

Forum

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Modern Science and the Genesis of Romanticism

To the Editor:

Hans Eichner begins the last section of "The Rise of Modern Science and the Genesis of Romanticism" [*PMLA* 97(1982):8–30] by reminding his audience of what he accomplished in the first section: isolating "some of the implications of the mechanical philosophy that made it unacceptable to the Romantics and that prompted them to branch out on new paths" (24). Were a reader to begin here, she or he might justifiably assume that the first section dealt at length with two phenomena, first, the unacceptability, to the Romantics, of the mechanical philosophy and, second, the Romantics' decision to take new paths. A reader might assume, further, that the article had presented at least one or two case studies of particular Romantics confronting the mechanical philosophy, responding in detail to its implications, and working out those new directions. These assumptions would be mistaken. As Eichner states at the beginning of his second section, "the preceding pages somewhat oversimplify and idealize both the Romantic rebellion and the system of thought it attempted to overthrow" (21). He continues, "Most of the oversimplifications in my presentation must be left to the reader to correct. . . ."

Now, as a reader I usually do not mind being asked to think for myself (although I would never advise my students to rely on their readers to supply crucial information), but I resent being asked to do so when the article has spent so much time elaborating the obvious. It is nothing new that "the basic epistemological convictions and mental habits of the seventeenth and eighteenth centuries . . . were overthrown" and that "organicism replaced the mechanical philosophy" (18). What I would like to know is just how these changes came about. I read that the Romantics did not reject "the *findings* of modern science" or "science *as such*" (17); what I look for, in vain, is a substantial discussion of Romantic commentary on either "modern science" or "the rise of modern science." Instead of a history of

individual minds working out solutions to individual problems, Eichner presents a story of essentially one mind, named at various times Descartes, Kant, Fichte, and so on, grappling with the "heuristic assumption that scientists *had to make* if they were to achieve any progress" and with the "horrible and absurd" implications of that assumption (12; my emphasis). Thus, Descartes "*must have at least glimpsed* these implications," and Geulinx and Malebranche "*must have been compelled*" to formulate their "fantastic" philosophy "by the need to escape the Cartesian impasse" (12, 13; my emphases).

This type of story does have at least two advantages. First, transitions are relatively easy to effect. Kant "must be mentioned" (13) after Leibniz, even though he does not appear anywhere later in the text, because "it seemed inevitable to conclude that he had failed" and hence that "the time was ripe . . . for radical . . . solutions" (14). The second advantage is that the collective mind need not always be coherent. Although the Romantics rejected neither modern science as such nor its findings, yet they "rejected not merely the foundations of the science of Descartes, Harvey, and Boyle but the foundations of science itself" (20). Such a contradiction adds psychological interest to the story but makes the thesis rather difficult to follow. (And speaking of thesis, what happened to "the rise of modern science" on pp. 12–15?)

I agree with Eichner that Peckham's paper of 1951 is still valuable. Instead of elaborating on that paper, Eichner might have considered exploring its implications for the transmission and alteration of specific concepts in individual minds. The real test of a theory such as Peckham's would be whether the general outlines hold up when tested against the conceptual revolutions experienced by small groups of individuals; this approach is the one taken by many recent historians of science. Who concluded, after Kant, that "the time was ripe," and what specific tenets of Kant's system, or which of his sentences, stimulated that conclusion? Was the conclusion debated in pubs or courts or lecture halls? Were letters exchanged? What specific con-

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.

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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 antisience 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