tacts existed between Romantic poets and their scientific contemporaries? Can it really be true that "the real scientists of the last two hundred years . . . took no notice of Romantic theory" (24)? Must the reader supply all these details? Eichner's wide-ranging notes show that he commands the primary and secondary sources necessary for the kind of study I am suggesting here. If he is now at work on the long book he mentions (8), I hope he considers these questions.

MICHAEL S. KEARNS
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To the Editor:

In a time when our collective critical effort seems ever more opaque and arcane—the unreadable in pursuit of the impenetrable—it is refreshing to come across an essay on an important topic presented lucidly point by point and coherently overall. Hans Eichner is to be congratulated for his enterprise as well as for the grace of his prose.

I must dissent, however, from Eichner's view that Romanticism and science are incompatible. To be sure, I agree that in large measure Romanticism involves a "revolt against mechanism" (17). But this is not to say that the Romantics therefore were all idealists or that they were fundamentally opposed to science as science. My contention, at least, is that the great English Romantic poets (Eichner draws his examples primarily from Continental authors) were neither antiscience nor antiscientific. Deeply concerned with and knowledgeable about the science of their day, they sought only to correct an epistemological error that they saw as having marred science from its inception—that knowledge derives solely from the object. Empiricists operating within the English empirical tradition (a tradition ultimately at odds with the simple rationalism of the Enlightenment), the English Romantics did not endeavor to reverse the epistemological model of science by replacing the object with the subject; rather, recognizing that science too is a product of imaginative activity, they sought a complex synthesis reflective of our experience of the world between outer and inner, object and subject, classical science and the imagination. Thus, in "Tintern Abbey," for instance, Wordsworth speaks of the mind as half creating and half perceiving; and Coleridge, in his *Theory of Life*, adds to the concept of "outness" that of "inness" but does not attempt to replace the former with the latter. I might add that, at the very time when geologists were distorting their findings into evidence of creation, the English Romantics broke with tradition and developed an evolutionary view of life (see, e.g., Marilyn Gaull, "From Wordsworth to Darwin," *Wordsworth Circle* 10[1979]:33–48). In some ways, then, as I argue more fully in my "Science and Romanticism" (*Georgia Review* 34 [1980]:55–80), the English Romantics were better scientists than many of their counterparts in science. At any rate, Hayden Carruth, speaking of British poets generally, has recently put the matter categorically: "I cannot recall a single serious writer from the time of Francis Bacon to the present who has rejected science or scientific thought. Shakespeare, Milton, Pope, Wordsworth, etc.—all incorporate the general scientific knowledge of their time in their work; it's there on the page" ("A Few Thoughts . . . ," *Georgia Review* 35[1981]:735). Speaking of the English Romantics specifically, Walter Jackson Bate amplifies: "English Romantic thought . . . was . . . naturalistic in its direction rather than frankly subjectivistic; for the intuitional empiricism upon which it relied was tempted to concentrate on the particular, and upon the revelation of its essential nature *as* a particular. This concentration had . . . an almost scientific direction" (*From Classic to Romantic* [1941; rpt. New York: Harper, 1961], 181–82).

I must also dissent from Eichner's view of the history of science and his reading of the posture of contemporary science. Arguing against Thomas Kuhn et al. Eichner seems to take science as ahistorical and, with respect to its fundamental outlook, as unchanging and unchanged. But the changes in scientific outlook (between the nineteenth and twentieth centuries) that we now clearly perceive show that science is not ahistorical. Except for B. F. Skinner perhaps, what scientist today would assent to Robert Monro's statement, made in 1893 before the British Association for the Advancement of Science, that "imagination, conceptions, idealizations, the moral faculties . . . may be compared to parasites that live at the expense of their neighbors" (quoted in Lewis Mumford, *The Pentagon of Power* [New York: Harcourt, 1970], 60). No, even if not fully understood by technicians or by the average scientist practicing what Kuhn calls "normal science," a revolution in science has taken place, especially at the highest level (i.e., most theoretical). The basis of that revolution is summarized by Werner Heisenberg (I, too, quote directly from Heisenberg):

