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Letters to the Editor

London, Ont., 18 April 1980

re: On the utility of signal detection theory pain measures

Dear Editor:

Two recent publications in *Pain* [16,32] have addressed themselves to the suitability of signal detection theory (SDT) procedures for pain research. It is gratifying to see that there is considerable interest in the issues which I raised in previous theoretical [24,25] and empirical [26,27] papers. Some response to the material and manner of the two articles seems, however, in order.

Jones [16] concurs with me that most previous studies of signal detection theory and pain have erroneously used discrimination measures to draw conclusions about the mechanisms underlying pain attenuation, since multiple interpretations of the outcomes can be derived. We differ in emphasis. Jones, for instance, chooses to amplify a point raised only briefly in my paper: in fact, one should use maximum-likelihood estimates of the best fitting linear ROC function [23], although, in many instances, a least-squares procedure might provide a close approximation. Likewise, he dwells on aspects of signal detection theory methodology which I deliberately omitted from the earlier paper, recognizing that it was meant to be an introduction to SDT and not a complete exposition.

We appear to have some remaining areas of disagreement, although many of them are due to Jones' selective interpretation of matters which could have been readily explicated and clarified in a very brief discussion. When I spoke of "threshold in the traditional sense" I was referring to a fixed neural barrier, always surpassed by an adequate signal and never surpassed by a weaker one, whereas Jones refers to the statistically defined threshold which accompanies the normal ogive. My notion of "properly calculating" d′ meant calculating it in a manner consistent with the requirements imposed by the nature of the underlying distributions, while Jones believes "properly" means that one has adequately gone through the series of steps required under the most simple assumption. I wonder whether the presentation of such quibbles provides any useful information for the readers of this journal. In the latter instance, it masks the important point that the simple assumption is often unjustified.

Jones wishes that I had come down harder on earlier studies for using pain responses rather than signal likelihood. Clark and Mehl [2] and I have studied aspects of this problem. Since they partition responses based upon different stimulus events, both measures provide a determination of discrim-

inability between adjacent stimulus levels, though possibly not equivalent ones [28]. The distinction between a likelihood ratio axis and one labelled "sensation continuum" (or, elsewhere [30], "sensory effect") is theoretically important, but the "invalidity of this equivalence" is not the basic point, since my criticisms apply in either event.

Jones may err in his analysis of the data in my empirical report [27]. He suggests that the area under the ROC function for the lower intensity pair of signals is greater than that under the curve for the stronger pair. Inspection of Table I in that paper, based upon individual linear functions for each pair in each session, shows that the discrimination indices were not significantly different.

My statements about the interpretations used by SDT advocates in the area of pain research, contrary to Jones' assertions, were not meant to limit the interpretations which could be offered by a signal detection researcher or, for that matter, to question the general validity of the SDT model. We do, however, differ in our optimism about the ultimate practical success of such a model, however expressed. As indicated earlier [25], "a change in d' is neither a necessary nor a sufficient indication that a treatment has any analgesic properties." Although SDT procedures may tell us "which analgesics do affect discrimination and which do not" [16], we are still left with fundamental questions as to the pain relieving properties of any putative analgesic, its mechanism(s), and the generality of interpretation from one treatment to the next. That is where the whole issue began.

While Jones states that "the main value of Rollman's critique lies in the convincing demonstration that questions about the discriminability of two ordinarily painful stimuli and questions about their perceived painfulness per se are logically independent", Velden [32] takes a diametrically opposed position by asserting that "measuring pain by measuring and cumulating the discriminability of adjacent stimuli, as is being done by most SDT pain researchers" is completely plausible.

Velden rightly notes that measuring sensations through discrimination is an "old tradition" in psychology. It is also one which has been generally rejected by contemporary psychophysicists. The view which he advocates, Fechner's proposition that "the magnitude of a sensation elicited by a stimulus is equal to the number of just noticeable differences (JNDs) above threshold up to the intensity of the stimulus" does not provide a valid measure of sensation on prothetic ("how much") continua [5,29] and the resulting dol scale [14] has been abandoned because of its limited validity.

Some of the problems involved in using summated discrimination steps to measure sensations are described by Dember and Warm [4]. They note that limitations to the generality of Weber's law of discrimination indicate that "Fechner's summated JND technique may introduce serious error into the form of the psychophysical relation between stimulus magnitude and subjective magnitude" and that the assumption that "all JNDs are subjectively equal to each other, and therefore that each JND contributes an equal increment to perceived magnitude, is contraindicated by empirical evidence."

Thurstone's work [e.g., 31], which Velden also cites, is a later approach to indirect scaling which makes different use of the discriminative capacity of the subject as a basis for measurement. Categorical judgments allow one to derive estimates of psychological distance which are analogous to the d' measure of signal detection theory, the modern counterpart of the Thurstonian model [30]. To say that frequently "confused" stimuli are psychologically similar provides a means for arranging a series of stimuli, but the resulting information does not overcome the problems I cited in my analysis of the SDT approach, since alterations in pain and alterations in discrimination are not demonstrably equivalent events.

Velden also provides a discourse on the differences between the term "sensory" as frequently used by SDT and by the gate control theory [21,22]. My own challenge to SDT in the measurement of pain did not arise from a confusion of these meanings. Velden's proposition that noxious stimuli can be discriminated on an emotional-affective scale as readily as on a sensory-discriminative one describes an approach which has been used for several years in my own laboratory as well as other ones [e.g., 6,9—11].

There is a danger in these debates of losing sight of the need to develop and validate scales of pain which reflect subjective experiences and yield evidence of the factors responsible for alterations in verbal reports. Signal detection theory provides both an experimental methodology and a framework for interpreting the resulting outcomes. My principal criticism was not of the former; both the papers I reviewed [25] as well as the more recent literature [e.g., 1,7,12,17,20] include studies which seem to have found reliable changes in d' or criterion (or both) as a consequence of some manipulation. The internal consistency suggests that the change in SDT parameters is one which is meaningful *for that experiment.*

But what is its meaning? Does the outcome tell us how the treatment acts? Can we utilize the results to compare the efficacy and mechanisms of potential analgesics? The difficulty lies in the fact that an analgesic may increase discrimination, decrease discrimination, or leave discrimination capacity unchanged. An analgesic may shift the criterion; a change in response bias may do the same. As stated earlier [25], "the conclusion of some individual studies may be correct, but they are not inevitably so."

Related concerns about the applicability of SDT in the study of pain have been expressed by others [e.g., 3,8,13,15,18,19,33]. The proponents of SDT must go beyond arguing about contentious interpretations and demonstrate that changes in d' or criterion provide unequivocal evidence about the role of "sensory" and "response bias" effects in the action of any potentially analgesic treatment.

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