basic phenomenon that will constantly preoccupy us in this book, the phenomenon of *regression*, of which we shall shortly better understand not only the temporal but the topological and dynamic aspects. In regression, we are led from concepts of meaning to concepts of force by this relation to the abolished, the forbidden, the repressed—this close connection between the archaic and the oneiric; for the realm of dream-fantasy is a realm of desire. If dreams are drawn toward discourse because of their

narrative aspect, their relation to wishes or desires throws them back on the side of energy, conatus, appetite, will to power, libido, or whatever one wishes to call it. Thus dreams, inasmuch as they are the expression of wishes, lie at the intersection of meaning and force.

Interpretation (*Deutung*), which has not yet become identified with the work of deciphering correlative to the dream-work, and which has been concerned more with psychical content than with mechanism, nevertheless has begun to receive its proper structure, and this structure is a mixed one. On the one hand, in terms of meaning, interpretation is a movement from the manifest to the latent. To interpret is to displace the origin of meaning to another region. The topography, at least in its static and properly topographical form, will be the pictorial representation of this movement of interpretation from the apparent meaning toward another locality of meaning. But even at this first level it is impossible to look upon *Deutung* as a simple relation between ciphered and deciphered discourse; it is not enough to say that the unconscious is another discourse, an unintelligible discourse. In its transposition or distortion (*Verstellung*) of the manifest content into the latent content, interpretation uncovers another distortion, that of desires into images; Freud investigates this distortion in Chapter 4. To use an expression from the “Papers on Metapsychology,” a dream is already a “vicissitude of instinct.”

But it is impossible to thematize this *Verstellung* more precisely without proceeding to the second task of accounting for the mechanisms of the dream-work (*Traumarbeit*), which is the subject of Chapter 6. More clearly than the first, this second task requires combining two universes of discourse, the discourse of meaning and the discourse of force. To say that a dream is the *fulfillment* of a *repressed* wish is to put together two notions which belong to different orders: fulfillment (*Erfüllung*), which belongs to the discourse of meaning (as attested by Husserl's use of the term), and repression (*Verdrängung*), which belongs to the discourse of force. The notion of *Verstellung*, which combines the two universes of discourse, expresses the fusion of these two concepts, for a disguise is a type of manifestation and, at the same time, a distortion that alters that manifestation: it is the *violence done to the meaning*. Thus the relation of the hidden to the shown in the notion of disguise requires a deformation, or disfiguration, which can only be stated as a compromise of forces. The concept of “censorship,” correlative to the concept of distortion, belongs to this same mixed discourse: distortion is the effect, censorship the cause. But what does censorship mean? The word is well chosen: on the one hand, censorship manifests itself at the level of a text on which it imposes blanks, word substitutions, softened expressions, allusions, tricks of arrangement—with suspect or subversive items being displaced and hidden in harmless, out-of-the-way spots; on the other hand, censorship is the expression of a power, more precisely of a political power, which works against the opposition by striking at its right of expression. In the idea of censorship the two systems of language are very closely interwoven: censorship alters a text only when it represses a force, and it

⁵ See the important methodological text that terminates Chapter 6: “Two separate functions may be distinguished in mental activity during the construction of a dream: the production [*Herstellung*] of the dream-thoughts, and their transformation into the content of the dream.” The dream-thoughts do not have a special nature. On the other hand, the dream-work is peculiar to dreams; this activity “is completely different [from waking thought] qualitatively and for that reason not immediately comparable with it. It does not think, calculate or judge in any way at all; it restricts itself to giving things a new form” (*GW*, 2/3, 510–11; *SE*, 5, 506–07). This theme is taken up again in Chapter 7: *GW*, 2/3, 597; *SE*, 5, 592. The translation of the word *Entstellung*, by which Freud globally designates the dreamwork, and which covers displacement, condensation, and other procedures, is difficult: it contains two ideas, that of a violent change of place, and that of a deformation, disfiguring, or disguise which makes something unrecognizable. Both the traditional French translation, *transposition*, and the English translation, “distortion” (*distorsion* is also a good French expression), retain only one of the intentions of the original term. That is why I write: “transposition” or “distortion.”

represses a forbidden force only by disturbing the expression of that force.

