Reviews

Creation and the History of Science. By CHRISTOPHER KAISER. London: Marshall Pickering, 1991. 316 pages. Grand Rapids, Mich.: William B. Eerdmans, 1991. 316 pages. £13.99; $17.95

A welcome development of the last few years has been widespread recognition that issues of science and religion can profitably be studied in terms of their history. Indeed, one can cogently argue that without historical perspective, all our current discussions will become curiously foreshortened and lack an essential element of understanding. The present book attempts to address an important aspect of this problem.

To place the doctrine of creation in its historical context must be much more than an exercise in theological history (or historical theology). Inevitably, questions of science, or at least of humanity's understanding of "nature," prove to be of critical importance. This arises for the simple reason that our modern compartmentalization of knowledge would have been incomprehensible in the early Christian world and unacceptable for the following several centuries. A sharp differentiation between science and theology is entirely a modern phenomenon. So in a series entitled The History of Christian Theology, it is surely right to include a volume entitled *Creation and the History of Science.*

The author, trained in both science and theology, is currently Professor of Historical and Systematic Theology at Western Theological Seminary, Holland, Michigan. Within three hundred or so pages, he has compressed nearly two millennia of Christian thought about the natural world. The series editor, Paul Avis, rightly commends the book as a resource for all concerned with the interaction of Christian theology with natural science. From Basil (on whom much is written) to Bohr (whose contributions are telescoped into a few pages), the constantly shifting relationships between science and the Christian religion are chronicled with care and thoroughness. Despite a certain dryness of style and the diversity of factual material included, the author succeeds in maintaining the reader's interest for much of his narrative. Any attempt to be as comprehensive as this is bound to pose problems for readers unfamiliar with the whole field of inquiry. Certainly, the author leaves no stone unturned beneath which may lurk an unsuspecting divine speculating on nature or a practitioner of science wrestling with profound theological issues. With one major exception (see below), the author can certainly not be accused of avoiding difficult subjects.

The value of Kaiser's book as a resource for the general reader is considerably heightened by his use of a wide range of secondary sources, an evident familiarity with at least some of the primary material, and a judiciously selective bibliography. As a broad-canvas survey, the book must surely replace older works that served their own generations so well in specific areas of the subject. One thinks of Wallace-Hadrill's *The Greek*

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Patristic View of Nature (1968) and John Dillenberger's *Protestant Thought and Natural Science* (1961). At least the present reviewer welcomes the author's insistence on specifying the relevant biblical passages, which are so often taken for granted (or even misquoted) in this genre of literature.

Yet certain features of the book leave one slightly uneasy. Perhaps the most insistent doubts are raised by the lack of clear-cut thesis or motif. To be sure, Kaiser speaks frequently of what he calls "the creationist tradition" (nothing to do with the modern movement of that name). However, this theme fails to unify the book for several reasons. Partly, the difficulty is a question of definition. At first, the tradition is defined in terms of four broad beliefs: the comprehensibility of the world, the unity of heaven and earth, the relative autonomy of nature, and the ministry of healing and restoration. However, on page 73, the author's summary identifies four rather different components—and goes on to add another two. Elsewhere the concept is used differently again. This lack of precision generates an uncomfortable feeling that what is called "the creationist tradition" is in reality nothing more than the views of those who believed in a Creator separate from the created world. These, of course, varied widely over space and time. A tradition so fluidly conceived thus encompasses everything and explains nothing.

There are some very good features of Kaiser's historical writing: his dislike of simplistic generalization; his willingness to engage with philosophies as widely ranging as hermeticism, materialism, and romanticism; and his recognition of the crucial importance for science of such events as the Paris Decree of 1277 and the Protestant Reformation. Nevertheless, it is at the level of historiography that he is most vulnerable. At a trivial level, Cavendish was Henry (not Charles), Coulomb was Charles (not Henry), and Davy was not Humphrey but Humphry. The supernova that transformed astronomical thinking occurred in 1572, not 1604. Throughout the book, the nineteenth-century term *scientist* is applied to persons long before the word, with its implications of specialization, was invented. Even more anachronistic is the suggestion that Aristotle was a "physicist"! The dubious evidence of Luther's *Table Talk* is presented as evidence of Luther's anti-Copernican attitudes.

