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Clinical Wisdom and Evidence-Based Medicine Are (Indeed) Complementary: A Reply to Bursztajn and Colleagues

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ABSTRACT

We briefly respond to Bursztajn and colleagues' commentary[1] on our article, "Clinical Wisdom in Psychoanalysis and Psychodynamic Psychotherapy: A Philosophical and Qualitative Analysis."[2]

We thank Bursztajn and colleagues for their detailed commentary that raises several important points related to qualitative research methodology, medical epistemology, and clinical education. In preparing their commentary, they have given us much to consider; we are appreciative of the opportunity to revisit several of the fundamental questions that moved us to collaborate on our article in the first place. Grateful as we are, we believe their critique is flawed. Most significantly, they attack a straw man when they claim we propose and advocate for a dichotomy between the kinds of knowledge provided by evidence-based medicine (EBM) and practice based in clinical wisdom. Second, their critique of our methods is reasonable, but stems from our decision to condense details regarding qualitative methods in favor of emphasizing conclusions and raising broader questions. Their last critique is that we advocate for pedagogical strategies that separate EBM and virtue-based practice; this is an extension of the false dichotomy claim and is similarly flimsy.

To be clear: we do not suggest a bright line exists—nor that it should it exist—between clinical practice or teaching that is evidence based and models that recognize the importance of clinical wisdom. We conceptualize clinical wisdom as a virtue that enables clinicians to understand, synthesize, and act on knowledge from a variety of sources. On this point, it seems we agree with Bursztajn and colleagues more than they recognize. Nonetheless, in this reply we will address each of their critiques briefly.

CLARIFICATION OF METHODS

To differentiate the "wise" clinician, we used findings from dynamic integration theory research, which anchors wisdom in affect regulation and posits that regulation is made up of two independent emotion-regulation strategies: affect optimization (gravitation toward positive emotions) and cognitive-affect complexity (the search for differentiation and objectivity).[3] Older individuals' affect regulation tends to improve because the balance of positive over negative affect increases into old age.[4]

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Research by Labouvie-Vief and Medler suggests that for those who achieve wisdom in old age, optimal functioning involves an integration and flexible coordination of the two core emotion-regulation strategies. They found that individuals grouped into four categories: (1) dysregulated (low complexity, low optimization), (2) self-protective (low complexity, high optimization), (3) complex (high complexity, low optimization), and (4) integrated/wise (high complexity, high optimization).[5] Individuals who were in the fourth group, "integrated/wise," scored high on conceptual complexity, personal growth, and empathy. They possessed high positive and low negative affect, high well-being, and low depression. They described themselves in terms of a moderate number of positive/negative blends, suggesting that they are able to acknowledge negative affect, but integrate it into an overall positive picture.

Since we adapted our recruitment design from the findings of Labouvie-Vief and colleagues, project participants were persons with high affect optimization and cognitive affective complexity who displayed complex thinking about emotions in terms of emotional blends and contrasts and transformations across time and context. Inclusion criteria stipulated that clinicians must be at least 72 years of age and in practice 40 or more years. Nominations were sought through announcements in major psychology and psychoanalytic periodicals and by word of mouth.[6]

Nominees were contacted and a brief phone discussion was conducted, along with dialogue via e-mail. Once an interviewee agreed to participate, a full interview was scheduled. In one instance, a face-to-face interview was not possible, and a telephone interview was conducted. The interview technique followed Cartwright's model for psychoanalytic research, designed to examine interviewees' understandings of concepts through analysis of personal narratives.[7] All subjects were asked the same core questions and were given a list of potential topics.

Interviews were transcribed and statements were coded. "Coding" here refers to the identification and categorization into coherent clusters of key words, concepts, or narrative constructs (metaphors) commonly invoked by interview participants.[8] The code categories presented were not *a priori*, but emerged after careful review of all interviews. We originally considered providing an appendix that outlined the interview questions and topics, but in consideration of space, we did not include them. We concluded that interested researchers would contact us, and we welcome such inquiries.

We hope this clarification helps allay some of the authors' methodological concerns. However, we sense a the deeper problem lies in skepticism about the value of qualitative research more generally. Such skepticism is not uncommon, and outright animosity toward narrative-based qualitative research is often expressed in claims it is too soft, unsound, unreliable, and unverifiable. The authors seem deeply dissatisfied that we were unable (by design) to generate statistical results to "prove" anything. Such is the dilemma often encountered by qualitative researchers, and we are unsure about how to satisfy such skepticism.

