
Commentary

Psychological Processing of Criticism: Reaction to Ducey and van der Kolk

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Our critique of van der Kolk and Ducey's (1989) article pointed out a number of errors and shortcomings (Cohen and de Ruiter, 1991). The authors have responded to our critique (Ducey and van der Kolk, 1991). Our reply consists of three sections. First we will review Ducey and van der Kolk's response and focus on the validity of our original criticisms. Second we will review some of the new points raised in their response. Third we will show where Ducey and van der Kolk have distorted our critique.

KEY WORDS: Rorschach; PTSD; scholarship.

THE VALIDITY OF OUR ORIGINAL CRITICISMS

Ducey and van der Kolk agree that the findings with regard to form quality and the findings with regard to the elevated thought disorder index could not be evaluated because they did not provide the data nor the requisite statistics in their original article (see points 4 and 5 in our critique). Yet, the authors do not now present results. We do not understand why they therefore have not retracted their findings nor their interpretation of the findings.

Among other things, we noted that the sizes of van der Kolk and Ducey's samples were small and that considerable caution was needed in

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generalizing from the findings. Perhaps Ducey and van der Kolk agree with us. They write in their response that they believe their study “to be suggestive of further directions and future research” and that it was “exploratory and hypothesis-generating” (p. 426). The author did not mention this in their original paper, however. They provided no cautionary remarks about drawing conclusions.

We suggested that the authors had not defined what they meant by the term “Sum C.” (Contrary to what Ducey and van der Kolk *think* we meant, no innuendo was intended. We wished to define terms for the subsequent exposition.) As the authors correctly point out they did provide a definition of the term on p. 262 and it should have been seen by us.

In our critique we noted that the Comprehensive System is generally considered the empirically most advanced Rorschach system. In their reply Ducey and van der Kolk appear to agree and speak of “Exner’s monumental array of empirical studies that systematize and provide norms for the Rorschach technique” and credit us with having “point[ed] out to readers not up to date with Rorschach technique that important advances in that area have made the Rorschach a more workable tool for scientific investigation” (p. 431).

CRITICISMS TO POINTS RAISED BY DUCEY AND VAN DER KOLK

1. We criticized van der Kolk and Ducey for employing various Rorschach systems in an unsystematic fashion and for not presenting arguments for selecting one system in one place and another elsewhere. In passing the authors now indicate that they were working with “archival data” and that “latitude needs to be granted regarding its availability for research” (p. 426). We are puzzled by what Ducey and van der Kolk mean by “archival data.” In the *Method* section of their article they write that the “subjects were respondents in a survey of nightmare frequencies among Vietnam veterans in a Veterans Administration (VA) Outpatients Clinic.” They go on to say that 37 Vietnam combat veterans were interviewed and that 13 of these were selected for the study on the basis of “(1) the intensity of their recurrent, intrusive nightmares (. . .); (2) meeting DSM-III criteria for PTSD on clinical examination by two independent examiners; (3) not meeting criteria for other Axis I or II diagnoses . . .” (pp. 260-261). The details would indicate that the patients investigated had participated in a study. Should that be the case, it’s not clear what would be “archival” about the data. And if they are “archival” it ought to have been reported in the article.

If the data are “archival” we nonetheless fail to see how this relates to the points raised in our critique. We criticized the authors for the way in which they treated their data, we called several of the findings they reported into question and we took them to task for the manner in which they drew conclusions from the results they presented. We do not object to the *presence* of shortcomings; we maintain rather that they should be taken into account in the report of the findings and in the process of drawing conclusions. Their remark that “latitude needs to be granted regarding its availability for research” is to us an unclear remark: we just don’t know what they mean. Even “archival” data can be (re)scored using new methods.

2. Ducey and van der Kolk write that they offer a “thorough interpretation [of high *m*] in two distinct places (pp. 263, 268)” (p. 426). On page 263 of their article the authors provide a brief review of the interpretative significance of *m* in general. This was part of the *Method* section, however, where the Rorschach was described. It appeared *before* the results had been reported and cannot be considered interpretation of findings.

We notice faulty logic in the reasoning they employ in “interpreting” their finding in the *Discussion* section on page 268. Their study was designed to test the hypothesis that PTSD subjects suffered from “the experience of the pressure of ego-alien thought or fantasy” (p. 263). This would be reflected in the Rorschach in a high frequency of *m*. In other words, high *m* would be *evidence* of the pressure of ego-alien thought in PTSD subjects. In their interpretation on p. 268, however, the authors reverse the argument. They say that the present study “supports the empirical literature that the proliferation of these [*m*] responses suggest [*sic*] an imagistic but unsymbolized awareness of dangerous but overwhelming ‘ego-alien’ thought or fantasy beyond the individual’s control.” This line of reasoning requires that they *assume* that PTSD patients are characterized by these ego-alien thoughts and fantasies. In other words, the authors assume the idea they set out to verify.