What we have just said of the notions of disguise, distortion, and censorship, which together characterize the “transposition” effected by the dream-work, is still more evident if we consider the diverse mechanisms separately; none of them can be enunciated without recourse to that same mixed language.

On the one hand, the dream-work is the inverse of the analyst’s work of deciphering and is homogeneous therefore with the mental operations of interpretation which trace it back. Thus the two main processes studied in Chapter 6 of *The Interpretation of Dreams*, “condensation” (*Verdichtungsarbeit*) and “displacement” (*Verschiebungsarbeit*), are meaningful operations comparable to rhetorical procedures. Freud himself compares condensation to an abbreviated, laconic turn of phrase, to a lacunary expression; it is at the same time a formation of composite expressions each of

which belongs to several trains of thought. He compares displacement to a shift away from the central point, or again to an inversion of emphasis or value, whereby the various ideas of the latent content transfer their “psychical intensities” to the manifest content. These two processes attest, on the plane of meaning, to an “overdetermination” which calls for interpretation. Each of the elements of the dream-content is said to be overdetermined when it is “represented in the dream-thoughts many times over.”⁶ Overdetermination also governs, though in different ways, condensation and displacement. This is clear in the case of condensation, where the problem is to set out or make explicit a multiplicity of meanings through free association. But displacement, which concerns psychical intensities rather than the number of ideas, also requires overdetermination: to create new values, to displace interests, to “disregard” the point of intensity, displacement must follow the path of overdetermination.

Reading 11.9

EXERCISE 16

Extract from: Grunbaum, A. (1988). *Precis of The Foundations of Psychoanalysis*. In *Mind, Psychoanalysis and Science*, (ed. P. Clark and C. Wright). Oxford: Basil Blackwell, pp 10–13.

The Clinical Method of Psychoanalytic Investigation: Pathfinder or Pitfall? (Part I)

Is Freud's theory empirically testable?

Freud himself always claimed that the treatment setting is the arena *par excellence* for psychoanalytic research, experimental tests being essentially superfluous, if not inappropriate. By contrast, Eysenck (1963) asserted that Freudian theory is experimentally testable but denied that well-designed clinical tests are feasible. Others, such as Glymour (1980), have contended that there are after all viable strategies for supporting or refuting psychoanalytic hypotheses on the couch. Glymour allowed for both clinical and experimental tests. Thus, all of these writers do agree that at least some parts of the Freudian corpus are in fact testable by empirical findings of *some sort*.

Just this shared assumption of actual testability has repeatedly been denied by Popper, who has even rejected the *logical* possibility of testing psychoanalysis empirically. As recently as when he replied to his critics in 1974 (pp. 984–5), Popper reiterated his earlier claim that Freud's theory, as well as Adler's, is 'simply non-testable, irrefutable. There was no conceivable human behaviour which would contradict them' (Popper 1962: 37). Indeed, Popper emphasizes that 'psychoanalysis was immune [to falsification by any logically possible empirical findings] to start with, and remained so' (1974: 986). It is then a mere corollary of this thesis of non-testability that clinical data in particular cannot serve as a basis for genuine empirical tests.

(At the time that the writing of *Foundations* was completed, Popper's (1983) book, *Realism and the Aim of Science*, was not yet available in print I could not take this later discussion of psychoanalysis into account, but I have the opportunity to do this as part of my Response to Critics in *Behavioral and Brain Sciences*, 9 (1986), 266–84.)

When Popper claims that his falsifiability criterion excludes psychoanalysis from the pantheon of the *bona fide* empirical sciences, his principal concern is not with Freudian theory as such, important though psychoanalysis is. In Popper's earlier works (1962: 156–7, 255–8) we find psychoanalysis playing a role not so much as itself the prime target of his charge of non-falsifiability, but rather as a centrepiece for his critique of inductivism as a method of scientific theory-validation or a criterion of demarcation between science and non-science. For, as I read Popper (1962: 33–8), inductivism does accept the claims of abundant empirical

confirmation made by adherents of Freud's theory, as well as by proponents of Adler's revisionist version of psychoanalysis and by contemporary Marxists.

