The Development of Case Formulation Approaches

Michael Bruch

There is nothing as practical as a good theory.

Kurt Lewin

Case Formulation and Psychiatric Diagnosis

Beyond diagnosis? The title of this book may sound intriguing to the reader, especially as, at first glance, cognitive–behavioural psychotherapy and psychiatric classification appear to be quite well suited to each other as evidenced by not only research publications but also textbooks on therapeutic techniques mostly presented according to diagnostic categories.

Are there any limitations with the psychiatric model? How can we define a relationship between psychiatric diagnosis and case formulation? Does case formulation require a diagnosis? And what is really necessary to know when understanding a problem and making a treatment plan? To answer these questions, it seems appropriate to produce reasons and developmental conditions for case formulation as a clinical approach.

Traditionally, clinicians dealing with behavioural disorders, especially in the psychiatric setting, were mostly expected to define and organize their clinical work in terms of nosological categories.

The practical end result of this was mostly classification and medication. There was little room for psychotherapy, and novel approaches were usually not encouraged. When, in the 1950s, behaviour therapy (BT) arrived on the scene, there seemed little willingness on part of the psychiatric establishment to change this tradition (Eysenck, 1990).
However, this attitude caused growing irritation and dissatisfaction among behaviour therapists, as hardly any instrumental value could be found in a classification system that aimed mainly at ‘scientific’ order and communication (often with dubious validity and reliability [British Psychological Society [BPS], 2011]), but appears less helpful or even intent in explaining mechanism and directing treatment of respective disorders.

What are the problems with psychiatric diagnosis?

Apart from being merely descriptive, there can be considerable overlap between categories. Despite considerable improvements and refinements over the last two decades (APA/DSM IV-TR, 2000), this is largely true to this day, as Turkat (1990) has pointed out in the case of personality disorders: For example, when clinicians were asked to sort the criteria for all disorders to the matching categories, they were only able to assign 66% of these correctly, indicating lack of validity and diagnostic overlap (Blashfield & Breen, 1989). They also found a tendency to assign presented problems to multiple categories to (six to eight in the case of personality disorders).

More recently, Sturmey (2009) has provided an excellent critique from the methodological point of view about the shortcomings of the psychodiagnostic approach according to the medical model. In the field of BT, the adoption of diagnostic criteria when selecting treatment procedures came about in the 60s, in tandem with the development of standardized techniques evaluated in randomized controlled trials (RCTs). Although this approach is presented in a very persuasive way by many professional bodies, mostly of a medical background, Sturmey points out the many limitations of this approach. Given the problems of validity and reliability of psychiatric diagnosis, it is questionable if homogenous groups for controlled trials can ever be constructed. Psychological processes in development are mostly ignored, and individualization of treatment is not encouraged.

Furthermore, any interaction between diagnosis and treatment is not contemplated in these designs as one treatment method is simply compared with another one, placebo or waiting list control, etc. Such a design does not allow us to determine whether a treatment effective for one diagnosis would also be effective for another diagnosis or whether it was the most effective treatment for a diagnosis. One might speculate that anxiety-based disorders associated with a range of symptoms might be best treated with a method addressing the mechanism of the disorder rather than focusing on individual complaints (symptoms), which are presumably the main focus of a diagnosis.

From a clinical standpoint, which deals with a unique patient, average improvement, as established in RCTs, does not make much sense – the average patient simply does not exist. Besides, significance in trials can be
achieved with large size samples and sensitive measures. And usually, substantial number of individuals improve little or not at all. And even with those who do improve, the effective ingredient remains unclear and might include non-specific effects such as the therapeutic interaction.

Taking these points together, it seems highly questionable whether parametric statistical methods are appropriate in evaluating the efficacy of psychotherapeutic treatments. On the assumption that treatment should be individualized, single-case experimental design methodology might be used instead.

Sturmey (2009) points out further limitations of the diagnostic model: When medication is recommended, individual responses can be varied and often quite problematic in the long term. For example, if perceived effectiveness of treatment is seen to lie outside one’s own resources, dependency on medication may occur and self-efficacy arising out of personal coping responses is not encouraged.

Standardized psychological treatments are equally problematic as the response to them is unpredictable, improvements are often not maintained or some problems remain unchanged.

For all these reasons, creative and experienced clinicians tend to reject the straightjacket of the diagnostic model and prefer an individualized approach.

Finally, reliability and validity issues are a continuous problem with ongoing DSM and ICD (World Health Organization, 2010) revisions. Research trials on these criteria are few and far between and are usually conducted after criteria have already been established. Sturmey also points out that clinicians tend to use their own methods and not the recommended structured assessment procedures that may achieve better reliability.

Unfortunately, over time, these developments did lead to an increasing medicalization in treating behavioural disorders – a far cry from what was originally intended by the pioneers of BT such as Eysenck, who preferred to focus on a learning principle-based analysis of individual cases. This was motivated by complex and difficult cases that were rejected by the established providers, usually psychiatry or dynamic psychotherapy (Eysenck, 1990).

As the latest revision towards DSM-5 clearly demonstrates, scientists and clinical experts still share many disagreements and, despite considerable non-medical input, there does not appear much progress regarding advice and guidance for non-medical treatments.

