my writing as a kind of knowledge other than scientific knowledge—a knowledge that is augmentative rather than reductive, that honors and invites the aesthetic, sensuous, compassionate, and visionary possibilities of experience as well as the rational and technical. The term is not meant to exclude scientific knowledge, but to embrace it within the program Abraham Maslow called "hierarchical integration" (*1*). It appears that Corrington speaks for those who continue to dichotomize the spectrum of human experience, and then to reject and repress the half they consider to be "discredited." ("Discredited" by what cultural agency, I wonder.) My position is that every such dichotomy is a symptom of disease in us: of fear, rigidity, compulsive distrust. The dichotomy is the problem, and choosing one or the other half of it is no solution, because no healthy personality, no healthy culture, no healthy science can be built on less than the full and integrated range of human mentality.

My controversy is with those (and they are many, in the behavioral as well as the natural sciences) who contend that *only* science is a valid way of "knowing" the nature of things. No matter that they are otherwise Renaissance men who allow art, poetry, flower arranging, yoga to coexist in their lives as entertainments; to hold that science monopolizes our knowledge of nature is to censor experience. If nature invites a compassionate and visionary response from us, the only intellectually honest thing to do is to give it, and trust the experience to carry us where it will. To do otherwise is to lie.

2) With Barash, I fully recognize that science begins in wonder and may well take off into towering flights of imagination—especially at its unexplored theoretical margins. It should be clear by the many examples I have offered in my writing that my argument is with *reductionist* science: science whose intention or effect is to disenchant and desacralize. Barash is being less than candid if he is not as troubled as I by how virulent, widespread, and persistent this strange vice of the scientific mind has proved to be.

Again to define the controversy: my contention is that reductionism has been a central and integral part of the scientific tradition since its inception in the 17th century; that science has provided a peculiarly fertile medium for its proliferation; that it distorts our understanding of man, society, and nature more today than ever before; and that the scientific community remains as oddly impotent to purge the vice now as in the past. In short, I do not think reductionism is a skin blemish of science, but the blood poisoning of the profession. This is not a failing that can be offset by listing all the nonreductionist science one can think of. Of course there is plenty. I accept that as being so. But nonetheless—and even more disconcertingly—reductionism continues to command full scientific status. This, I think, will continue to be so until scientists reflect deeply upon the psychology of objectivity and its proper place in our total experience of nature.

My writing has been an effort to trace reductionism back to its historical and psychological roots, to delineate its several and changing varieties, and to diagnose the effect that the many converging strands of reductionist research have had upon our culture at large—not least of all upon the scientists's own sense of social responsibility. My conclusion (in brief) has been that reductionism results from imposing the objective mode of consciousness upon a personality that has segregated and repressed its sensuous, visionary, and compassionate powers: the peculiar personality type produced by the Judeo-Christian religious sensibility.

This does not deny for a moment that there is a place for scientific objectivity in our psychological repertory. The most succinct formulation I have offered of that place is perhaps on pages 374–378 of the Anchor edition of *Where the Wasteland Ends* (*2*). It is impossible for me to see that what I have sugested there deserves to be called "anti-scientific" or "anti-rational." If, however, that formulation is held to be an "attack" upon science, so be it. But it is intended as a therapist's attack upon a neurotic complex that profoundly flaws the epic grandeur and humane potentialities of science.

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References and Notes

1. A. Maslow, *Psychology of Science* (Harper & Row, New York, 1966).
2. T. Rosak, *Where the Wasteland Ends* (Doubleday, New York, 1973).

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Clarification

I wish to correct, and to apologize for an unfortunate slip in my review (30 Aug. 1974, p. 777) of Lewis Feuer's *Einstein and the Generations of Science* (*1*). The phrase regarding Engelbert Broda (p. 778), which in any case is far too strong, was of course not intended to apply to Broda personally, but to his book on Boltzmann (*2*), and not to the accuracy of the citations and quotations in the book, but to its characterization of fin-de-siècle physics, particularly of the battle between Boltzmann and the antiatomists.

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References

1. L. S. Feuer, *Einstein and the Generations of Science* (Basic Books, New York, 1974).
2. E. Broda, *Ludwig Boltzmann* (Deutscher Verlag der Wissenschaften, Berlin, 1957).

Accurate References

Duncan Blanchard (Letters, 20 Sept. 1974, p. 1003) clearly illustrates the danger of relying on the accuracy of references cited in papers and the necessity for checking references personally before using them. Blanchard points out that a particular paper published in *Tellus* was cited in six different papers in the 21 September 1972 issue of the *Journal of Geophysical Research.* Four of the six citations to the *Tellus* paper were incorrect in some way. Blanchard's comments and concerns are especially relevant for the scientific community in this era of the quick copy and publication explosion. His letter struck home particularly hard in our laboratory, since three of the four erroneous citations were in papers from my group. Errors of this type are inexcusable and undoubtedly occur all too frequently. However, it is sometimes difficult to avoid them, even when one has the best of intentions. In Blanchard's letter, he refers twice to the 21 September 1972 issue of the *Journal of Geophysical Research.* The correct date was 20 September 1972. Thurber's moral ("There is no safety in numbers, or in anything else"), cited by Blanchard, is certainly well taken.

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