

An Historical Perspective on the Impact of Case Formulation

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When I introduced the term *case formulation* into the behavioral literature (Bruch & Bond, 1998, p. 3; Johnstone & Dallos, 2006, p. 8; Johnstone & Dallos, 2013, p. 10), the goal was to move the field away from a focus on treatment techniques to developing behavioral conceptualizations of psychopathology (Meyer & Turkat, 1979; Turkat, 1982, 1985; Turkat & Meyer, 1982) as the foundation for intervention innovation, and subsequent scientific investigation. As part of that effort, a definition for a case formulation was provided with specific criteria as to what precisely constituted a case formulation and, therefore, what did not. This appeared essential to me at that time in our discipline's history because, despite concerted effort, I was unable to locate even one definition of what specifically was meant by claiming one had a clinical conceptualization or formulation of a particular case. Back in those days, behavior therapists were a clear minority and the diagnostic manual of American psychiatry, DSM-II (American Psychiatric Association, 1968)—a psychoanalytic-influenced document of unacceptable reliability and validity (Langenbucher & Nathan, 2006)—made us cringe.

Fast forwarding, we find the term “case formulation” has evolved. Today, it represents a core skill required of every clinical psychologist belonging to the 50,000 member British Psychological Society (2011).

Likewise, the latest edition of American psychiatry's diagnostic manual states: “The primary purpose of DSM-5 is to assist trained clinicians in the diagnosis of their patients' mental disorders as part of a case formulation . . .” (American Psychiatric Association, 2013, p. 19). Regardless of where one stands on issues relating to psychiatric diagnosis (e.g., Meyer & Turkat, 1979; Turkat & Maisto, 1983), it is fair to say that decades following its introduction, the term “case formulation” has come a long way.

The British Psychological Society's (2011) issuance of best standard practice guidelines exclusively for case formulation is the first of its kind. Therein, the history of case formulation is summarized and four “influential clinicians” (p. 4) were identified who helped to create the field: Hans Eysenck, Victor Meyer, Monte Shapiro, and me. As the only living member of that group,¹ I would like to offer some ideas I hope will positively impact the future of case formulation and, in turn, help the recipients of our efforts in the mental health professions at large.

Early Innovations

There is no doubt that the road from clinical hypothesis creation to evidence-based treatment has been a successful journey, exemplified superbly by some of

behavior therapy's early pioneers, such as British psychologist Victor Meyer and South African psychiatrist Joseph Wolpe.

Today, response prevention for compulsive motor rituals is a well-established evidence-based treatment (Simpson, Maher, Page, Gibbons, & Foa, 2010). In the 1950s, this disturbing and often incapacitating clinical abnormality was consensually viewed as untreatable. Creatively applying learning principles in the psychiatric setting at that time (e.g., Meyer, 1957; 1966), Vic developed response prevention from his clinical observations and reasoning about such cases in relation to the animal literature on ritualistic behavior (cf. Abramowitz, Taylor, & McKay, 2012). From there, Vic hypothesized that preventing the performance of such rituals in clinical patients when exposed to the eliciting stimuli would force a reduction in their frequency and then demonstrated it successfully with compulsive hand-washing cases (Meyer, 1966). The rest is history (Foa, 1996). Without Vic's formulation-based treatment innovation, thousands of patients who suffered from debilitating compulsive motor rituals might still be unable to function. Detailed accounts of and references to his clinical ingenuity are available in Bruch (1998; 2014; Bruch & Bond, 1998; Bruch & Prioglio, 2006), the authoritative expert on Vic Meyer's approach and contributions to the field.