It is difficult to see how, after many controversial revisions, DSM-5 will ever deliver a classification system acceptable to clinicians and sufferers. The recent response by the BPS (2011) addresses these expected shortcomings:

The Society recommends a revision of the way mental distress is thought about, starting with recognition of the overwhelming evidence that it is on a spectrum with ‘normal’ experience, and that psychosocial factors such as
poverty, unemployment and trauma are the most strongly-evidenced causal factors. Rather than applying preordained diagnostic categories to clinical populations, we believe that any classification system should begin from the bottom up – starting with specific experiences, problems or ‘symptoms’ or ‘complaints’. Statistical analyses of problems from community samples show that they do not map onto past or current categories (Mirowsky, 1990; Mirowsky & Ross, 2003). We would like to see the base unit of measurement as specific problems (e.g. hearing voices, feelings of anxiety, etc.)? These would be more helpful too in terms of epidemiology.

While some clinicians find a name or a diagnostic label helpful, our contention is that this helpfulness results from a knowledge that their problems are recognised (in both senses of the word) understood, validated, explained (and explicable) and have some relief. Clients often, unfortunately, find that diagnosis offers only a spurious promise of such benefits. Since – for example – two people with a diagnosis of ‘schizophrenia’ or ‘personality disorder’ may possess no two symptoms in common, it is difficult to see what communicative benefit is served by using these diagnoses. We believe that a description of a person’s real problems would suffice. Moncrieff and others have shown that diagnostic labels are less useful than a description of a person’s problems for predicting treatment response, so again diagnoses seem positively unhelpful compared to the alternatives. There is ample evidence from psychological therapies that case formulations (whether from a single theoretical perspective or more integrative) are entirely possible to communicate to staff or clients.

Another problem with diagnosis is the lack of interest in understanding the underlying psychological mechanism of a disorder. Obviously, this is quite unacceptable from a behavioural-learning perspective. Grouping problems into categories cannot advance this search for causes as learning biographies are ignored. Thus, psychiatric diagnosis will not help us to explain onset, development and maintenance of a behavioural disorder. Such information, however, must be considered to be crucial for a comprehensive case formulation with explanatory power.

Finally, psychiatric classification is clearly linked with concepts of mental illness and normality. Such labels are frequently experienced as stigmatizing value judgements by clients and have been shown to be counterproductive for learning-oriented therapy (e.g. Szasz, 1961). Despite great efforts towards operationally defined categories in diagnosis, the issue remains contentious, especially for non-medical psychotherapists.

However, it seems surprising that only recently claims were made to the contrary, i.e. the suggestion that medicalization of behavioural problems might provide relief for the sufferer (Markowitz & Swartz, 2007). There appears no substantial clinical evidence for this; however, labelling may be preferred by some individuals as it allows evading personal responsibility in preference for medicated treatment over psychological therapy. I believe this to be a short cut likely to undermine a personal locus of control in the
development of new resources and coping behaviours, thus preventing enhancement of self-efficacy processes (Bandura, 1977).

These comments on the recent development of psychiatric classification clearly demonstrate that not much has been achieved since the publication of the first edition of this classification. There are no signs for a ‘bottom–up’ approach based on information experienced by problem sufferers and conceptualized according to case formulation methodology. Nevertheless, the commentary report of the BPS on the development of DSM-5 must be welcomed as a reminder that new initiatives are needed.

Although mostly inspired by psychologists, early developments of BT usually took place in psychiatric settings. However, right from the start, there was a difference of opinion how this approach should be applied to behavioural disorders. While psychologists preferred an experimental learning–based approach, psychiatrists saw the ‘symptom focus’ of this new method compatible with the medical model and were hopeful of finding symptom-focused techniques. It is also noteworthy that clinical psychologists in those early days were working under psychiatrists and were given limited scope to conduct BT without medical supervision. Originally, this method was expected by psychiatrist to supplement, like psychometry, psychiatrists diagnosis! (Eysenck, 1990).

In view of this background, it is hardly surprising that, apart from the pioneering experimental work carried out mostly by psychologists, efforts to develop treatment methods were targeted on technologies designed to match a diagnosis. These efforts were motivated by a need to satisfy the requirements of psychotherapy outcome studies, rather than addressing the real needs of individual patients. More recently, advances in ‘operationalized’ diagnosis (e.g. DSM-IV-TR, 2000) have facilitated the development of more disorder-focused treatment manuals. These tend to be more sophisticated as most are developed in clinical settings (see Wilson, 1996, 1997 for a discussion). Despite such substantial improvements, there remain strong doubts as to whether or not treatment manuals will ever be able to address fully the complexity of individual problems (Hickling & Blanchard, 1997; Malatesta, 1995a, 1995b). That is not to say that manuals, as a product of technical expertise, may not be useful in clinical training and therapy (e.g. the acquisition of technical skills by trainees) provided adjustments to individual cases can be made (see discussion later in this text).

To conclude, it appears obvious that psychiatric diagnosis has not become a facilitating tool for the development of individual-focused BT. Considering ongoing problems with reliability, validity, prognostic value and co-morbidity, there must be considerable doubts (e.g. BPS, 2011) that any psychiatric classification system does fulfil such criteria in a satisfactory way.