More seriously, one may question a judgment that dismisses Copernicus's references to hermetic writings as "merely in the way of literary illustration"; plenty of other evidence exists for his hermetic leanings. And in the light of evidence that Davy was more of a romantic than an orthodox Christian, it is curious that Kaiser sees the inconsistency as more of a problem for the present-day historian than for the early nineteenth-century scientist. Is it not the task of the historian to identify as precisely as possible the nuances of belief in his or her subjects? In Davy's case, the argument for a creationist position in almost any sense of the term rests on the flimsiest foundation.

One other major historical judgment demands some comment: the decision to concentrate on physical science and to avoid altogether questions associated with evolutionary biology. Remarkably, the author offers no preface (in which such decisions might be explained), but the publisher's blurb does proclaim the book to be "a comprehensive survey of the relationship between the theology of creation and the history of science." It is hard to imagine any more important example of that relationship than in the

context of the Darwinian controversies when, for example, questions of natural theology assumed a wholly new significance. In these circumstances, comprehensiveness can hardly be claimed, but within its self-imposed limits, the book can be recommended warmly as one of several new essays on this theme. Lacking the historical sophistication of John Brooke's *Science and Religion: Some Historical Perspectives* (1991), the book nevertheless displays more clearly the theological issues at stake. And although it does not focus as closely on specific themes as Harold Nebelsick's recent books *Circles of God* (1985) and *Renaissance and Reformation* (1992), it ranges much more widely in its use of recent historical scholarship. It deserves a wide readership.

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Conceptual Revolutions. By PAUL THAGARD. Princeton, N.J.: Princeton Univ. Press, 1992. 344 pages. $35.00.

The dust-jacket blurb for Paul Thagard's book claims, "It is clearly written and should be accessible to anyone interested in problems of scientific change." This true up to a point, but Thagard's main argument turns out to be directed at other groups in his own fields of cognitive psychology and artificial intelligence. Take this remark, for instance: "In contrast to the sometimes acrimonious debate between proponents of symbolic AI and purportedly subsymbolic connectionism, I see cognitive science as using a continuum of complementary computational methods" (p. 29). Both the language and the reference here exclude the common reader. Thagard similarly excludes historians and sociologists of science from his debate, concentrating on a very narrow, abstract, decontextualised account of how conceptual revolutions in science can be modeled with the aid of computer programs. He claims to prove by these means that these revolutions in scientific thought are wholly rational, not influenced by social or historical considerations.

This is perhaps the point of interest for the noninitiate reader. But in order to make it, Thagard uses a whole number of unexamined and sometimes unspoken assumptions: For instance, that what he calls "mental structures" *are* "analogous to data structures in computers" (p. 21); that his history of "scientific revolutions" has some kind of objective validity; that his reconstructions of scientific revolutions bear any relation to the ways they were thought of or presented at the time. Throughout the book, there is also a tacit assumption that scientific thinking is the highest form of thinking, or can stand for all human thought; that science is "true"; and that scientific "facts" are unquestionable. (For a much more skeptical philosophy of science, I would refer readers to Paul K. Feyerabend's splendid *Against Method* [rev. ed., London: Verso, 1978].) These assumptions lead to the presentation of his computer models not just as "descriptive," but to some extent "prescriptive"—they can help us, he claims, both to

teach thinking to computers, and to organize our own criteria for accepting new concepts.

Thagard's book begins with five introductory chapters that set out his definitions of terms like *concept* and *explanation.* Chapter 1 is entitled "The Problem of Revolutionary Conceptual Change"; chapter 2, "Concepts and Conceptual Systems"; chapter 3, "Conceptual Change," uses Lavoisier's oxygen theory as an illustration of conceptual change in science; chapter 4, "Explanatory Coherence," introduces a computer program named ECHO, which models conceptual change. Arguments against other positions, especially sociological ones, occur in chapter 5, "Theory Dynamics, Rationality, and Explanation."