Likewise, the development of the first scientifically documented behavioral treatment of phobic conditions—systematic desensitization—traces its historic roots to Joseph Wolpe's conceptualization and demonstration of the genesis of anxiety as a function of conditioning by inducing and eliminating experimental neurosis in cats (Wolpe, 1952). Impressed negatively with the treatments of his day (i.e., psychoanalysis and medication therapy), Wolpe successfully adapted his conditioning conceptualization of anxiety to modify the suffering of “war neurosis” among some of South Africa's World War II soldiers (Wolpe, 1958). His creative desensitization method was revolutionary (Rachman, 2000) and with his quantification of impressive patient improvement, facilitated the rapid growth of empirical approaches to treatment of behavioral problems. Many across the globe view Joe Wolpe as the father of behavior therapy (e.g., DiTomasso, Golden & Morris, 2010; Grawe, 2000;² Kaushik, 1988;³ Prochaska & Norcross, 2013; Stein, 2012), but I suspect he would nominate the Russian physiologist Ivan Pavlov (see Wolpe & Plaud, 1997).⁴ Ultimately, Joe was

¹ For readers less familiar with these individuals, a few brief facts might prove beneficial. Hans Eysenck devoted his career to developing clinical psychology in the UK as a science and at the time of his death in 1997, was one of the three most cited intellectuals in history—the others being Sigmund Freud and Karl Marx, according to Social Science Citation Index data (Jensen, 1997 p. 543). In the 1950s, M.B. Shapiro pioneered clinical training in the UK, where he developed and taught his innovative application of the experimental method to the problems of the individual case (cf. Turkat & Maisto, 1985), and was the most impactful British psychologist in developing the integration of science and clinical practice (see D. Shapiro, 2002). Vic Meyer is widely known for his pioneering efforts to creatively apply learning principles to complex cases viewed traditionally as treatment resistant, resulting in a highly individualized, formulation-based approach that broadened behavior therapy beyond the more prevalent technique orientation existing at that time (see Bruch & Bond, 1998; Bruch, 2014).

² I thank psychiatrist Irmgard Oberhammer of Austria for providing this reference.

³ I thank Professor Sandhya Kaushik of India for recently confirming her 1988 opinion stands today.

⁴ Designation as the “father” of behavior therapy is ultimately subjective and a thorough analysis of the topic is beyond the scope of the present article.

known best for his innovative treatment method, although his writings on behavior analysis (Wolpe, 1973, 1976, 1977, 1982) were highly instructive and he insisted on first developing a proper case formulation prior to clinical intervention (Wolpe & Turkat, 1985).

From our joint efforts I can assure the reader that Vic Meyer (Meyer & Turkat, 1979; Turkat & Meyer, 1982) and Joe Wolpe (Wolpe & Turkat, 1985) would have advocated against using their (now scientifically supported) intervention procedures with every case of motor rituals and phobias, respectively. Rather, each emphasized the importance of first developing a proper case formulation from which treatment would be devised specific to the clinical indicants of that formulation. For both of these pioneers, the unique aspects of the individual case required an idiographically designed treatment which may or may not require devising novel intervention. In their day, they did not have the luxury of evidence-based treatments; they created them.

Following introduction of the initial definition for the term “case formulation,” interest in the behavioral community accelerated (see Sturme, 2008), including its utilization in cognitive therapy (Persons, 1989) with continual elaboration (see Persons, 2012; Persons & Davidson, 2010; Persons & Tompkins, 2007), and the term became fashionable more broadly with all kinds of permutations appearing, such as psychodynamic case formulation (Perry, Cooper, & Michels, 1987), psychiatric case formulation (Sperry, Gudeman, Blackwell & Faulkner (1992), psychoanalytic case formulation (McWilliams, 1999), multimodal case formulation (Gardner, 2003), biopsychosocial case formulation (Ingham, Clarke & James, 2008), and psychotherapeutic case formulation (Berthoud, Kramer, de Roten, Despland, & Caspar, 2013)—to name a few.

Putting aside all the spin on the term and the consequent mishmash, the mere fact that specific guidelines on case formulation have been developed as a requirement for practice in the United Kingdom is a landmark event and praiseworthy. Given the range of theoretical diversity among clinical psychologists, I did not imagine this would have been an easy task and a reading of the guidelines on case formulation pro-

vides a sense of the underlying struggles. Nonetheless, a fine effort was made and I am confident that with each successive revision we shall see continual improvement.

