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Functional analysis and response covariation in the assessment of personality disorders: a reply to Staats and to Bissett and Hayes

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Abstract

In this reply to Bissett and Hayes (this issue) and Staats (this issue) we address critical comments in response to our initial proposal and highlight points of agreement. The overall thesis of our reply is that classification schemes based on nomothetic response covariation, such as DSM, complement, but do not substitute for, an idiographically-based functional analysis and behavioral assessment. In the context of our reply, we address the following primary concerns raised by Bissett, Hayes, and Staats: (a) we are essentially proposing the melding of two theoretically incongruent approaches, and that such a melding is inherently not viable or useful; (b) the behavior analytic approach cannot account for personality or psychological constructs; and (c) that categories based on topography do not have demonstrated treatment utility. We also discuss points of agreement with our respondents: (d) a theoretically-based descriptive classification system is required to ultimately advance clinical science, (e) the DSM personality disorder classification system, to remain viable, needs a stronger empirical base; and (f) that alternatives to DSM classification that more strongly emphasize behavioral principles are in need of development. © 1999 Elsevier Science Ltd. All rights reserved.

1. Introduction

Staats (this issue) and Bissett and Hayes (this issue) have provided thoughtful commentary on our article. The differing perspectives expressed in these commentaries reflect a broad range of views on the potential utility of nomothetic response covariations in forming an idiographically-based functional analysis and behavioral assessment. In response to the issues

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raised by these commentators, our reply seeks to address the main criticisms leveled at our approach and to highlight points of agreement we share with our commentators.

2. Responses to criticisms

2.1. Our melding of two incongruent approaches, or eclecticism, is neither viable nor useful

Bissett and Hayes suggest that we advocate the combining of two theoretically incongruent approaches to the description and accounting of behavior, namely syndromal classification of DSM and behavior analysis. We would argue that there is not necessarily a theoretical inconsistency present. To do so, we must first distinguish between some of the theoretical assumptions and philosophical tenants that eventually gave rise to DSM syndrome classification and those of an alternative approach more consistent with our proposal, the empirical classification approach.

Contemporary DSM classification has as its roots the methodological work of the Washington University-St. Louis group who, in the 1970s, formulated an approach to classification based on the proposal of discrete syndromes, the delineation of operational criteria that define these syndromes, standardized interview techniques for assessing the criteria that define syndromes, and the development of statistical tests to evaluate the reliability of diagnostic judgments (Klerman, 1986). This group was also central to the development of what would be eventually referred to as the neo-Kraepelinian movement, which largely consisted of psychiatrists who believed that mental disorders could be accounted for by biological or genetic etiologies and that such disorders are best classified using a categorical framework (Klerman, 1986; Morey, Skinner, & Blashfield, 1986). The contemporary syndromal classification approach in DSM has been significantly influenced by this school of thought and is widely reflected in its structure. Among other indications, the DSM framework implies that behaviors associated with diagnostic concepts are caused by underlying syndromes which, in turn, are diagnosed as present if the behaviors that define those syndromes are manifest to some specified degree (see Hickey, 1998).

Theoretical incongruence in our proposal would be evident if we simultaneously embraced the theoretical tenants of neo-Kraepelinian movement and the assumptions underlying functional analysis. We do not. Similar to the neo-Kraepelinians, we agree that a reliable system of classification is important for the advancement of a clinical science, and that classification systems should be based on some set of objective criteria, which includes among the possible options behavioral sets that demonstrate reliable nomothetic response covariation. However, the similarity of our approach to those of the neo-Kraepelinians stops there. We do not embrace the concept of syndrome in the traditional sense, and are troubled by the degree of logical circularity and reification inherent in the DSM approach to classification (Hickey, 1998). We do see, however, the potential utility for behavioral assessment of a topographical classification system based on nomothetic response covariation. Such a system has the potential to identify commonalties across sets or subsets of persons, a goal that a strictly idiographic approach to the understanding of individuals cannot realize. To this end, the methods and approach of the empirical classification movement (e.g.,

Blashfield, 1986; Morey et al., 1986; Skinner, 1986) can potentially inform behavioral assessors of responses that nomothetically covary through the use of appropriate sampling and statistical methods, and can evaluate the degree of association that such behaviors have with other clinically relevant phenomena or behaviors.

In a somewhat different manner, Staats has suggested that our approach is an exemplar of the behaviorizing of psychology (see also Staats, 1995). Staats' criticism is not that "behaviorizing psychology" is something to be avoided in the absolute sense. Rather, he has asserted that this approach, done within a coherent theoretical framework, can bridge behavioral concepts and principles to the observations, concepts, and methods of traditional psychology. Staats (1995) has further suggested that for behaviorism to remain relevant or important to psychology, such bridging is necessary and should serve as a principle goal to guide future behavioral research.