Strangely, Thagard takes sociologists of science to be arguing that individual subjective "motivation" (rather than "rationality") influences the adoption of new scientific concepts. He ignores the role of social institutions and cultural systems. He displays a touching faith in the institutions of the scientific community to sustain "objectivity": "Through the process of peer review, personal motivations tend to be cancelled out. In science on the international scale at which it is now practiced, group and national motivations also tend to be cancelled out" (p. 113). This reference to science "now," of course, cannot apply to his earlier examples, "science" itself being a fairly recent, nineteenth-century construction; and it is precisely the whole cultural edifice of the scientific institution that interests sociologists of science.

In chapter 5, Thagard also attacks and demolishes an analogy that has been made between religious conversion and the adoption of a new scientific paradigm. According to him, religious conversion has a large emotional element that is missing from scientific change. So he assigns emotion to religion, and rationality to science, in a rather old-fashioned, late-nineteenth-century move—not surprisingly, as his main authority for what religious conversion is like is William James. But how can we know what element of emotion comes into scientific "conversion"? And this opposition of emotion and rationality also has no room for the subtle rationality of a religious thinker like Newman.

Having established his main arguments and definitions in chapters 1 through 5, Thagard then applies them to various particular cases in subsequent chapters: "The Darwinian Revolution," "The Geological Revolution," "Revolutions in Physics." Chapter 9, "Revolutions in Psychology?", looks at some movements in psychology (Behaviorism and Cognitivism), and concludes that these do not count as revolutions. The assumption seems to be that disciplines like psychology and sociology are striving to become sciences, but have not quite got there yet. Chapter 10, "Conceptual Change in Scientists and Children," argues convincingly against an analogy between scientific revolutions and conceptual change in children.

The only one of these areas I know much about in detail is the Darwinian "revolution." Here, Thagard sets up a computer model of his definition of Darwin's hypothesis, based on its articulation in *The Origin of Species,* and pits it against his construction of what he calls the rival "creationist" hypothesis. Of course, Darwin wins, on grounds of complexity and comprehensiveness of rational explanation of facts. (Interestingly, the only facts used are those mentioned by Darwin himself, for and against his

theory.) This approach ignores completely the complex work that has been done on Darwin's notebooks, showing that the *Origin* represents a recasting of his hypothesis in the prevailing deductive scientific model of his time. (See especially Howard E. Gruber, *Darwin on Man: A Psychological Study of Scientific Creativity* [New York: E.P. Dutton, 1974].) In this case, the *Origin* could be regarded as a triumph of rhetoric, not of rationality. Thagard notices in passing the complexity of thought revealed by the notebooks, wistfully remarking, "I expect someday we will have computational models of reasoning and learning that are rich enough to provide a much more detailed account of the conceptual generation displayed in Darwin's notebooks" (p. 135).

In simply pitting Darwin's "rationality" against "the creationists," Thagard also ignores the interesting and subtle work that has been done on the complex interdependences of science and religion in the nineteenth century (see for instance John H. Brooke, *Science and Religion: Some Historical Perspectives* [Cambridge: Cambridge Univ. Press, 1991]); as well as the ways in which Darwin's thought fit into other contemporary social and political patterns, as shown recently by Adrian Desmond and James Moore in their biography *Darwin* (London: Michael Joseph, 1991). Seen from these angles, both Darwin's thought, and its acceptance, can be given historical and cultural explanations.