Definition of Case Formulation

To the best of my knowledge, the first definition of a case formulation with requisite criteria was provided in 1979 as an explanatory hypothesis that:

- (1) relates all of the patient's complaints to one another, (2) explains why the individual developed these difficulties, and (3) provides predictions regarding the patient's behavior given any stimulus conditions. (Meyer & Turkat, 1979, p. 261)

Numerous illustrations of the use of this definition were provided back then along with instruction on how to meet the criteria in clinical practice (see Meyer & Turkat, 1979; Turkat, 1982, 1985, 1986, 1987, 1990; Turkat & Carlson, 1984; Turkat & Levin, 1984; Turkat & Maisto, 1985; Turkat & Meyer, 1982; Wolpe & Turkat, 1985). At the time we introduced this definition, we hoped it would stimulate a new direction in the field. It did. Just a few years following our original definition and subsequent elaborations, I was invited to present our approach to American psychiatry, and in 1986 our definition appeared in the *American Psychiatric Association Annual Review*:

The behavioral formulation is defined as an hypothesis that: 1) specifies the mechanism responsible for all of the symptoms presented by the patient; 2) details the etiology of these problems; and 3) provides predictions of the patient's behavior in future situations. (Leibowitz, Stone & Turkat, 1986, p. 358)⁵

Decades later, one can find a variety of behavioral approaches to case formulation today, and for recommended comparative reviews the reader is referred to Sturme (2008, 2009). Likewise, other theoretical orientations (e.g., psychodynamic, systemic) have come to offer positions on case formulation as well and an examination of common and contrasting features can be found in thoughtful analyses by Corrie and Lane (2010), Johnstone and Dallos (2006, 2013) and Sturme (2009).

Now that the term “case formulation” has grown to be commonplace in the mental health literature with its adoption and adaptation by diverse schools of thought, it should come as no surprise that there is no universally accepted definition for it (British Psychological Society, 2011). Likewise, the American Psychological Association Presidential Task Force on Evidence-Based Practice (2006) emphasized the importance of case formulation but was silent on its definition. Thus, a significant void remains. What one calls a “case formulation” others do not. This is most unfortunate because without agreement on definition, it perpetuates a sea of conceptual mud. Science cannot advance well without consensually approved operational definitions. And ultimately, the best definition of what constitutes a case formulation is an empirical question.

So what definition of case formulation should we use as a starting point? I would certainly welcome a comprehensive and clearly conclusive body of scientific literature supporting a superior definition than the original one Vic and I provided decades ago that remains commonly used today in various parts of the world (e.g., Antick & Rosqvist, 2002; AuBuchon, 2014; Australian Centre for Posttraumatic Mental Health, 2012; Bruch, 2014; Malatesta, 2010), not to mention others' adaptation, evolution, or reformulation of it (see Corrie & Lane, 2010; Lane & Corrie, 2006; Persons, 1989). Unfortunately, that sorely needed body of scientific facts does not exist (cf. Hart, Sturme, Logan, & McMurrin, 2011; Sturme & McMurrin, 2011). So, in the absence of clear scientific data to determine how best to define a case formulation, it leaves us in the unsettled state of competing arguments and advocacy. It is one thing to provide a definition when one did not exist (i.e., Meyer & Turkat, 1979), but quite another to have a scientific consensus for a definition derived from a highly developed, incontrovertible body of research. Science should ultimately dictate the best definition of case formulation for researchers and clinicians to use, not the appeal of any one particular advocate.

From the very beginning, our definition of case formulation aimed to place requirements on the clinician to put one's thinking on the line in a clear and comprehensive way, with potential benefits not just for the client in the room but for the field at large. In other words, we strived to not only provide a definition where one did not exist but to raise the bar. As noted above, we defined case formulation as having specific compo-

⁵ As noted in this section of the *Annual Review*, coverage responsibility was assigned as follows: Leibowitz (psychopharmacology), Stone (psychoanalytic psychotherapy), and Turkat (behavior therapy). I provided this definition. Leibowitz and Stone are prominent academic psychiatrists; I did not know them prior to the invitation.

nents, and failure to meet each component meant one did not have a case formulation. To illustrate this point, the reader may find the case of a “dependent personality” (Turkat & Carlson, 1984)⁶ instructive. In that case, initially we were unable to meet our definition for a case formulation and thus our failure was discussed with the patient. This led to symptomatic treatment. It failed. During the course of symptomatic treatment, interactions with the patient led to discovery of clinical information that enabled us to meet the definition of a case formulation. We then went about testing its validity. After documenting objectively successful predictions of the patient’s behavior, we devised a treatment specific to the case formulation that proved efficacious and maintained at follow-up.