Staats' main concern with our article is not that we are behaviorizing psychology, but rather that we do this without the benefit of a coherent theoretical framework that has the ability to bridge principles of behaviorism to traditional psychology. This evaluation has led Staats to conclude that our approach is eclectic and, as such, is inherently limited in its utility. We disagree. For example, Staats has remarked that our acknowledgement of recent therapies that target emotions to produce behavior change in other response modes is incongruent with the functional analytic approach. In response, we see the focus on the idiographic assessment and targeting of responses within a set to change other responses as consistent with the keystone response class strategy of assessment (Nelson, 1988). This assessment approach is based on the identification of response–response relationships, and is premised on the notion that a change in one response may, in turn, produce changes in other responses within a class. From a behavioral assessment perspective, change in one response area such as emotions (e.g., the keystone) may result in changes in other response areas (e.g., approach behaviors). McKnight, Nelson, Hayes, and Jarrett (1984) provide some evidence in support of the efficacy of the keystone response class strategy for the treatment of depression.

Staats has also criticized our use of terms and phrases such as "maladaptive beliefs about the self" and "underlying psychological mechanisms are viewed as the primary causal determinants of behavior" as indicative of our eclecticism. It is important to note that in the context of this discussion, we were referring to Jacqueline Persons' (1989) cognitive modification of Turkat's (1990) case formulation approach. We recognize that provision of causal status to cognitions is consistent with the cognitive view and inconsistent with the traditional behavioral view. Finally, Staats has also raised the issue that a behavioral analytic approach cannot account for personality concepts or constructs, a criticism that we address in the next section.

2.2. The behavior analytic approach cannot account for personality or psychological constructs

If we consider personality constructs to be indicated by collections of associated overt and covert responses that are manifest across diverse environmental contexts and time, then we can conceptualize such constructs in terms of response covariations, a concept that is not inconsistent with the functional analytic view (Nelson & Hayes, 1985). Within this view, such

covariations can be explained with reference to sets or subsets of environmental contingencies that, to varying degrees, control each of the covarying responses within a response set. From the perspective of behavioral assessment, the DSM can be viewed as a diagnostic classification scheme based on presumed or previously demonstrated nomothetic response covariations (Nelson & Barlow, 1981). Although responses outlined within DSM criteria sets may covary at the group level among a subset of individuals, it is important from the standpoint of behavioral assessment to idiographically determine the degree and extent to which nomothetically identified patterns of response covariation are relevant for the particular individual being assessed.

In the case of DSM-defined personality disorders (PDs), criterion sets that define each of the PDs are often characterized by topographically distinct responses (i.e., responses that are distinctly different in form). From the behavior analytic view, each of these topographically distinct responses may be controlled by similar or identical contingencies. In other words, different PD categories may share similar functional analyses. For example, in the face of threats of abandonment in an important relationship, a person diagnosed with dependent PD may become subservient (“goes to excessive lengths to obtain nurturance and support from others, to the point of volunteering to do things that are unpleasant”—Diagnostic Criterion #5, DSM-IV, p. 668), whereas a person diagnosed with borderline PD may become enraged (“frantic efforts to avoid real or imagined abandonment”—Diagnostic Criterion #1, DSM-IV, p. 654) (Baird, 1999). These topographically different behaviors may share the same function. Nonetheless, the diagnostic categories are useful because these different behaviors are part of nomothetic patterns of response covariation.

Conversely, topographically similar behaviors may have different functions. Persons diagnosed with major depressive disorder and with borderline PD may both cut themselves, but for different reasons: the depressed person may wish to die to escape totally from life’s aversive experiences, while the borderline may wish to escape momentarily from a dysphoric state (Leibenluft, Gardner, & Cowdry, 1987). The diagnosis is useful in suggesting different functional analyses for similar behavior. Thus, in our view, while functional analyses and syndromal classification may be relatively independent, both are useful and are compatible.

Although nomothetic response covariation may imply in some instances the presence of nomothetic categories of controlling variables, these, too, would need to be evaluated for each individual case. Finally, nomothetic response covariation, in addition to suggesting nomothetic controlling variables, may also suggest effective treatments, a notion more fully elaborated below.

2.3. Categories based on topography do not have demonstrated treatment utility

Because of DSM’s atheoretical and descriptive nature, a diagnosis of PD alone does little in the way of informing an understanding of the processes involved that give rise to and maintain the behaviors that define the disorder. It is possible that the application of a functional analysis to an understanding of the behaviors that define PD concepts can assist client conceptualization based on well-established psychological principles, which is one of the main theses of our initial article.