Paradoxically, Thagard's whole project reminds me of the circular reasoning of the early nineteenth-century natural theologians. Having decided that the natural world was evidence of a benign Creator, they then investigated it to prove that this was so. Similarly, Thagard, having decided that science, thought, and computation are all the same thing, that science is rational and true, and that certain episodes in the history of science are "revolutions," then constructs computer models to prove all this is so. By the way, the natural theologians produced a mass of useful natural history; Thagard, too, may be producing useful models for computer training, but his claims can go no higher. A further point of analogy is that the natural theologians used a mechanical model of the universe, in keeping with their machine age; similarly, Thagard uses the contemporary computer as his model for thought (and truth). He, too, is a product of his age.

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Pierre Duhem: Philosophy and History in the Work of a Believing Physicist. By R.N.D. MARTIN. La Salle, Ill.: Open Court, 1991. 274 pages. $44.95; $19.95 (paper).

German Science. By PIERRE DUHEM Trans. John Lyon. Introduction by Stanley L. Jaki. La Salle, Ill.: Open Court, 1991. 140 pages. $38.95; $17.95 (paper).

The publication of two books—one by Duhem and the other about him—raises a number of questions. Who was Pierre Duhem? What did he do that was memorable? What does he have to do with the science-religion dialogue? Pierre Duhem (1861–1916) was a French physicist who spent his life feuding with the scientific establishment of France and was therefore unable to get an academic position in Paris. His contributions to physics are remembered more reliably by physical chemists than by physicists. He was a master of classical thermodynamics, and the Gibbs-Duhem equation still bears his name. The equation is one way of expressing the combined first and second laws of thermodynamics.

After his reputation in science was well established, Duhem branched out into studies of the philosophy and history of science. In these endeavors he was influenced by a devout Roman Catholic background that left him with a faith and commitment that were rare among the rationalist French scientists of his period. As a historian of science, Duhem—after his medievalist conversion in 1903—led the way into an appreciation of the Middle Ages, not as an arid period that had to be endured scientifically until the Renaissance could burst forth, but rather as a period of genuine if measured scientific progress. His multivolume work, *Système du Monde,* on the history of science between Plato and Galileo, was not complete at the time of his death; it expresses at great length his findings on the Middle Ages.

To understand Duhem's place in the science-religion dialogue, it is necessary to realize that in his youth he was steeped in the writings of Blaise Pascal, who was, like Duhem, a believing scientist from France. It is no accident that both Duhem and Pascal were never entirely trusted by their church. Both of them took seriously both their science and their religion, believing that there is a meaningful connection between the two subjects. Duhem's interest in the Middle Ages is an integral part of this attitude. As he aged, Duhem was increasingly influenced by Pascal's thought; quotations from Pascal appeared with ever greater frequency in Duhem's writings.

In his own time, Duhem and his ideas were not very influential, but during the past few decades there has been an upswing in interest in him. For a long time the only biography of him was by his daughter Hélène (1936). His writings were mostly available only in French—but until recently the typical English-speaking Ph.D. in most subjects could be expected to read material in that language. A full-scale biography of Duhem was published by Stanley L. Jaki in 1984, and quite a few works on Duhem have been appearing in recent years. The book by R. N.D. Martin is not a biography; rather, it is an authoritative yet readable account of the work of Duhem as historian and philosopher of science.

The work by Duhem, *German Science,* is an English translation of *La science allemande,* a set of four lectures delivered by Duhem in 1915 at Bordeaux under the auspices of the Catholic Students' Association of the University of Bordeaux. The title and the date remind one of the fact that Germany and France were locked in mortal combat at the time; Duhem lacked nothing as a loyal *enfant de la patrie,* and so it would be expected that there would be a lot of Boche-bashing in such a series of presentations. Under the circumstances, Duhem's work seems almost moderate on the surface. But underneath, there is a current of deep-running prejudice on the part of Duhem, not against Germans or their science, but against certain

scientific attitudes that Duhem was able to tag with the convenient label *German,* almost as Mark Twain used as pejorative the word *French.*

Duhem's teachings shine through in his lectures on German science. (1) He did not believe in atoms. (2) He disliked mechanical models and geometric arguments, preferring intuitive thinking and algebraic constructions. In his distaste for atomic physics, Duhem was already in his own time approaching scientific isolation; Ernst Mach, his Austrian contemporary, was notorious for sharing this already outmoded prejudice. The physics of Britain was for Duhem little better than the physics of Germany, because of the domination of the Scots Lord Kelvin and James Clerk Maxwell; Kelvin was especially fond of trying to reduce all systems electrical, magnetic, optical, hydrodynamical, etc. to mechanistic models.