Despite such concerns, psychiatric diagnosis continues to be a powerful and deeply rooted system in mental health services across the world that
casts a long shadow over the provision of psychological psychotherapies. And there is another, slightly more mundane, reason why, especially in the United States, the home of DSM, psychiatric classification is not indispensable: Usually, clinicians cannot expect remuneration by health insurance companies unless an established psychiatric diagnosis is submitted, together with matching techniques (I. D. Turkat, personal communication, 2012)!

Given the diverse natures of case formulation and psychiatric diagnosis, it may seem contradictory that early attempts to experiment with applications of learning theory took place in psychiatric settings. However, this is easily explained, as these settings were usually the location for complex and difficult-to-understand disorders, not suited for psychiatric intervention and even less so for psychodynamic approaches that tend to be highly selective. It was mostly for these reasons that psychologists were encouraged to experiment with alternative approaches (Eysenck, 1990).

A Short History of Case Formulation

Individualized BT using experimental methodology goes back to the very roots of BT itself. Indeed, most pioneers (e.g. Lazarus, 1960; Meyer, 1960; Wolpe, 1960; Yates, 1960) started out by applying learning principles to the assessment and treatment of clinical cases. It is this body of work that can be considered as the foundation of case formulation. However, the current label arrived much later: Turkat (1985) introduced the term ‘case formulation’, which is now fairly consistently applied for this concept and practised in the field of behavioural and cognitive therapies and beyond (e.g. Eells, 2007; Sturmey, 2009). However, as case formulation has become fashionable in recent years, its definition and description has changed in a less precise and focused direction, moving away from the original intention of its pioneers (Eells, 2007; Sturmey, 2009; Tarrier, 2006). For some, it just indicates good clinical practice with greater concern and attention for the individual and the therapeutic relationship. For others, the term itself is regarded as controversial (e.g. Dryden, 1998), for allegedly ‘objectifying’ human beings as cases. Nothing could be further from the nature and intentions of case formulation. Given today’s sensitivity regarding politically correct terms, this may not be the most fitting label although it should be the substance that matters most. In an earlier period, no distinctive label other than ‘individualized’ was used, nor did it appear necessary or fashionable in those days. The remainder of this chapter addresses the origins of this important and unique approach to analyzing psychological problems.
The Contribution of the Maudsley Group

The foundations for clinical–experimental work on the basis of learning principles, both for assessment and treatment, were laid by Hans Eysenck (1990) and his team in the early 50s at the Maudsley Hospital in London. Eysenck’s critical account and now-famous paper on the effects on psychotherapy and spontaneous remission (Eysenck, 1952) had inspired new psychotherapeutic thinking, mainly guided by experimental and learning psychology. This approach to psychotherapy was subsequently labelled ‘Behaviour Therapy’. It was timely that this therapeutic innovation coincided with the establishment of clinical psychology in England, also led by Eysenck: Both developments, the ‘new profession’ compatible with a ‘new approach’, formed the basis for strong synergistic effects for years to come.

In the early days, Eysenck strongly encouraged experimental investigation of single cases on the basis of learning principles. This task was taken up by Monte Shapiro (1955, 1957) (Shapiro & Nelson, 1955) who pioneered a suitable methodology that allowed assessment and conceptualization of psychiatric disorders in their clinical context. Its main assumption was that each patient constitutes a scientific problem of its own and that the skills of the clinical psychologist were used to solve this unique problem by applying general methods of experimental psychology in a special framework of learning theory.

To elaborate, Shapiro proceeded as follows: Patients were interviewed to achieve a precise description of their problem behaviour. Next, it was attempted to quantify these subjective reports with suitable individualized measures. Further, learning models were employed in an attempt to explain the problem under investigation, or new models were formulated on the basis of individual data and learning principles. From here predictions were made, which were subsequently tested in clinical experiments, in order to eliminate false hypotheses. Having conducted such rigorous procedure, Shapiro expected to arrive at a valid model of explanation in learning terms that could be subjected to further verification procedures. It is notable that in this way the clinician operates like a researcher in the clinical field, which necessitates a considerable amount of time and resources.

There can be no doubt that Shapiro’s experimental method has enabled gathering of relevant information to facilitate behavioural treatments based on learning principles. For these reasons, it must be regarded as a pioneering implementation of the scientist–practitioner model (Boulder Conference on Graduate Education in Clinical Psychology, 1949), generally accepted as the most suitable procedure for BT. At the time, it seemed appropriate and necessary to carry out such detailed and meticulous work to further the fledgling BT in clinical settings.
However, in the process, a number of problems and limitations became increasingly obvious with Shapiro’s approach. Shapiro himself was of the opinion that his method should be universally applied in clinical practice; however, such views proved misguided as it soon turned out that his rather academic aspirations proved quite unrealistic for clinical settings. Only a small number of clinicians (i.e. the Maudsley group) working in specialized research settings were sufficiently interested and trained to follow his recommendations and protocol in detail. Clinicians working in routine practice, even given strong interest, did not have the expertise, time, and necessary resources to develop explanatory models guided by learning principles and individually tailored treatment programmes according to Shapiro’s demanding experimental procedure.

Other problems concerned appropriate operationalization and measurement of problem behaviours. It turned out that clinicians, if not specially trained, found it difficult to select or even develop suitable instruments for psychotherapeutic evaluation. Instead, there was a tendency to resort to old-style psychometry, which is more suited to the assessment of personality traits, diagnostic labelling, and statistical comparisons.