Indeed, Popper (1962: 35–7) seems to have become convinced of the unfalsifiability of psychoanalysis partly *because* he thought it was always confirmed inductively, come what may. Thus, by 1919 he had persuaded himself both that inductivism does not have the methodological resources to challenge the scientific status of psychoanalysis *and* that Freud's theory—as well as Adlerian revisionism and Marxism—was in fact empirically irrefutable. On this basis, Popper argued that the inductivist method of confirmation and its criterion of demarcation are unacceptably permissive.

Hence, the real philosophical villain of Popper's story was inductivism rather than psychoanalysis or Marxism as such, although he deplored the latter in their own right. Having found to his dismay in 1919 that inductivism still held sway as a criterion of demarcation, Popper used psychoanalysis—Freudian and Adlerian—as the *pièce de résistance* in his case against it. He therefore concluded: 'Thus there clearly was a need for a different criterion of demarcation' (1962: 256). In short, psychoanalysis has been and (at least as of 1974) remains Popper's prime illustration of the superiority of his falsifiability criterion. But if he were right that Freud's theory is untestable altogether, then it would clearly be pointless to inquire whether this theory has been or can be tested *clinically*. Popper's challenge must accordingly be dealt with from the outset. It turns out to be ironic that Popper should have chosen psychoanalytic theory as a prime illustration of his thesis that inductive confirmations can easily be found for nearly every theory if we look for them: it is precisely Freud's theory that furnishes compelling evidence against this caricature of the inductivist tradition (Grünbaum 1984: 280).

Popper is concerned to contrast the scientific status of modern physics with the non-scientific or pseudo-scientific character of psychoanalysis. He does so by claiming that the former is empirically falsifiable whereas the latter is not. But before he can say that potential negative findings can refute physical theories, Popper must make use of two important qualifications:

- 1 All reports of observations codifying contrary evidence in physics are fallible, if only because they themselves are already theory-laden; hence 'falsifications' are revokable.
- 2 'Disregarding the possibility of immunizing strategems' (Popper 1974, II: 1004), potential falsifying data do exist in physics. But because of this ever-present logical possibility of resorting to immunization, it would, in my view, have been preferable if Popper had used the term 'disconfirmability' rather than 'falsifiability' or 'refutability'. In discussing his views, I will use his locutions however.

It is important to note that, immediately after predicating the falsifiability of Newtonian physics upon 'disregarding' feasible immunizing strategies, Popper declares in the next sentence: 'And this is the heart of the matter, for my criticism of Freud's theory

was that it simply does not have potential falsifiers' (1974, II: 1004). But what is sauce for the goose is, I submit, sauce for the gander. Accordingly, it is to be understood that when I claim that Freud's clinical theory *does* have potential falsifiers, I do so subject to the same two qualifications on which Popper relied in the case of physics.

Chapter 1 sets forth seven sets of grounds to refute Popper's charge that psychoanalytic theory is not falsifiable. This charge, I must emphasize, is logically independent of the sociological objection that Freudians are not responsive to criticism of their hypothesis. After all, a theory may well be invalidated by known evidence, even as its true believers refuse to acknowledge this refutation. That the recalcitrance of Freudians in the face of falsifying evidence, however scandalous, is not at all tantamount to the irrefutability of their theory should be especially evident from some of Popper's other doctrines. As he tells us, theories, on the one hand, and the intellectual conduct of their protagonists, on the other, 'belong to *two entirely different "worlds"*' (1974, I: 144; italics in original). Yet because Popper sometimes discusses them in the same breath, my response to his views on psychoanalysis takes both into account.