To the professional scientist of the late twentieth century, Duhem's judgments seem outrageously wrongheaded. German physics is discussed with mention of Hermann von Helmholtz and Rudolf Clausius, but with no mention of Max Planck or of Wilhelm Roentgen. The latter was the discoverer of X-rays and was awarded the first Nobel Prize in physics, with the approval of just about everyone in that time or since. Albert Einstein and Hermann Minkowski are mentioned by Duhem, but their work is sadly misinterpreted.

The Germans are accused of never taking an intuitive approach to science. In this regard Duhem may be excused for not knowing that Heisenberg would in 1925 discover quantum mechanics by a highly intuitive process. But it is not excusable that he mentions the German chemist August Kékule for his work on chemical bonding without remembering the intuitive process by which he discovered the ring structure of benzene. Conveniently enough, when Duhem discusses trigonometric series, he mentions all sorts of great names in mathematics, but never gets around to Joseph Fourier, the Frenchman whose methods were too analytic (hence Germanic) for Duhem's taste, but whose work on such series was so important that Fourier series are named for him.

The modern reader will with difficulty forgive Duhem his chauvinism and will find charming his propensity for quoting Pascal. But there can be no forgiveness for his dismissal of Maxwell's electromagnetic theory. Duhem said, "The equations of Maxwell, in fact, not only run counter to the teachings of a scientific and learned physics: they directly contradict truths accessible to everyone" (p. 100). Any physicist who in 1915 said such things must be considered suspect as a guide through matters scientific.

So why should we pay attention to Duhem? Perhaps the times call for a fresh look at "German science" as something certain to appear in our times from a newly reunited Germany. What is the true meaning of "German" when one speaks of science? Does this attribute stem from genetics, from culture, from language, or from another source? What is the place of religion in a German version of science? Duhem never gets near any of these questions in his lectures. He notes that not all German scientists behave "like Germans" (using Duhem's definition of the term, Heinrich Hertz was Germanic, but not Hermann von Helmholtz). He never recognizes that the Franks were originally Germanic in their genes, culture, and language. He shows no sensitivity to regional differences among the Germanies in all these matters. The inescapable conclusion is that Duhem

can tell us very little of value about German science, for his own time or for ours.

In our age it seems inappropriate to talk of science on a nationalistic basis, or even to consider European science as distinct from the American or Asian varieties. Large laboratories show an increasingly international flavor. Science faculties at large universities have more multicultural diversity than in years past. The results of scientific research, especially in the physical sciences where Duhem worked, have produced principles that do not respect nationality. The Gibbs-Duhem equation is valid in Germany, Italy, Russia, Japan, and England—not just in America and France where its respective inventors lived.

It is good to read about Duhem as a forceful if sometimes wrong-headed intellectual leader. It is good to have Duhem's own clear summary of his opinions published in English. It will be bad if very many scientists of subsequent generations follow him too closely.

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Mortal Questions. By THOMAS NAGEL. New York: Cambridge Univ. Press, [1979] 1991, 213 pages. $12.95 (paper).

"There are more things in heaven and earth, Horatio, than are dreamt of in your philosophy" is a statement that expresses a central theme of *Mortal Questions,* a collection of fourteen essays by Thomas Nagel, professor of philosophy at the State University of New York. Writing in a clear, nontechnical, but analytically rigorous fashion, Nagel finds an underlying duality in questions concerning the meaning of life, the mind-body problem, and foundations of value. The problems arise because certain subjectively apparent facts about the self seem to vanish as one ascends to a more objective standpoint.