Furthermore, therapists were not prepared to subject their interview style or preferred treatment techniques to empirical scrutiny and would continue with procedures that were perceived as effective according to their own experience and convictions (Meyer & Chesser, 1970).

Finally, patients suffering acutely were looking for quick results and did not want to participate in what was seen as guinea pigs in long-winded experimental investigations. However, it is surprising that even academic research–oriented psychologists lost interest in this methodology as they were lured into the world of diagnostic labelling and treatment techniques. The research paradigm of the natural sciences was considered as more relevant and important.

To conclude, despite its creative inventiveness, high scientific standards and potential usefulness, especially for complex problems, Shapiro’s method was never fully established as a clinical tool in the field of psychotherapy. Typical examples of early experimental work were published in two separate volumes (Eysenck, 1960, 1964). To this day, these creative experimental studies provide fascinating reading for the student of cognitive–behavioural psychotherapy, offering extensive insight into the pioneering work of the early behaviour therapists. It is surprising that this line of work was not further pursued by academic researchers who instead got increasingly involved in diagnostically orientated research: The goal was to define matching techniques that could be evaluated in RCTs – thus following an established research paradigm considered to be more scientific and powerful than single-case methodology.
The Contribution of Victor Meyer

Victor Meyer was one of Eysenck’s early students who began practising as a clinical psychologist in 1955. Meyer was predominantly interested in developing workable clinical applications of this new approach to behavioural disorders. He soon began to realize that BT, with growing scientific ambitions, was moving away from single-case work and was not addressing itself to real clinical problems as research focus shifted to analogue group design. Describing this dilemma, he later stated (Meyer, 1975):

The emphasis in the literature is on the experimental approach to various problems in the field, and the majority of textbooks on BT have been written by authors who have little or no experience in the clinical application of the principles of BT. As a result, the current literature deals mainly with various problems and issues for pure and applied research and prepares the reader only to become a research worker in the field. Clinical practice and training, and the problems involved in these aspects of BT, are relatively ignored…thus, would be behaviour therapists find very little help from the literature concerning the nature of their duties and the problems pertaining to their role as clinicians (p. 11).

Instead, Meyer suggested that basic clinical questions ought to be addressed to achieve best clinical practice and a clinical research strategy:

What treatment, by whom, is most effective for a particular individual, with a specific problem, under which set of circumstances, and how does it come about?

Obviously, the intent was to create a ‘scientist–practitioner’ who investigates empirically but adapts to the clinical setting in a pragmatic way at the same time.

However, the mainstream development of BT went into a different direction. As academic psychologists and also psychiatrists became interested in BT, research endeavours became increasingly focused on developing standardized techniques developed through controlled research trials. It became obvious that BT was moving further and further away from its original clinical base. In fact, researchers even started preferring analogue settings, typically investigating students with small animal phobias who were easily accessible in university research departments. This practice culminated in the practice of ‘symptom–technique matching’, the idea being to select standard methods to match diagnostic categories. This greatly facilitated research but was otherwise providing poor clinical outcomes, especially with more complex disorders. For the clinician, if he was at all able to follow such academic aspirations, this process provided little guidance in dealing
with the complexities of real clinical problems and raised more questions than it answered.

Meyer (1975) sums up the problems from the clinician’s point of view:

...most of the writings on behaviour therapy by ‘experts’ are experimentally oriented and advocate a technological approach to treatment by giving ‘scientific respectability’ to the techniques that have been derived from learning principles and intensively investigated. This no doubt has an influence on the practice of behaviour therapy. It is not surprising, therefore, that many practising behaviour therapists apply certain prescribed techniques to certain psychiatric conditions. Also, psychologists and psychiatrists seeking training in behaviour therapy expect to be grilled in techniques and the range of their applicability. Originally, I did subscribe to this approach, but, after gaining wide clinical experience, I was struck by its considerable limitations and its inappropriateness in many cases (Meyer & Chesser, 1970). The problems of technology are twofold. First, not all patients sharing the same complaint respond to the procedural requirements of techniques. For example, in the case of systematic desensitisation, some patients find it difficult to relax, some fail to conjure up clear images of phobic scenes in relevant modalities, some do not report anxiety to clearly imagined phobic stimuli, and a proportion fail to generalise adequately to real-life situations.

Second, and more important, one seldom finds cases with isolated complaints in psychiatric clinics. It is more likely that one is confronted with a number of complaints and problems that may or may not be directly related to the main complaint presented by the patient; or the main complaint may turn out, according to the therapist’s assessment, to be other than what the patient himself believes it to be (p. 16).

Typically, the research-oriented behaviour therapist tends to ignore individual differences and is rather concerned about matching group designs to allow, e.g. comparison and evaluation of treatment techniques.