One point raised by Bissett and Hayes in refutation of our approach was the notion that classification systems based on topography alone do not have demonstrated treatment utility. Treatment utility of assessment refers to the impact that different methods of assessment or case conceptualization have on treatment outcome (Hayes, Nelson, & Jarrett, 1987). If the goal of identifying nomothetic response topographies is to inform the selection of successful treatments, then there are indications in the existing research literature of treatment utility based on this approach. For example, one goal of the medical model approach to the conceptualization and treatment of mental disorders is to ultimately validate a patient's diagnosis through the efficacy of some sort of somatic intervention, usually psychotropic medications, thereby demonstrating the treatment utility of the diagnosis. To an extent, there is evidence available indicating that specific psychopharmacological interventions are relatively effective for some categories defined by sets of covarying symptom features (e.g., American Psychiatric Association, 1993, 1997, 1998).

Similarly, in the area of psychosocial interventions, some diagnostic categories defined by response topography have demonstrated treatment utility. Division 12 (Clinical Psychology) of the American Psychological Association, for example, has recently updated its list of empirically validated treatments (Chambless et al., 1998), with many of these treatments validated as a result of their ability to produce reductions in behaviors that define specific diagnostic concepts. Included among these is Linehan (1993) Dialectical Behavior Therapy for borderline PD. As Hayes and Follette (1992) themselves note, if syndromal classification (or classification of constructs based on nomothetic response covariation) show treatment utility, then there would be little objection to such classification from a contextualistic point of view, as the analytic strategy based on nomothetic response covariation has utility in accomplishing the primary goal of the analysis, namely the application of an effective treatment.

Additionally, apart from the fact that there is no substantial evidence suggesting that functional analysis has more treatment utility than diagnostic classification (Hayes & Nelson, 1986), there are currently several problems associated with a strictly functional analytic approach to understanding and treating human problems. Hayes and Follette (1992), for example, outline several such problems including: difficulty in performing research on functional analysis, a relative lack of systematic approaches to guide the functional analytic process, unreliability of functional analyses, and a relative absence of a framework for moving from a functional analysis to specific treatment recommendations. Moreover, the behavioral functional analytic approach is only one functional approach among other possibilities (including psychoanalytic, biological, familial, and cultural).

Consideration of diagnostic categories based on nomothetic response covariations can serve as a framework to guide the behavioral assessor, particularly during the initial stages of the analysis when the identification of target behaviors is the focus of the evaluation (First, Frances, Widiger, Pincus, & Davis, 1992; Scotti, Morris, McNeil, & Hawkins, 1996). In the absence of nomothetically derived guides, what does a behavioral assessor use to arrive at judgments as to where to begin the analysis?

3. Areas of agreement and directions for future exploration

3.1. *The need for a theoretically-based descriptive classification system to facilitate clinical science*

One point of agreement between ourselves and Bissett and Hayes is the need for a theoretically-based classification system to facilitate clinical science. Classification systems of behavior can provide a framework for testing hypotheses, serve as nomenclature or language base that defines the elements of a field or study, and provide a communication function that, among other functions, promotes research on clinical phenomena and assists in the compilation of statistical data and record keeping (Blashfield & Livesely, 1991; Millon, 1991; Nelson, 1987). We would further agree that DSM, with its emphasis on syndromal classification, diagnosis, and description, is limited in its ability to advance clinical science. Descriptive classification, although limited to the description of concepts and associated features, can be useful in the early stages that eventually lead to the development of more advanced systems of classification that provide explanations of concepts or allows for predictions from those concepts (Morey, 1991). Ultimately, however, a useful classification scheme is one that can be viewed in the context of theory, and potentially subjected to empirical validation and falsification (e.g., Cantwell & Rutter, 1994; Follette, 1997; Hempel, 1961; Schwartz & Wiggins, 1986). Bissett and Hayes argue for a classification system that is theory-based, and we concur. We recognize, however, that considerable foundation work remains to be done before such a goal is tenable. In the absence of a widely accepted alternative, DSM diagnostic categories represent a useful starting point for the description of some behavioral phenomena, including personality features that represent marked deviations from that which is typical. Given the nature of DSM, functional analysis and behavior theory may not inform subsequent modifications of this system of classification provided that it continues its atheoretical and descriptive design. However, if the state of clinical science exceeds the boundaries of its most advanced classification systems, then the science, in order for it to progress, requires the development of new systems that can incorporate new observations and insights.