For example, in "The Absurd," he claims that from the subjective perspective of a human agent, life is "full of effort, plans, calculation, success and failure" (p. 14), but from an external detached perspective all seems arbitrary and meaningless. In "What It Is Like To Be a Bat," Nagel notes that from the point of view of the conscious organism, there is a reality of what it is like for that organism to have a sensation. But the objective scientific view does not even attempt to explain this reality, for its methodology has no access to the subjective character of experience. In "Fragmentation of Value," Nagel argues that an agent-centered morality which absolutely prohibits intentional killing of innocent people crumbles when faced with horrendous outcomes viewed from a point of view external to the agent.

Nagel claims that the objective and subjective pictures are both true, but partial. He insists on the reality of facts beyond the reach of human concepts, "the Hamlet Insight," (p. 171). The objective and subjective perspectives present conflicting points of view, but both are necessary for understanding mortal questions (as Nagel maintains in

his 1986 book *The View from Nowhere* [New York: Oxford Univ. Press]).

Nagel's strength is his ability to set out clearly and concisely important arguments that force us to look at the world differently. His weakness is that he too quickly accepts the view that he has uncovered some paradox revealing the inevitable and irresolvable dispute between the subjective and objective points of view. I will examine two examples of this over-hasty skepticism.

In "Panpsychism," Nagel shows how the commitment to nonemergence and the irreducibility of the subjective character of experience entails panpsychism, the view that all matter has psychological properties. He sets out the argument as follows:

1. Any living organism is a complex material system. No constituents besides matter are needed to explain the organism.
2. Ordinary mental states like thought, feeling, emotion, and sensation are not physical properties of the organism.
3. Mental properties are real properties.
4. There are no truly emergent properties of complex systems.
5. So all matter must have mental properties. (pp. 181–82).

Nagel does not endorse the conclusion. Rather, he takes the argument as a sign that we have not yet thought of all the possibilities for solving the mind-body problem. Panpsychism is but "one more mutually incompatible and hopelessly unacceptable solution to the mind-body problem," further confirmation of Nagel's skeptical leanings (p. 193).

In "What It Is Like To Be a Bat," Nagel supports premises 2 and 3, for he uses the conscious life of a bat to convince readers of the reality of the subjective point of view; i.e., that there is "something that it is like to *be* that organism—something it is like for the organism" (p. 166). This seems to suggest that mental states are real without being physical properties of the organism. To be a physical property is to be a property that can be accessed from some point of view, but to be a subjective reality is to be a reality that can be accessed only from some particular point of view.

However, Nagel's supports for premises 1 and 4 are not as convincing. He supports premise 1 by arguing that denying premise 1 entails dualism. He supports premise 4 by arguing that denying premise 4 entails denying the principle of causal determinism. But there is no reason to think that denying premises 1 or 4 has these consequences. Denying premise 1 entails dualism only if different systems of explanation must correspond to different kinds of entities. But reifying systems of explanation commits what Alfred North Whitehead called "the fallacy of misplaced concreteness" (*Science and the Modern World,* New York: Macmillan, 1925, p. 52).

This fallacy also explains why Nagel mistakenly thinks that premise 4 entails the denial of causal determinism. According to Arthur Pap, any scientific explanation system applies to the concrete case only if certain closure assumptions hold; i.e., assumptions that traits not mentioned in the state variables of the explanation system do not matter to the values of the state variables (*Introduction to the Philosophy of Science,* Glencoe, Ill.: The Free Press, 1962, p. 315). For example, the parabolic course of a Kirby Puckett home run can be described or explained without reference to any electromagnetic fields present in the stadium. If the baseball were wound with wire instead of string, then the theoretical account would not apply

to the ball unless another system of laws, electromagnetic ones, were introduced. Such a state of affairs would not falsify the Galilean equations of motion governing free fall, but it would indicate the need for another system of explanation without positing any additional substances.