Meyer, on the other hand, was rooted firmly as a clinician in the psychiatric setting where he attempted to apply the fledgling BT to more severe and chronic problems, e.g. existential problems, personality disorders and obsessive-compulsive disorders. It was also seen as important to complement clinical work with research and training based in the same setting. In other words, research and training ought to be adjusted to clinical requirements and not the other way round. Meyer (1975) expands on this as follows:

Thus, as a therapist, I attempt to guide all my activities in terms of learning principles, no matter whether the goal of treatment is to modify motor, autonomic, or cognitive aspects of disordered behaviour. For myself, behaviour therapy is an ongoing process during which observations are collected, hypotheses put forward to account for them, tests of hypotheses carried out,
adjustments to new observations made, and so on...the advantage of the approach advocated here is that the clinician would be able to formulate treatment programmes for every patient that would be flexible enough to meet the myriad problems and practical obstacles found when treating patients and would not be obliged to put technical straitjackets on his patients. It is not the technological approach but the application of the principles that has enabled behaviour therapy to be extended to virtually every type of psychiatric picture.

In my opinion, learning principles offer a distinct contribution in the attempt to understand aetiology and to design treatment for psychiatric disorders...I attempt to structure my approach explicitly and systematically in terms of learning principles (p. 21).

Further, Meyer (1975) pointed to the crucial role of initial interviewing in this approach. He was critical of standardized or over-comprehensive schemes (e.g. Kanfer & Saslow, 1969), which he thought were too distant or cumbersome. Instead of rigid adherence to one scheme, therapists should be encouraged to develop hypotheses about the nature of the complaint. Such an approach shapes the interview and makes it fluent and will also be helpful in establishing rapport with the patient. Sources for hypotheses can be contextual, knowledge of learning principles, and personal experience, as Meyer (1975) explains:

The importance of this is obvious, since decisions about the goal and choice of treatment depend on the kind of information elicited from the patient...The main sources of information are the patient’s verbal report, nonverbal behaviour during the interview, and response to psychological tests. In addition, the therapist has at his disposal other means of gathering information, such as seeing the relatives of the patient, observing the patient in his own environment, or getting the patient to observe and record his own behaviour (p. 22).

On the basis of such information, a so-called ‘problem formulation’ is established, which attempts to integrate all relevant data into a meaningful picture. This is then discussed with the patient.

The patient is then given the formulation in simple terms, and the objective of treatment is discussed with him. The subject should give his consent concerning the goal of treatment. Close relatives should also be consulted about it, particularly when the target behaviour is social behaviour. The therapist should consider the patient’s wishes within the context of his own formulation, and it should be his duty to attempt to adjust the patient to the environment to which he wishes to be discharged, if possible.

Following this, the patient is given a simple general outline of possible treatment procedures and the rationale underlying them. The therapist emphasises the tentative nature of his formulation, but he also indicates that the patient’s
responses early in treatment may demonstrate any necessary modifications. While motivating the patient to undertake treatment, the therapist at the same time attempts to give him a realistic expectation for outcome. In addition, the patient is told that he is expected to be an active participant in the treatment, and that as soon as possible he will be required to become his own therapist and to exert self-control. Understanding of every step undertaken in therapy is essential and will enable him to cooperate and participate actively (Meyer, 1975: p. 23).

In other words, great emphasis is placed on understanding the patient’s problem, explaining the rationale, seeking cooperation and thus building resolve and motivation for the treatment programme.

Another important aspect of Meyer’s approach was the development of the therapeutic relationship. At the time, this issue was more or less ignored in BT, partly as a (over)reaction to psychodynamic therapies, where relationship issues were seen as being too prominent and thus distractive regarding treatment goals. Meyer and Liddell (1975) explain their rationale as follows:

The relationship between the behaviour therapist and his patient is conceptualised in terms of learning principles from the start of the behavioural analysis. The patient’s life style will give some idea of what type of behaviour the therapist must show to become a potent reinforcer. For the behaviour therapist there should be almost as many types of relationships as he sees patients. This adaptability is more important in cases of interpersonal problems than with isolated phobias. The behaviour therapist aims at something approaching an instructor/trainee relationship; he tries to avoid to hide behind the therapeutic veil. In the ideal situation he hopes that the patient will gradually become his own therapist and their interaction should reflect this (p. 237).

According to Meyer, in most cases, the therapist should structure his behaviour so it serves as an effective social reinforcer. The type of relationship to be developed should precisely meet the needs of the individual patient. This is illustrated as follows:

If, for instance, the basic problem presented is fear of authority, then the therapist will behave in such a way as to make it easier for his patient to learn skills appropriate when dealing with people in authority. On the other hand, the therapist who treats an isolated phobia will attempt to inspire confidence and to make his patients relax in his presence (Meyer & Liddell, 1975: p. 226).

Obviously, there should be limits in accommodating patient’s needs as this may lead to over-involvement (pathological relationship) or unwanted shaping of patient’s behaviour by means of verbal conditioning, etc. Also, for any intensive and supportive relationship to facilitate the therapeutic
procedure, there needs to be a clearly defined exit strategy to prevent unintended long-term dependency.

Meyer’s groundbreaking work eventually led to the establishment of the first Behaviour Therapy Unit at St. Luke’s Woodside Hospital, London, and the first training course of BT based on the case formulation model at the Middlesex Hospital Medical School in London in 1970. Two years later, Vic Meyer became the founding father of the British Association of Behavioural Psychotherapy (BABP, now BABCP).