Since decades later we do not have a consensual operational definition for case formulation, nor is the science even close to generating the necessary data supporting one, we find ourselves in a dilemma. If we do not agree a priori on what is and isn’t a case formulation for all to use, we remain at risk to find ourselves in a cloud of conceptual clutter. I will propose a potential solution to this dilemma, a little later in this article.

Psychiatric Diagnosis and DSM-5

Scientists need reliable and valid ways to classify and communicate about the phenomena they study or their field cannot advance optimally (Adams, 1981). Psychiatric diagnosis is a classification system. Whether it is a good classification system or not is beyond the scope of the present manuscript. However imperfect the DSM-5 may be, psychiatric diagnosis aims to provide shorthand descriptions of certain behaviors that tend to group together across individuals that may be exhibited by the person under study. Hence, diagnosis is primarily descriptive.

Case formulation, on the other hand, provides *an explanatory theory with predictive power specific to the behavior of the individual case*. Two individuals with the same diagnosis may have very different case formulations (cf. Leibowitz et al., 1986; Meyer & Turkat, 1979; Turkat & Maisto, 1985; Turkat & Meyer, 1982) and consequently, different treatment. Diagnosis and case formulation complement each other but their purposes are not the same (Turkat & Maisto, 1983)—a point now recognized by

many in psychiatry (Winters, Hanson, & Stoyanova, 2007). Further, some psychiatrists admit that psychiatric diagnoses “. . . do not help us predict which patients are suitable for which therapy” (Sim, Gwee, & Bateman, 2005, p. 289). A good case formulation does.

The most widely used system for classification in the mental health professions today is the DSM-5 provided by the American Psychiatric Association (2013). In light of the importance of classification to science and the widespread acceptance of DSM-5, I support the use of psychiatric diagnosis in appropriate circumstances, mindful of its limitations and potential negative impact (see British Psychological Society, 2011; Frances, 2012; Zeev, Young, & Corrigan, 2010). However, when it comes to case formulation, there is a significant problem with DSM-5. More specifically, as noted above, the current version of the classification system defines psychiatric diagnosis as a component of a case formulation (American Psychiatric Association, 2013, p. 19). It is here where we have a clear and fundamental difference with DSM-5: A case formulation does *not* include psychiatric diagnosis as a part of it. It never has.

Definitionally, developing a case formulation does not preclude or require simultaneous classification via psychiatric diagnosis, but the former does not include the latter. Whether one chooses to utilize the classification system provided in DSM-5 is an independent decision when formulating a case, based on a number of factors the evaluator faces (e.g., client best interest, insurance requirement, research design, etc.). The way DSM-5 uses the term “case formulation” muddies the water. Related terms, such as “diagnostic formulation” (Kuruville & Kuruville, 2010), however well-intended, perpetuate the confusion (Turkat & Maisto, 1983).

Let us take it upon ourselves to educate our less knowledgeable colleagues and students on the differences between case formulation and psychiatric diagnosis.

Case Formulation and Clinical Duty to the Field

The first and primary duty of the clinician is to the well-being of the person one is charged with helping. This involves utilizing established scientific findings to guide intervention wherever indicated based on an accurate case formulation. However,

proper application of evidence-based treatments to problems well understood in the scientific literature does not make one’s clinical activity “scientific.” Rather, such application should be considered standard practice today. But when necessary scientific information is unavailable, *the clinician bears a secondary duty* to the field at large. There are several ways to fulfill this responsibility beyond routinely providing the highest level of practical and ethical service delivery. Each has to do with contributing to the scholarly literature.