ALLEGATION OF FALSE DICHOTOMY

Bursztajn and colleagues spend most of their commentary developing and defending a claim we are pushing a false dichotomy between EBM and clinical wisdom. They claim we have inappropriately bifurcated two kinds of knowledge that are apparently impossible to disentangle, and thus we are guilty of fallacious reasoning. They write that our article "establishes a sharp dichotomy between scientific, evidence-based medicine . . . and clinical wisdom," and they indict us for basing this dichotomy on a conflation of individual cases and population studies (a fallacy of division) and for seemingly universally damning EBM. These claims are inaccurate. Nowhere did we argue that EBM and clinical wisdom are mutually exclusive. It is true we do use certain tokens and descriptors ("technical," "formulaic," "objective") to circumscribe the concept of EBM. It is also true that these are contrasted against subjective qualities of clinical wisdom, and particular qualities come into sharp relief. However, pointing out the uniqueness of two concepts need not lead to a conclusion that the concepts are mutually exclusive. To be clear, we explicitly acknowledge the

complementarity of technical knowledge—an essential aspect of clinical judgment—and wisdom. One section of our article bears repeating:

> We should note that a distinction is often drawn between clinical judgment on the one hand and practical wisdom on the other. In the case of the former, the clinician is able to make sound decisions about a medical problem using the technical tools of medical and scientific decision making. The integrated nature of clinical wisdom, however, encompasses both clinical judgment and practical wisdom that is gained through decades of practice. Both concepts have as their aim an idea of what is best for the patient. Indeed, as Pellegrino contends, a key element of clinical judgment includes answering the normative question, "What should be done?" This question requires practical wisdom—thus the virtue of clinical wisdom is a fully integrated form of clinical judgment combined with practical wisdom.

We do claim that formulaic methods of psychotherapy are often overly relied upon and allude to possible reasons for such overuse. But such a claim need not entail that there is no place for highly structured, well-tested, and reliable forms of psychotherapy.

Moreover, nowhere do we endorse a premature retreat from attempting to measure and systematically study subjective states. It would be grossly naïve to either think that EBM is useless because of its limitations or to criticize it for being unable to answer normative questions (that is, issues related to patients' preferences). Bursztajn and colleagues recognize that certain clinically relevant questions lie beyond the scope of EBM, and warn that we must not rely on it for such answers. This is precisely the point we are making. Further, the authors reiterate something that we would have uttered: that EBM guidelines are "not the be-all and end-all of clinical practice." It is in both recognizing this fact and navigating complex normative questions that clinical wisdom plays a critical role. Thus, it is their logic that is fallacious. By attributing to our argument a nonexistent false dichotomy, they attack a straw man.

EDUCATION

The authors attack our rather modest proposals for elevating the conversation and pedagogy around clinical wisdom. In so doing, they attribute to us a false dichotomy, alleging that we endorse a move away from teaching science and evidence toward haphazardly teaching anecdote. We do not. In a confusing facet of their rejection of our proposals, the authors seem to contradict themselves by welcoming the contributions of experienced practitioners ("We are all in favor of those with more knowledge teaching those with less. . . .") while citing empirical evidence that indicates experience may, at best, not be helpful, and may even be harmful. The point that clinicians often develop bad clinical habits that fly in the face of evidence is true enough, and we would hope such practitioners would not be held up as exemplars. Decades of practice are not sufficient for the development of clinical wisdom worthy of placement on the pedagogical stage. As we note at the close of our article, when clinicians have practiced for years and think they are wise or ethically adept, their confidence may be misplaced.[9]

Finally, to be clear: we are not suggesting teaching rule breaking, but rather that trainees question the assumptions of their day. Those interviewed during the Wisdom Project, in reflecting on their own careers, described such radical curiosity. Do the authors really think that these clinicians flagrantly broke rules willy-nilly? On the contrary, the clinicians in our study questioned prevailing paradigms to build new research programs and clinical models. For example, one interviewee, who saw schizophrenic children at a time when mothers were generally blamed for their child's illness ("schizophrenigenic mothers"), took her observations to the laboratory and began to do research in childhood schizophrenia, which evolved into an interest in family therapy and new treatment techniques.[10] Another clinician helped develop observational research techniques, which led to findings in separation-individuation research that have been applied to the treatment of severely disturbed children;[11] this clinician was also instrumental in creating a doctoral training program for the treatment of inner city chil-

dren. A clinician who treated and studied patients with severe anxiety disorders ultimately pioneered new research on annihilation anxiety and developed now-standard assessment tools.[12] The clinical observations of a husband and wife led them to turn away from traditional theories and modes of practice and introduce a whole generation of clinicians to a new way of understanding the human psyche and how to treat it.[13] One psychologist who treated psychotic patients rigorously studied changes over time and questioned methodological issues in research.[14] A psychiatrist, who was supervised to believe his patient was a hysteric, saw shame as central in the patient's difficulties, and went on to a prolific career writing and thinking about shame.[15] These professional thumbnails clarify our point: it is essential to inculcate thoughtful and wise questioning as part of the armamentarium of the treating clinician and psychotherapy researcher.