Foundations offers the following items in connection with the supposed unfalsifiability of Freudian theory: (1) examples of Freudian causal hypotheses that are demonstrably falsifiable and of predictions derivable from them that qualify as 'risky' by Popper's standards; (2) Popper's failure to furnish an actual proof of empirical irrefutability, with reliance instead on a contrived illustration of psychoanalytic explanation involving two men and a drowning child (1962: 35; 1974, II: 985); (3) Freud's own successive modifications of his evolving clinical theory (Fancher 1973; Sulloway 1979), conceptual changes that were neither immunizing manoeuvres nor capricious, but rather reactions to seemingly adverse findings; (4) Freud's explicit statement about the sort of evidence he would acknowledge as a refuting instance for his hypothesized aetiology of anxiety neurosis, as well as other documentation of sophisticated methodological tactics not vulnerable to Popper's wholesale criticism, even when Freud delayed for nearly a decade public acknowledgement of the 1897 collapse of his seduction theory of hysteria; (5) Popper's total neglect of Freud's 1937 'Constructions in Analysis' paper, which is addressed to the issue of evading clinical falsifications on the principle 'heads I win, tails you lose'.

account of the symbolic relation that accounts for gratification, whilst doing justice to the role of disguise. The key lies in playing down the assimilation of psychoanalytic symbolic meaning to the kind of meaning that we find in natural language, for it is this assumption that provides Sartre's argument with its force. I will set out the proposed reconstruction in four parts.

One: It is crucial to note that symbolisation occurs in a *context of desire*. Sartre assumes that the symbol, S, takes the place of the original object of desire, X, through a judgement (of 'internal analogy'), which *gives reason* for S to be substituted for X. The sequence would then be:

desire for X → (inhibition) → judgement of analogy →
substitution of S for X → desire for S.

This model is appropriate to rational symbolic substitutions, as when, for example, a message is put into code, or where, given a belief that waving a white flag means 'I surrender', it is rational to let my desire to surrender cause me to desire to wave a white flag. Such substitutions occur in *cognitive* contexts, and are effects of practical reasoning.

By extending the account of wish-fulfilment, we get a different story:

desire for X → (inhibition) → wish for X → X/S association →
wish for S.

Here the substitution of S for X occurs at the point where wishes are created, i.e. on the border of the propositional and the prepropositional. So the process in which a dream-thought is formed, for example, does not consist in 'coding' one propositional thought into another, and the relevant symbolic substitution does not require an act of thought.

Two: Replacing the cognitive context by one of desire obliges us to add other elements to motivate the substitution of S for X, in compensation for the rational motive that has been removed. These consist in certain dispositions attaching to desire. A desire held in check will be disposed to do *something*—almost anything—rather than remain idle. Freud's economic theory, and the relation of desire to instinctual demand, mean that a desire's force does not consist in, although it may be reflected by, the strength of a person's preference, understood as his assignment of value to the desire's satisfaction. This creates a gap, and a relation of partial independence, between a desire's force, and its intentionality: desires have force *whether or not* they have appropriate objects. Now it is clearly economically 'preferable' for a desire to have some object rather than none, because the expenditure of energy by the mental apparatus in active pursuit of an object, whatever its suitability, will at least reduce the organism's level of tension, even if no objective change in the conditions stimulating its instinct is thereby brought about. Because there is a motive for desire to find some object rather than none, a motive that need not be negotiated by a preference, desire is plastic or 'object-hungry'.

(Psychoanalytic theory therefore endorses a limited sense in which the point of desire is simply the expending of force,

or exercise of will; but it stops short of the fully 'pessimistic', Schopenhauerian view according to which the explanation for people's having reasons at all lies in a state of blind conatus that is ultimately *antithetical* to the very having of reasons!)

Three: The original mechanism of linkage of S and X does not require conscious recognition of similarity. Freud's emphasis was often on linguistic connections, the arbitrariness of which brings symbol-formation into line with jokes and parapraxes. But even where there is an objective similarity between S and X (e.g. between towers and phalluses), knowledge of which can be presumed in the preconscious, it does not follow that the substitution is a rational act on the model of the conscious creation of a metaphor: recognitions of similarity can enter obliquely and non-rationally into mental processing (as when a detail of appearance causes a stranger in the crowd to be momentarily mistaken for a friend).