In that regard, in cases where there is inadequate scientific information, not only is it the duty of the clinician to attempt to conceptualize and hopefully come to ameliorate the presenting difficulties, one should utilize the opportunity when merited to pass worthy information on to those with proper investigative resources and thereby, potentially expedite the growth of scientific knowledge (Turkat, 1988, 1990). More specifically, clinicians can and should contribute useful information about: (a) the mechanisms of poorly understood problems; (b) the etiology of such difficulties; and (c) the creation of new assessment and treatment methods.

Wherever appropriate and reasonably possible, the clinician applies the experimental method to test the validity of one’s thinking (Carey, Flasher, Maisto & Turkat, 1984; Meyer & Turkat, 1979; Turkat, 1984; Meyer & Turkat, 1979; Turkat, 1990; Turkat & Maisto, 1985; Turkat, Maisto, Burish, & Rock, 1988; Turkat & Meyer, 1982) in addition to evaluating treatment efficacy (Barlow, Nock & Hersen, 2008; Kazdin, 2011). Of course, there are plenty of obstacles to consider, especially in regard to the former.

First, compared to the laboratory, there is usually a larger set of uncontrolled variables involved with limited resources to implement control or well-structured study. Second, there are institutional considerations such as insurance restrictions, managed care limitations, mandatory facility protocols, time constraints, and other potential problems (e.g., legal liability) when introducing novel procedures. I think back on a case of formulation-based treatment of an incapacitated and hospitalized vomit phobic (see Turkat & Meyer, 1982) in which we flooded the patient in vivo over 2 days (i.e., numerous individuals actually vomiting on the patient or within her reach); it is hard to imagine such novelty readily receiving institutional approvals today.

Let me now turn to the importance of clinicians reporting information pertaining to the etiology of poorly understood disorder

⁶ At the time this case was seen, DSM-III (1980) was the authoritative nomenclature in American psychiatry.

ders. As I have stated elsewhere, “The ultimate goal in the field of mental disorders is to prevent psychopathology from occurring at all” (Turkat, 1988, p.196) and “. . . the clinician can play a useful role in this endeavor” (Turkat, 1988, p.185). Clearly, preventing behavioral problems from developing in the first place has far greater appeal than not doing so and then providing amelioration. Practitioners are the “first responders” and, as such, clinical study of etiological factors may provide important clues for the prevention of particular behavior problems (Turkat, 1986). The recently published American Psychological Association (2014) guidelines on prevention underscore this point.

Aligning Scientists and Clinicians on Case Formulation

Finally, I wish to address those researchers with a specific interest in case formulation, and for those scientists who don't have such an interest—I hope you acquire it.

Since we don't have a consensus on an operational definition for case formulation, we need to change this state of affairs. There

are two obvious routes. The first is to wait for our science to generate a solution organically. Unfortunately, over three decades have passed since introducing the initial definition of case formulation requiring specific criteria to be met, and we are still waiting.

The second route is to attempt to speed things up by bringing together leading scientists and clinicians on case formulation to work on providing a consensual statement on an operational definition for it. The aim should be to:

1. Provide no less than one consensually derived operational definition of a case formulation that will meet clinical and research standards;
2. Restrict the number of such definitions to the fewest needed to reach consensus, the ideal number being one;
3. Label the consensually derived operational definition(s) of a case formulation in a distinctive way to facilitate professional communication and signify collective expert endorsement;
4. Promote the use of the consensually derived operational definition(s) of a case formulation actively to all clinicians and

researchers for immediate application; and

5. Evaluate the case formulation operational definition(s) periodically following appropriate intervals of accumulated scientific and clinical findings with an eye toward improvement.

At this time in our discipline's history, I support the second route.

When the American Psychological Association and British Psychological Society addressed case formulation in recent years, their pronouncements were reflective of large memberships holding a wide range of theoretical views that naturally inhibits specificity in some areas in order to bridge significant differences. In contrast, the readership of *the Behavior Therapist* is far more homogeneous and less constrained by the size of the pool of professionals needing to come together. As such, developing a consensual operational definition for case formulation would appear more likely to emerge from this group, if it accepts the challenge to do so. A step back in history may help put the present proposal in perspective.