CONCLUSION

To conclude: the authors falsely accuse us of accepting conceptions of wisdom that are based in "authority, mystery, and magic." The purpose of the Wisdom Project and our article was to begin to try to articulate what constitutes clinical wisdom in psychotherapy and bring some clarity to the concept. We hope that our article, the commentary by Bursztajn and colleagues, and this brief reply continue to stimulate needed conversation and debate about clinical wisdom in psychodynamic psychotherapy, psychiatry, and medicine more generally.

NOTES

1. J. De Freitas, O.S. Haque, A.A. Gopal, and H.J. Bursztajn, "Response: Clinical Wisdom and Evidence-Based Medicine Are Complementary," in this issue of *JCE*.

2. C. Baum-Baicker and D.A. Sisti, "Clinical Wisdom in Psychoanalysis and Psychodynamic Psychotherapy: A Philosophical and Qualitative Analysis," in this issue of *JCE*.

3. G. Labouvie-Vief, "Dynamic integration theory: Emotion, cognition, and equilibrium in later life," in *Handbook of Theories of Aging*, ed. F. Bengston, M. Silverstein, N. Putney, and D. Gans (New York: Springer, 2008): 277-93.

4. G. Labouvie-Vief and M. Medler, "Affect optimization and affect complexity: Modes and styles of regulation in adulthood," *Psychology and Aging* 17, no. 4 (2002): 571-88.

5. Labouvie-Vief, see note 4 above.

6. Announcements appeared in *Psychologist-Psychoanalyst* and *Monitor* of the American Psychological Association.

7. D. Cartwright, "The psychoanalytic research interview: Preliminary suggestions," *Journal of the American Psychoanalytic Association* 52 (2004): 209-42.

8. C. Auerbach and L.B. Silverstein, *Qualitative Data: An Introduction to Coding and Analysis* (New York: New York Press, 2003).

9. D.P. Sulmasy, M. Dweyer, and E. Marx, "Knowledge, confidence, and attitudes regarding medical ethics: How do faculty and housestaff compare?" *Academic Medicine* 70 (1995): 1038-40.

10. W. Goldfarb, "The mutual impact of mother and child in childhood schizophrenia," *American Journal of Orthopsychiatry* 31, no. 4 (1961): 738-47; N. Fieldsteel, "The Uses of Family Treatment in the Understanding of Narcissistic Personality Disorders," *Group* 3, no. 2 (1979): 88-95.

11. M. Mahler, F. Pine, and A. Bergman, *The psychological birth of the infant: symbiosis and individuation* (New York: Basic Books, 1975).

12. P. Beneviste, N. Papouchis, A. Rhiannon, and M. Hurvich, "Rorschach assessment of annihilation anxiety and ego functioning," *Psychoanalytic Psychology* 15, no. 4 (1998): 536-66; R. Levin and M. Hurvich, "Nighmares and annihilation anxiety," *Psychoanalytic Psychology* 12, no. 2 (1995): 247-58; M. Hurvich, R. Allen, and H. Mcguire, *Manual for the Hurvich Experience Inventory*, forthcoming.

13. P.H. Ornstein, "The Elusive Concept of the Psychoanalytic Process," *Journal of the American Psychoanalytic Association* 52 (1974):15-41; A. Ornstein and P.H. Ornstein, "Speaking in the Interpretive Mode and Feeling Understood; Crucial Aspects of the Therapeutic Action in Psychoanalysis, in *Understanding Therapeutic Action in Psychoanalysis* (Hillsdale, N.J.: Analytic Press, 1996).

14. B.P. Karon and P. O'Grady, "Intellectual test changes in schizophrenic patients in the first six months of treatment," *Psychotherapy: Theory, Research, and Practice* 6 (1969): 88-96; B.P. Karon and G.R. VandenBos, "Issues in current research on psychotherapy vs. medication for schizophrenics: Methodological issues in research," *Psychotherapy: Theory, Research and Practice* 12 (1975): 143-8.

15. L. Wurmser, *The Mask of Shame* (Lanham, Md.: Jason Aronson, 1975).