3. There appear to us to be inconsistencies in the interpretation Ducey and van der Kolk provide of the color response (p. 428). On the one hand they say that the color response is “characteristically as passive and unbidden as the immediate experience of affect.” On the other hand the color response is referred to as “purposeful and ‘deliberately initiated’.” We do not understand in what way the same response category is to be regarded as both “passive and unbidden” and “deliberately initiated.”

4. The authors write that subsequent research has “borne out” their original findings (p. 426). However, on p. 429 they refer to findings of a study of 27 traumatized subjects by Levi (1990, 1991) which apparently

did *not* uphold the van der Kolk and Ducey finding of lowered *M*. In their original paper the finding of low *M* is considered critical evidence for the authors' theory of relative lack of symbolic processing in PTSD (van der Kolk and Ducey, 1989, p. 264). From the information provided by Ducey and van der Kolk (1991, p. 429) it appears that Levin's subjects did not differ from nonpatients with regard to *M*. Therefore, at least one study subsequent to van der Kolk and Ducey's provided findings inconsistent with the latter.

5. The authors (1991, p. 428) write that "symbolization, 'meaning-making' and 'working through' of trauma is based on emotional integration." They go on to say that the capacity for emotional integration "would be best measured on the Rorschach by *M*." This is not at all evident to us. There is evidence which can be interpreted as suggestive of the opposite. Exner's (1990) data, for example, disclose that the balance of *M* to *WSumC* in patient groups is greater than that for nonpatients ($N = 700$). On the average, schizophrenics ($N = 320$) show high levels of *M* and very low levels of *WSumC* and depressives ($N = 315$) and character disorder ($N = 180$) patients both show greater *M* than *WSumC* (though both are lower than the corresponding levels for nonpatients). Furthermore, higher levels of *M* than *WSumC* have also been noted in Rorschach studies with patient groups in the Netherlands [e.g., panic disorder patients (de Ruiter and Cohen, submitted), insomnia patients (Cohen, 1990) and gender identity disorder (Cohen *et al.*, 1991)]. Admittedly these data are not from studies about PTSD but they do suggest that psychopathology in general is associated with relatively higher rather than with relatively lower levels of *M*. If one assumes that the level of emotional integration is lower in the patient groups than in the nonpatient group then these findings do not rhyme with Ducey and van der Kolk's assertion.

DUCEY AND VAN DER KOLK'S DISTORTION OF OUR CRITIQUE

Ducey and van der Kolk express the fear that our critique could lead readers to the journal to "dismiss substantive findings" (p. 426). We do not see why this should be the case. In our critique we provide a systematic review of their results. At the conclusion of our critique we state plainly that "Van der Kolk and Ducey have made a scientific contribution" and we summarize their findings (p. 416).

The authors write of us that we "remain wedded to a narrow conception of *M* and *SumC*" (p. 428). In fact we happen to entertain several ideas con-

cerning *M* and *WSumC*, to none of which we are “married.” In our critique we referred to Exner’s (1986) empirical work and noted that both *M* and *WSumC* can be regarded as psychological resources which an individual can use to adapt to his environment. Subsequently we posed the question whether, therefore, not both affective and ideational resources could be used to process traumatic experience. Furthermore, it is not clear what is so “narrow” about the conception to which we refer in our critique. Ducey and van der Kolk state that we propose that the blood and anatomy contents “*should have been scored for An, MOR, and DR*” (p. 430, italics added). In our critique we clearly stated that “*we would have liked to see the authors code these responses according to an established method*” (p. 412, italics added). We noted that such quantitative treatment of the data would have permitted comparison with other data, and could have been interpreted accordingly. The authors write that we “ignored” the importance of idiographic interpretation of the projective material. We *never* dismissed the value of qualitative analyses nor is that our intention. But, when Ducey and van der Kolk report on qualitative analyses of their protocols we believe they are responsible for providing a mechanism by means of which their findings can be checked.

Finally, we would like to make a few comments in general. In their concluding paragraph the authors seem to imply that we were “erect[ing] obstacles and establish[ing] walls and competing camps of knowledge” (p. 431). Quite to the contrary, we were pointing out how Ducey and van der Kolk’s study could have benefited from improvements in methodology and reasoning.

Ducey and van der Kolk accuse us of “failing to introduce new empirical data on the subject” (p. 426). They go on to write that this “called to mind the old Dutch expression, ‘The bests sailors stay on shore.’” They imply, therefore, that one is not entitled to review a study on a subject unless one has conducted research in that area. Only a very small portion of the scientific community would therefore be entitled to criticize other researchers’ work, i.e., only those scholars who have conducted highly similar research. We believe neither the advancement of psychological science nor the application of scientific insights to clinical practice would benefit from such a point of view.

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