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As stated on the website of the Association for Behavioral and Cognitive Therapies in regard to its development:

The organization was originally founded in 1966 under the name Association for Advancement of Behavioral Therapies . . . by 10 behaviorists who were dissatisfied with the prevailing Freudian/psychoanalytic model (founding members: John Paul Brady, Joseph Cautela, Edward Dengrove, Cyril Franks, Martin Gittelman, Leonard Krasner, Arnold Lazarus, Andrew Salter, Dorothy Susskind, and Joseph Wolpe).

No doubt, these individuals helped to forever change the mental health professions by taking that step forward. Today, using the same commitment to the same principles, once more we have the opportunity to sharply advance the field by developing an operational definition of case formulation acceptable consensually to clinicians and researchers alike. In 1966, *resistance to a behavioral approach could not ultimately overcome the wealth of forthcoming scientific data* resulting in today's evidence-based treatments. The historical lesson is clear. Right now, the time is ripe, the need is strong, and the potential benefits run deep, if we take the lead once more.

Let us dedicate ourselves today to facilitating a more rapid growth of scientific and clinical knowledge of case formulation along the lines recommended herein and unleash the great potential for its impact. Worry not about the early imperfections that may emerge, for in time—science will remedy them.

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Clinical Forum

A Preliminary Investigation of the Effects of Aerobic Exercise on Childhood Tourette's Syndrome and OCD

Loren Packer-Hopke and Robert W. Motta, *Hofstra University*

In the April 2014 issue of *the Behavior Therapist*, ABCT president Dean McKay decried the medicalization of mental illness. One such "illness" that is most frequently treated medically is Tourette's Syndrome (TS). TS is often comorbid with OCD and drug therapies are the most frequently used forms of treatment (Piacentini & Chang, 2001). Given the significant role played by anxiety in TS and the utility of exercise in reducing anxiety, the purpose of this study was to explore the effects of aerobic exercise on children and adolescents with comorbid TS, obsessive-compulsive disorder (OCD), and anxiety.

TS

TS is characterized by the presence of both motor and vocal tics (Leckman, Bloch, Scahill, & King, 2006). The onset of TS is

before age 18, although most cases present early in life, within the first 7 years. TS is often comorbid with other disorders, most frequently OCD and attention-deficit/hyperactivity disorder (ADHD). The prevalence of individuals with comorbid TS and OCD is high; some researchers have found that the prevalence of this comorbidity is between 40% and 60% (Chang, McCracken, & Piacentini, 2007; Kadesjo & Gillberg, 2000).

CBT Intervention

The most common forms of behavioral therapy used in treating TS include self-monitoring, habit reversal training (HRT), and CBT (Woods, Conelea, & Himle, 2010). HRT includes awareness training, relaxation, and the regular practice of socially acceptable, competing responses to the tics (Azrin, Nunn, & Frantz, 1980;

Azrin & Peterson, 1988). Similarly, the CBT procedures of exposure and response prevention are behavioral interventions often used in treating OCD.

Psychological Effects of Aerobic Exercise

Aerobic exercise has been used as an adjunct to therapy, or even a replacement for therapy, for a number of different disorders or ailments including, but not limited to, the following: OCD, chronic fatigue, post-traumatic stress disorder (PTSD), depression, anxiety, low self-esteem, and poor quality of life (Abrantes et al., 2009; Broman-Fulks & Storey, 2008; Brown et al., 2007; Gordon, Knapman, & Lubitz, 2010; Lancer, Motta, & Lancer, 2007; Motta, McWilliams, Schwartz, & Cavera, 2012; Newman & Motta, 2007; Smits et al., 2008). In one study exploring the effects of aerobic exercise on childhood PTSD, anxiety, and depression, participants were required to exercise 3 times a week for 20 minutes each workout, lasting a total of 8 weeks (Newman & Motta, 2007). The results indicated that this type of exercise regimen led to significant reductions in PTSD, anxiety, and depression, with lasting effects measured at 3-month follow-up.

Similarly, studies on the effect of aerobic exercise on OCD symptoms have shown that 12 weeks of moderate-intensity exercise 3 to 4 times per week can reduce OCD symptoms drastically, to the point that