Whatever establishes the initial linkage, the relation requires reinforcement, and this is effected by a *sharing of phenomenology* between S and X. An analogy with art shows what is meant. Finding a pictorial or linguistic expression for a mental state involves creating something external to 'match' it, and this relation can not be fully captured or engineered by beliefs about properties shared by mental states and the objects matched up with them. Similarly, through the force of desire's interest in satisfaction, the subjective representation of the object S adopted as a symbol is worked on so as increasingly to assume the phenomenology of the original object of desire, thereby strengthening S's capacity to draw on the motivation rightfully due to the usurped object, such that S and X, initially paired only mechanically, come to be related in such a way that *S provides a path to the phenomenology of X*.

Four: An explicit reply is still owed to Sartre's insistence that there is a sustained relation of *meaning* between X and S to be accounted for. Such a relation might seem to be evidenced by the fact that the substitution of S for X does not occur only in single events of wish-fulfilment, but recurs in, for example, cycles of dreams, thus apparently established in a private 'lexicon'.

The recurrence of a symbol is however insufficient for *rules of meaning*, as opposed to merely expressively cemented constant conjunctions. The absence, or attenuation, in the unconscious of the usual apparatus of monitoring and feedback allows the desire for X to go through the same cycle of processing with S, after the initial substitution, as it would have done with X: this negative condition does the work that would have been performed by a positive representation of their association. Desires do not verify that the 'right' symbol has been used on any given occasion, and interpretations of dreams do not reveal mistakes in the use of symbols. Items can fail in the service of symbolism, only in the sense of being causally inadequate vehicles of gratification. Psychoanalysis may talk of a 'language' of dreams, but this does not stretch to attributing to the dreamer's *Ucs.* a Gricean disposition to communicate with *Cs.* or the analyst; as Freud says, '[a] dream does not want to say anything to anyone. It is not a vehicle for communication.'

In sum, dispositions attaching to desire serve to create symbolism in the place of other, properly meaning-based processes. Symbolic disguise does not require that symbols be interpreted as such by the subject, or that the unconscious form an independent *subject* of understanding; 'unconscious meaning' is not *known* by its subject. There is, nevertheless, a good sense in which some sort of bond of comprehension follows from the forging of shared phenomenology between X and S—just as an individual may be said to 'comprehend' their twitching, of which they are apparently oblivious, as expressing their nervousness. But such attenuated consciousness or comprehension clearly does not create a paradox.

The psychoanalytic model of symbolism is cogent and nonpartitive.

5.9 Symbolism III: Freud and Jung

The general tenor of Freudian interpretation of symbols is brought out sharply through its contrast with that of Jung. Jung requires a much stronger sense in which there is such a thing as *symbolic thinking*. This is quite different from the model of disguise: Jung envisages symbolism as providing a unique means of thinking about certain kinds of objects that can not be grasped in discursive, non-symbolic thought. These are, for Jung, quasi-Platonic

entities, subjectively identified as 'archetypes'. Such a theory, with its conception of the unconscious as a semi-independent cognising subject distinguished by its heterogeneity from ordinary cognition, will locate the origin of irrationality in faults in symbolic thinking (discord between archetypes), or maladjustment in the relations of symbolic to non-symbolic thought (failure in the process of 'individuation'). The presence of symbolism in behaviour and experience does not, in Jungian interpretation, require conation: when symbols figure in contexts of desire they do so derivatively, transplanted from their primary, cognitive context.

The direction of interpretation in Freudian analysis is, by contrast, fundamentally towards experience of particulars rather than conception of universals, the grounds determining symbolic substitution lying as far as possible within the orbit of individual experience. The existence of an innate symbolising function is explicitly rejected by Freud: 'there is no necessity to assume that any peculiar symbolizing activity of the mind is operating in the dream-work'. Put another way, for Freud, the creation of symbols is not itself a goal of the mind's. Given the universal character of the relevant basic levels of experience, Freud acknowledges the consequence; 'in contrast to the multiplicity of the representations in the dream, the interpretations of the symbols are very monotonous, and this displeases everyone who hears of it; but what is there that we can do about it?'