Further Developments Based on Meyer’s Approach

Further developments on Meyer’s case formulation approach were carried out by some of his students, who, for the first time, attempted to apply case formulation in other settings, e.g. outpatient services and behavioural medicine. In particular, Turkat’s important contributions enabled the model to take root in the United States where the field at the time was dominated by concern with academic research. Luckily, this coincided with a growing interest in clinical issues and more complex problems, like personality disorders. This new emphasis was reflected in several meetings of the American Association for the Advancement of Behaviour Therapy in the early 80s. Critical analyses and recommendations regarding the nature and direction of BT were put forward by a number of distinguished behaviour therapists (e.g. Wilson, 1982).

In the present context, I shall focus on the salient contributions of two outstanding clinicians who provided conceptual frameworks for Vic Meyer’s ideas and clinical practice.

The Contribution of Ira Turkat

Ira Turkat was a clinical psychologist and academic from the United States, who had done his clinical internship with Victor Meyer in the late 70s. He embraced the model, refined it and made it more accessible. In his work, Turkat was also greatly influenced by Henry Adams, Monte Shapiro and Joseph Wolpe, all of whom had also collaborated with Meyer at one time.

As Meyer was a rather reluctant publicist, Turkat’s great achievement was to define and operationalize the ‘Middlesex approach’ in numerous publications, thus putting it ‘on the map’. Most relevant in this respect were his two textbooks entitled ‘Behavioural Case formulation’ (Turkat, 1985) and ‘The Personality Disorders’ (Turkat, 1990). Initially, Turkat proposed the label ‘Behaviour-Analytic Approach’ (Meyer & Turkat, 1979) to emphasize the individual analysis of behaviour, but later he argued against this ‘poor choice
of a label’ as he feared that it implied synthesis of behavioural and psychoanalytic approaches (Turkat, 1986). Eventually, the term ‘Case Formulation’ was finally adopted, making it more consistent with Meyer’s original choice of words, which were ‘behavioural formulation’ or ‘problem formulation’. However, there still appears to be some confusion about the concept as was recently documented in a debate in the ‘Behaviour Therapist’. Adams (1996) has clarified this by pointing out that...

‘case formulation is not a treatment procedure. It is a method for understanding the patient and their problems that allows for the selection and design of treatment procedures based on the knowledge of their case’ (p. 78).

To elaborate, Turkat emphasized the ‘tripartite nature’ of case formulation, consisting of the initial interview, clinical experimentation and modification methodology. Continuous hypothesis generation and testing was proposed as a logical link between these components, and it also allows for corrective feedback when hypotheses cannot be verified. Apart from these rules, the process should be adjusted to the individual case in a flexible manner.

The main function of the initial interview is to achieve a problem formulation, as opposed to a diagnosis, which Turkat mainly advocates as a tool for communication. Turkat also states that a diagnosis has no immediate value in achieving a valid problem formulation. The problem formulation itself comprises three elements:

1. A hypothesis about the relationship among various problems of the individual;
2. Hypotheses about the aetiology of the aforementioned difficulties;

In other words, the formulation assumes the status of a personalized clinical theory to explain the nature and mechanism of the patient’s problem. Although an experimental approach is adopted, Turkat (1990) points out that a clinical assessment can hardly have the rigour of a controlled research experiment. The main focus, in consideration on current limited knowledge on psychopathology, has to be on reasoned hypotheses and clinical data.

In conducting the initial interview, Turkat (e.g. 1990) makes a number of recommendations:

The initial interaction should be closely monitored, as it can provide non-verbal cues regarding, for example, the appearance, mannerisms and motivations of the patient. However, Turkat cautions that hypotheses may be wrong and the temptation to seek consistent data should be resisted.

Next, he recommends constructing a precise problem specification on the basis of information offered by the patient. This may involve a number of problems that are not always presented in order of priority by the patient or possibly not mentioned at all. To guide more detailed specification, Turkat has
proposed the behavioural analysis matrix (Turkat, 1979), which investigates components of behaviour in more detail, including antecedent and consequent conditions. This procedure shall be addressed in the next chapter.

The gathering of relevant data helps to generate hypotheses that in turn open the path for further information. Of great importance is what Turkat labels ‘etiology inquiries’, that are directed at identifying, predisposing, precipitating and maintaining factors. Aetiology inquiries are in essence a combination of assessment of individual learning history and functional analysis. Investigation into predisposing factors aims at identifying vulnerability factors arising from genetic, social, cultural, professional, religious backgrounds, etc. in interaction with learning processes. In contrast, precipitating factors refer to the immediate trigger and maintaining factors to reinforcing conditions of the problem behaviour.

The precise examination of all these variables, continuously guided by hypotheses, is seen as critical for the development of the formulation, which is discussed in great detail with the patient. For further verification of this clinical theory, Turkat proposes ‘clinical experimentation’, a quasi-experimental procedure. This serves two main functions: (1) to validate the formulation by making predictions that are subsequently evaluated and (2) to specify further relationships between independent and dependent variables. The design and measurements should fit individual requirements, i.e. simple and obvious problems may not require extensive testing whereas complex problem formulations may have to be examined in a more rigorous manner. Turkat (1990) notes:

Given the tremendous range of variables that could be generated from the diverse nature of psychopathology presented by personality disorder cases as well as the infinite possibilities involved in formulation of those problems, it is impossible at present to articulate very specific step-by-step guidelines for engaging in clinical experimentation. The notion is in its infancy and at present time its success in everyday clinical practice depends on the ingenuity and skill of the particular clinician involved (p. 28).