3.2. *DSM requires a stronger empirical base*

Bissett and Hayes raise the problem of internal consistency of PD features and reference a couple of select citations. While it is true that the prototype nature of DSM PD classification allows for considerable within-group heterogeneity as group members only need to display a subset of possible symptoms to receive a diagnosis, the internal consistency of these features (as indexed by coefficient alpha) has been usually found to be within acceptable limits (e.g., Blais & Norman, 1997; Morey, 1988). Internal consistency of PD criterion sets in subsequent editions of DSM could be enhanced through the elimination of symptom features that are only weakly associated with other criteria within PD concepts. Reliance on statistical approaches that index item consistency (e.g., coefficient alpha) or reveal clusters of symptoms that nomothetically covary with themselves but not others (e.g., cluster analysis, factor analysis) could aid this purpose. However, the architects of DSM seem to have an aversion to developing classification schemes largely influenced through

statistical operations, as evident in a statement by Robert Spitzer (1979) cited in Morey et al. (1986): “no category has ever been added to a classification of mental disorders for clinical use that was first identified by a mathematical procedure designed to generate diagnostic categories”. As such, it is hard to find fault with Follette, Houts, and Hayes’ (1992) criticism of DSM’s “truth by committee” approach to identifying behavior sets that define syndromal disorders. However, the possibility exists for having a classification system based on nomothetic response covariations identified or supported by appropriate sampling and statistical methodologies.¹

3.3. Alternatives to DSM classification that more strongly emphasize behavioral principles are needed

There have been previous attempts at developing classification systems by behaviorally-oriented researchers (Adams, Doster, & Calhoun, 1977; Bandura, 1968; Cautela & Upper, 1973). Recently, Follette, Hayes, and colleagues have provided additional suggestions for classification alternatives more consistent with the behavioral assumptions and principles (Follette, 1997; Follette et al., 1992; Hayes & Follette, 1992; Hayes, Wilson, Gifford, Follette, & Strosahl, 1996; see also the special series introduced by Follette, 1996). For instance, Hayes and Follette (1992) offer some examples of alternative systems including empirically derived analytic systems, logical functional analytic systems, and diagnostic categories based on nomothetic functional analyses. We applaud these efforts.

4. A final note

We note with regret that our very capable commentators did not specifically address the challenging issue of the conceptualization and assessment of PDs in particular, the focus of our article. We believe that PDs present a special challenge to behavioral conceptualizations because many of the phrases included within the DSM definition of PDs seem antithetical to a behavioral framework.

We would have liked Staats to address whether or not, and how, his theory of normal personality development can also be applied to the development of PDs. Similarly, we would have liked Bissett and Hayes to speculate on how a nomothetic assessment scheme based on functional analyses might apply to personality-disordered behaviors in particular.

¹ Although we see the use of literature reviews, data reanalyses, and field trials to inform modifications of disorder criteria in DSM-IV (Widiger, Frances, Pincus, Davis, & First, 1991) as a step in the right direction, it is ultimately unsatisfactory as the concepts used as reference criteria for subsequent modifications were primarily those disorders defined in accordance with previous editions of DSM. Such an incestuous exercise only adds to the reification and circularity already inherent within the DSM system (Hickey, 1998), and makes it difficult for subsequent editions of DSM to substantially evolve beyond its predecessors.

5. Conclusion: the complementary nature of functional analysis and topographical delineation of personality disorders

Traditionally, behavior theory and assessment have focused on the identification of specific target behaviors for intervention, and in so doing, have largely dismissed the potential utility of nomothetic response covariations or constructs in the consideration of target behavior selection. However, personality disorders, like many clinical phenomena, present a challenge, as there are often several target behaviors to address. Diagnostic labels can suggest specific target behaviors (specific symptoms or criterion behaviors) that nomothetically covary.

DSM, as previously noted, is a descriptive classification system that does not address behaviors' function. Bissett and Hayes argue for a functionally derived nomothetic level of analysis, and in so doing, downplay the significance of topographical description. However, there is a significant amount of evidence from a variety of sources that indicates the presence of classes or clusters of behaviors which have a tendency to be interrelated both idiographically and nomothetically (e.g., Kazdin, 1982); however, behavioral assessment has generally only paid minimal attention to these observations, perhaps because of a historical tendency to emphasize function over topography and to focus on single target behaviors (Nelson, 1987). Perhaps for these reasons, theorists such as Staats (e.g., 1995, this issue) have argued that traditional radical behavioral theory and classical functional analysis cannot account for clusters of nomothetic response covariations (or personality constructs) given the emphasis on function over form and on single behaviors over covarying responses.

We have argued in this and in our initial article that functional and topographical delineation of conditions are not mutually exclusive but rather complementary. Consideration of behavioral topography in conjunction with behavior function not only has the potential to provide a framework for client description, inform target behavior selection, guide treatment selection, and suggest dependent measures to index therapeutic change, but also to promote theoretical explanation and prediction related to the specific form of behavior topography. Eventual goals might include the development of a behavioral alternative to DSM classification and the generation of a conceptual framework that specifies how sets of behaviors develop into covarying sets or clusters, the processes by which such clusters vary or remain stable across situations or time, and the therapeutic processes that result in pervasive behavior change (Kazdin, 1982). To this end, we thank our commentators for their pioneering work in these areas.

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