Similarly, to say that there are emergent properties of the mental that cause and respond to physical processes need not entail a second substance, but only a different explanation system. Explanation of human behavior may require interpenetration of different systems of explanation; it need not require different substances. Causal determinism holds at the level of abstraction and in the concrete as long as the closure assumptions hold. When closure fails, it is a sign that more than one system of explanation is needed to account for the concrete reality (See Harold Austin, "'Orientation to Situation' in Talcott Parsons's Action Theory," Ph.D. diss., Univ. of Chicago, 1975, pp. 13–18).

If this account makes sense, one can reject premises 1 and 4 and escape the absurdity of panpsychism. One can also see a way of appropriating recognition of the subjective point of view without accepting dualism. Skepticism about Nagel's skeptical conclusion seems warranted.

Another irresolvable arena Nagel points to is the foundation of value. In "Fragmentation of Value" he argues,

> I myself do not believe that all value rests on a single foundation or can be combined into a unified system, because different types of values represent the development and articulation of different points of view, all of which combine to produce decisions. (p. 138)

But the fact that there are different points of view that produce different decisions does not necessitate that there is no single foundation for morality. Only if there is no way to rank different types of values does this follow. But the latter claim is just what is at issue.

Perhaps what leads Nagel to embrace the skeptical conclusion concerning values is his criticism of agent-centered moral absolutes in "War and Massacre." Nagel simply assumes that it is reasonable to violate the absolute prohibition against intentional killing of the innocent when the stakes are extremely high. He then concludes that there just are situations that the world presents where nothing we can do is morally right.

Nagel's emphasis on the dualism of the objective perspective characterized by outcomes and the internal perspective characterized by an agent-centered morality prevents him from seeing an important relationship between agent-centered and outcome-based value. A utilitarian concern for the general happiness must assign a value to the happiness of an individual agent. But a violation of an agent-centered restriction undercuts the very value that grounds the utilitarian's concern for the general happiness. Nagel is wrong to interpret agent-centered morality as unconcerned with the common welfare or the common good, a value allegedly reserved for the objective perspective. The common good for the agent-centered view is a fully shareable good (see Thomas D. Sullivan and Gary Atkinson, "Benevolence and Absolute Prohibitions," *International Philosophical Quarterly* 25 [September 1985], 247–59). It is a good in which one person can participate without depriving others of that very same good.

For example, friendship is a common good. One and the same good is possessed by friends. Friendship is the good of each. This contrasts with

the utilitarian's conception of the common welfare as the aggregate good. An aggregated good cannot be possessed in common. It cannot be fully shared.

The agent-centered view of the common good is no less objective than is the utilitarian's. But it is a notion that would take precedence over the utilitarian's notion of common good since the aggregated good presumes some value in the individuals. If the utilitarian holds that maximizing the aggregate happiness is always the better action, then the individuals' goods become mere instruments to the aggregate good. By contrast, these goods retain their value as nonmeans in the agent-centered view. Contrary to Nagel's thesis, there seems to be a recognition of the primacy of the agent-centered concern over the outcome-based concern. As Atkinson and Sullivan claim,

> In short the absolute refusal to perform sordid and ignoble acts proposed in the name of "general happiness" reflects the belief that to attack our life in common by seeking to cut someone off from that common life constitutes a betrayal of humanity, a betrayal of what gives moral point to our struggles and sacrifices and deaths. ("Benevolence and Absolute Prohibitions," p. 259)

While I have focused on the weaknesses of Nagel's overarching claims about the irresolvable nature of some problems that pit subjective against objective perspectives, there is much worth the reader's attention in his book. Nagel builds a strong defense of the autonomy of ethics as a discipline against the sociobiologists in his chapter "Ethics without Biology." He offers an excellent summary of the recent work on brain bisection and its implication for the unity of mind in his chapter "Brain Bisection and the Unity of Consciousness." Nagel's prose is crisp and elegant. Many of the essays in the collection have become contemporary philosophical classics over the last fifteen years, because they set out the problem so clearly and because they point philosophers to new worlds not dreamt of in their present-day philosophies.

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