Once this process has resulted in an acceptable formulation, the ‘modification methodology’ is developed accordingly. In the first instance, agreement is sought with the patient regarding both the formulation and intervention hypotheses. The ‘ideal plan’ emerging from this is not always acceptable or practical, but any compromise must be supported by the formulation. On the other hand, a valid formulation should also enable the clinician to make predictions about obstacles that are likely to occur during the course of treatment. Another important aspect in the design of treatment is described as ‘intervention sequencing’, which is a formulation-based prioritizing of treatment targets, especially with complex cases comprising
several complaints that may be interdependent. As Turkat (1990) points out, this is in stark contrast to ‘technical’ BT (symptom focused), where only the most obvious presenting complaint receives attention. Finally, the style of relationship is also developed on the basis of the formulation, thus rejecting standard enhancement techniques as, e.g. applied in the Rogerian approach. These points will be elaborated further in the next chapter.

The Contribution of David Lane

I conclude this chapter with a brief introduction and evaluation of David Lane’s work, also a former student of Vic Meyer, who has gone furthest in refining and operationalizing the case formulation approach. As his approach was predominantly developed in school settings working with children and adolescents, the label ‘context-focused analysis’ was proposed (Lane, 1990).

Lane has been developing his model over a span of two decades. It provides a particular challenge as it focuses on a particularly difficult-to-treat client group. In more detail, Lane suggested five phases for the complete individualized case formulation procedure. This was labelled as DEFINE:

Define the problem or objective, Explore the factors of influence, Formulate an explanation of factors of influence, Intervene using an action plan based on the formulation, and Evaluate the outcome of the plan based on the formulation (p. 116).

In the definition phase, information is sought from those who are involved with a problem as help is not always requested by the sufferer (especially important in the context of children and adolescents). The goal is to identify a target problem:

The process is one of growing awareness (p. 118).

In the exploration phase, expert-led gathering of relevant data proceeds to determine influencing factors for the presented problem according to principles of learning:

The process is one of observation (p. 118).

In the formulation phase, observations are evaluated and integrated to achieve a model of explanation. Behavioural experiments are carried out to test the validity of explanations:

The process is one of hypothesis testing (the pragmatic scientist) (p. 118).

In the intervention phase, a treatment strategy and plan is developed that builds logically on the formulation:
It will ideally specify the ‘what, how, who and when’ necessary for behaviour change. The process is one of structured practice (p. 119).

Finally, in the evaluation phase, problem- and goal-oriented measures are employed to assess gains and failures and determine new objectives, if necessary. Reinforcing feedback is provided throughout:

The process is one of monitored achievement (p. 119)

Lane’s operationalized model has served as the blueprint for the current UCL model, which will be presented in the following chapter.

More recently, Corrie and Lane (2006, 2010) have further developed a generic framework for formulations labelled ‘The Purpose–Perspectives–Process Model’. They define this …as the construction of a narrative which provides a specific focus for a learning journey. This learning journey takes the client from where they are now to where they want to be, based on a process of negotiating appropriate goals. The task of formulation centres on the creation of a shared framework of understanding that has implications for change (see further elaboration of this approach in the chapter by Lane and Corrie of this volume).

Finally, there were substantial contributions by Victor Malatesta and Peter AuBuchon regarding formulation-guided therapeutic relationship in case formulation (AuBuchon & Malatesta, 1998; see also chapter by AuBuchon in this volume). Both were involved in the development of formulation-based inpatient programmes (Malatesta & AuBuchon, 1992) building in the main on Turkat’s work. Their contributions seem especially important as they are clinically based and are providing strong evidence for successful outcomes, despite the restrictions and limitations in a routine service provision. Malatesta also contributed to the ongoing debate on formulation-guided versus manual-based treatment strategies (e.g. Malatesta, 1995a, 1995b). This issue was discussed in the first edition of this volume (Bond, 1998).

Overall, Ira Turkat’s and David Lane’s elaborations have been most influential for refinement of the UCL case formulation model. Ongoing collaboration led to further conceptual clarity and optimization of clinical operationalizations. The current model shall be presented in the next chapter.

Concluding Remarks

Since the publication of the first edition, there has been a growing number of publications on case formulation in the cognitive–behavioural framework and beyond.

It does not seem relevant in the present context to offer a comprehensive review of these developments; this has already been provided elsewhere
(e.g. Eells, 2007; Sturmey, 2008, 2009; Tarrier, 2006). Some additional comments shall be offered here instead.

To begin with, there appears considerable disagreement about historical perspectives. For instance, where Eells (2007) and also Tarrier (2006) assert that case formulation is a recent development, Sturmey (2008, 2009) points to long tradition starting in the 1960s. It was already stated earlier that case formulation procedures following a scientist-practitioner approach were instrumental in developing BT in the first place.

It is also noticeable that the term ‘case formulation’ is now being applied with a much broader meaning than originally intended by the pioneers of this approach. Some definitions would no longer include individualized clinical–experimental procedures as a core aspect of case formulation. Instead, it may merely describe enhanced individualized clinical practice within the preferred model and often with reference to diagnostic classification. This can mean that many forms of assessment and analysis found in various theoretical approaches may be labelled as such. Is this a useful and logical way of proceeding?