> When we speak of the picture of nature in the exact science of our age, we do not mean a picture of nature so much as *a picture of our relationship with nature.* The old division of the world between object and subject—in other words, the Cartesian distinction between

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res cogitans and *res extensa*—is no longer suitable for a point of departure for the understanding of the modern natural sciences. In the field of vision of natural science, above all stands the network of connections upon which we as living creatures are dependent and which at the same time we as human beings make an object of our thinking and our acting. The scientist no longer confronts nature as an objective observer, but sees himself as an actor in this interplay between man and the natural world. (*Das Naturbild der heutigen Physik* [Hamburg: Rowohlt Taschenbuch Verlag, 1955], 21)

That description sounds much closer to Romantic organicism than it does to any shade of nineteenth-century positivism. The very thing that positivism is not is relational. And surely, the one thing that positivism, in its quest for the absolute, cannot tolerate is uncertainty, however meliorated by the principle of statistical probability.

Both science and Romanticism, of course, are complex—too complex to be characterized as simply incompatible. Since, moreover, the artistic temper of our period remains essentially romantic, such a characterization severs the "two cultures" even further and consequently confirms what many already believe—that the humanities are irrelevant. But the directions of theoretical physics and the ascendance of biology and cognitive psychology in our day, it seems to me, point to ways of bridging the gap. Such is the task that I believe we need to pursue.

EDWARD PROFFITT
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To the Editor:

Thank you for the mischievous Eichner essay. I enjoyed it as I haven't enjoyed a *PMLA* article in years.

But isn't it a rather serious omission for an article on that topic published at this late date and in the profession's leading journal to ignore Richard Rorty's refinement of Kuhn and analysis of the Western epistemological tradition (*Philosophy and the Mirror of Nature* [Princeton: Princeton Univ. Press, 1979])?

Eichner lets himself off awfully easy with Laudan. To have followed through to Rorty would have given his rather complacent conclusion a good deal more bite and made the whole essay even more helpfully mischievous.

KENNETH A. BRUFFEE
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Mr. Eichner replies:

I am afraid I have the impression that Michael Kearns does not see very clearly what I was trying to do in my article. I was not concerned with repeating once again that the Romantics replaced the mechanical philosophy by organicism and that they strove to overthrow the epistemological convictions and habits of mind of the seventeenth and eighteenth centuries. I was concerned with showing that the epistemological convictions and habits of mind that form an essential part of the story of modern science led to serious problems, that some of the most astute and courageous thinkers of the seventeenth and eighteenth centuries tried to solve these problems and failed, that the Romantics were therefore driven to search for even more radical solutions, and that the solutions they found are incompatible with good science. In order to present a coherent case, I could not avoid occasionally saying the obvious, but I tried to say it as briefly as possible. As for the method I employed, it seemed to me, and still seems to me, appropriate. I was trying to contribute to the history of ideas, and hence I don't see what is wrong with my saying that Geulinx and Malebranche "must have been compelled" to formulate their philosophy by their need to escape the Cartesian impasse: I was simply showing that there was a serious problem staring them in the face and that their need to solve it led them to ingenious but rather desperate stratagems. And I was trying to show this, and whatever else I was trying to show, in an *article*. If I had done what Kearns thinks I should have done, that is, added "*at least* one or two case studies of particular Romantics confronting the mechanical philosophy, responding *in detail* to its implications" (emphasis mine), I would have filled up the whole issue of *PMLA*. Kearns also complains that I create the impression that I was writing not a history of individual minds but a "story of essentially one mind, named at various times Descartes, Kant, Fichte, and so on," but this complaint merely suggests to me that I succeeded to some modest degree in telling a coherent story, and I hope I did so without falling prey to the errors of *Geistesgeschichte*. In any event, there are dozens of case studies that I could refer Kearns to. On reading them all, one gradually begins to lose sight of the wood for the trees. I was trying to paint the wood, and in pointing out, to Kearns's annoyance, that I did so with a wide brush, I for once really did no more than state the obvious.

Edward Proffitt of course understands perfectly what I was trying to do. Before replying to his