For example, Kanfer’s early work on learning theory–inspired functional analysis (Kanfer & Saslow, 1969) has been described as an early version of case formulation (Sturmey, 2009). It is also curious how (and why?) various case formulation models are grouped together into general categories. For example, the present UCL model with its traditional behavioural background, later expanded with cognitive concepts, is now listed by Sturmey as ‘non-behavioural!’ Obviously, on close inspection, one finds that case formulation models tend to be unique in promoting individualized procedures and might incorporate concepts from a number of theoretical frameworks. For these reasons, such classifications appear oversimplistic and unnecessary as they serve no purpose.

Sturmey and Eells also discuss case formulation models from other theoretical perspectives. In both volumes, clinical illustrations are provided and with Sturmey, for the first time, direct comparisons between models of common clinical problems derived from different theoretical orientations were made possible using a rather complex procedure. A detailed description of a case was given to two clinicians of different orientations to formulate. Thereafter, these case formulations were given to a third clinician of another orientation for comment. As this may be an intriguing way of comparing approaches, results obtained must be judged with caution as standardized transcripts rather than real patients were used. It also seems unclear what the real benefit of such comparisons is. We know already that different models might work with different clients using their own methods of evaluation. Sturmey’s claim is to fill a gap as “…educators providing professional training in case formulation, students and practitioners lack resources to learn about these differences.” (p. 13).
Another issue of debate refers to empirical validation of case formulation (e.g. Kuyken, 2006). How should the utility of case formulation approaches be evaluated? And is it appropriate to make comparisons with diagnostically led standardized treatments?

Eells (2009) finds it puzzling that, given increasing attention and popularity, there is very little research in evidence. But what kind of research might be suitable and appropriate given the experimental and dynamic methodology of case formulation? Current proposals are mostly designed to achieve a psychometric type validity and reliability assessment. Obviously, as Eells points out, “…reliability should not be achieved at the expense of producing generic ‘one size fits all’ formulations that might satisfy us psychometrically but not fit the person well…” (p. 294). It seems questionable whether these are suitable criteria for evaluation of case formulation.

Instead, preference should be given to individual evaluation of treatment utility. Obviously, a functional and experimentally guided case formulation process designed to investigate, select, interpret and verify or reject information from the very beginning of the interview can be regarded as a unique individualized research procedure (see also Chapter 2). For these reasons, any empirical validation of case formulation should be led by advanced methodology in single-case experimental design (Barlow & Hersen, 1984; Petermann & Müller, 2001). Petermann and Müller have made useful suggestions for such strategies; however, it has to be acknowledged that such methodology might pose an enormous challenge. And of course, this should include subjective evaluation by respective clients regarding both presenting complaints and general satisfaction with their lifestyles – after all the proof of the pudding would be in the eating.

As it is not recommended to compare apples with pears, RCTs to evaluate treatment utility of case formulation in comparison with standard procedures appear equally problematic and are hardly compatible with the rationale of case formulation. For example, the frequently cited Schulte study (Schulte and coworkers, 1992) did not find any significant differences between standard procedures and case formulation. However, on close inspection, serious flaws can be detected, i.e. case formulation therapists were rather inexperienced and in the standard condition treatments were individualized to some degree.

In clinical training, the therapist ought to be encouraged to provide a running commentary on his action to make his reflections as transparent as possible, for himself, for observers and of course for the client himself. In addition, the role of co-therapy and close supervision should assume special significance. In more detail, a co-therapist might monitor the process for comparing notes and critical discussion. The supervisor might scrutinize any transcript or videotape line by line, for instance, to investigate the process of generating hypotheses leading to testing questions (see also Chapters 2–4).
In reviewing recent developments in case formulation, it is also apparent that behavioural concepts, previously out of fashion and almost forgotten, appear to be staging a comeback as, e.g. demonstrated with Sturmey’s Functional-Analytic Model (Sturmey, 2008, 2009). As a ‘radical behaviourist’, Sturmey rejects structuralism in preference to functionalism in psychotherapy. In his recent text (Sturmey, 2008), Sturmey offers a comprehensive overview of basic learning principles that had formed the bedrock for pioneering clinical–experimental work in the establishment of BT. Another example is Persons’ (2008) recent expansion of her cognitive model (ironically originally inspired by Turkat’s (1985) behavioural model [see discussion with Bruch, 1998]), which now incorporates traditional learning theories, clinical hypothesis testing and behavioural analysis procedures as well as emotional processing concepts. However, the model still relies strongly on diagnostic classification and associated assessment methods and also treatment protocols based on randomized controlled research trials – a square peg for a round hole?

Despite many remaining problems with case formulation, the surge of interest and activities has to be welcomed. This can be seen as evidence that both therapists have become increasingly dissatisfied with standard techniques according to the nosological model. And this seems especially relevant for complex and chronic problems labelled as personality and psychotic disorders. Recent advances in these fields owe much to experimental case formulation approaches (e.g. Turkat, 1985). At UCL, we began in the early eighties to treat such difficult clients in the experimental spirit of the early pioneers of BT. These patients were considered as little motivated and unresponsive to standard treatments; however, closer analysis did inspire new concepts of understanding as well as innovative ideas for treatment (e.g. schema-focused therapy; Bruch, 1988).

References


