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Methodological Naturalism and Theistic Science

Law professor Phillip Johnson, author of *Darwin on Trial,* has emerged as a leading spokesman for religious opponents of Darwinism, with many followers, many lecture invitations, and (for a while) his own e-mail discussion group. One of his themes, more fully elaborated in a recent book, *Reason in the Balance* (Johnson 1995),[1] is that Darwinian biology is only one expression of the underlying source of most contemporary evils, *philosophical naturalism.*

Many critics (including a number of scientists who are evangelical Christians or theists of other varieties) have argued that Johnson goes astray by failing to distinguish between *ontological* and *methodological* naturalism. A theist, they say, can embrace the latter though not the former.

Johnson and his followers reject the distinction. To see why, we must first note that Johnson understands what "methodological naturalism" is in one way, his theistic critics another. He treats it as a general epistemological thesis, to the effect that reliable knowledge is to be had only when we eschew supernatural causes and explanations (Johnson 1995: 17; 206f.). If *that* is what "methodological naturalism" is, then of course ontological naturalism will follow quite quickly, and the distinction has no point (unless perhaps as a devious ploy by naturalists to trick religious believers into giving away the store). His theistic critics understand it as a thesis about the nature of *science*, and hold that scientific inquiry is not our only way of gaining knowledge. The latter claim is obviously essential if there is to be a significant difference between the brand of methodological naturalism they favor and the brand Johnson attacks. However, I shall not discuss it here, concentrating instead on the thesis that *in the sciences* supernatural causes and universe-transcending agents are not to be invoked. Proponents of the thesis point out that this requirement has long been among the agreed-upon ground rules of the sciences, and that without such a rule "explanations" would be too easily come by and the *explanantia* quite untestable.

Johnson offers no serious response to the latter argument, and dismisses the ground-rules point as merely an expression of the cultural hegemony of naturalism (Johnson 1995: 98). However, his followers include some philosophers who have taken up these issues, and it is their arguments that I propose to examine. In particular, I will be discussing two substantial

papers in *The Creation Hypothesis* (Moreland 1994),[2] a collection of essays with a foreword by Johnson.

In "Theistic Science & Methodological Naturalism," J. P. Moreland suggests that "theistic science" should be regarded as a research program in the sense defined by Imre Lakatos (Moreland 1994: 51f.; 13). Perhaps he would be willing to amend that to "family of related research programs in different sciences," lest he be committed to too ambitious a unity-of-science project. In any case, his main concern is to argue that "theistic science" is legitimate *science*.

Moreland's response to the "ground rules" point is that "theistic science has been recognized as science by philosophers and scientists throughout much of the history of science [,so] the burden of proof is on anyone who would revise this tradition" (Moreland 1994: 51). He doesn't identify the philosophers and scientists he has in mind, but presumably they include Newton and many of his contemporaries and predecessors, as well as the clerical natural historians of the first half of the nineteenth century. After all, Newton included several paragraphs about God in the General Scholium to his *Principia*, concluding "thus much concerning God; to discourse of whom from the appearances of things, does certainly belong to natural philosophy" (Newton 1934: 546).

But that quotation illustrates the flaw in Moreland's argument. All or nearly all the authorities he could cite come from periods when no distinction was made between "science" and "natural philosophy." Whenever the distinction *has* been made, it has been agreed that appeals to supernatural powers have no place in *science*.

Moreland might be willing to concede the historical point. His primary claim is that any such ground rules are misguided, since Laudan and others have shown that no demarcation of science from philosophy and other allied enterprises is possible. This is not the place to rehash the disputes over demarcation criteria. But why is Moreland so insistent on calling his enterprise "theistic science," if it was a mistake to try to segregate science from natural philosophy in the first place?

In any case, if theistic and naturalistic research programs are to be competitors within a common enterprise, whatever it is to be called, there must be common standards against which their relative success can be measured. Moreland agrees that problem-solving effectiveness, and the promise of future problem-solving, are appropriate standards. But not the only ones. We must recognize that the sciences face conceptual as well as empirical problems, and that the conceptual ones come in two varieties: internal and external. The external problems facing a discipline are those arising out of conflicts between its currently-accepted theories and those of

other disciplines. Theology is among the disciplines that can legitimately pose conceptual problems for various sciences, and it is a point in favor of theistic research programs that they can solve such problems more successfully than naturalistic research programs can. In general, people assessing rival programs may legitimately differ in the relative importance they assign to empirical and conceptual problem-solving. In particular, responding to complaints that theistic science has not been an empirically fruitful program, Moreland says:

> Creationists may elevate the virtue "solves theological or philosophical internal and external conceptual problems" above the virtue "offers solutions yielding empirically fruitful lines of research." There is nothing unscientific about this at all, and it is question-begging to claim that a criterion of empirical fruitfulness set by one research program (say, the search for evolutionary mechanisms) should be most important for a rival program. . . . (Moreland 1994: 63)

I want to make three points about this passage. First, it seems to run together two distinct issues: (a) the relative importance of empirical and conceptual problem-solving, and (b) whether different research programs may legitimately generate different standards as to what counts as *empirical* fruitfulness (Johnson 1995: 108).[3]

The second point is that if theology is a legitimate source of "external conceptual problems" for a research program, then so is a naturalistic ontology. If it counts for theistic and against Darwinian biology that the former comports better with central propositions of traditional theology, then the greater ability of Darwinism to solve "external conceptual problems" stemming from ontological naturalism is a point in *its* favor. Applying the "solves external conceptual problems" standard leads to a standoff, as it always will when a research program impinges on an area where the fundamental issues are in controversy. No doubt that is one reason why people who think of themselves as scientists have given little weight, in assessing research programs, to the criterion "solves external conceptual problems."

That leads us to the third point. Whatever difficulties philosophers have encountered in formulating a demarcation criterion, there is a recognizable set of disciplines, research programs, and practitioners that everyone will agree are "scientific." And among these practitioners, nearly all (whatever their philosophical or religious convictions) make empirical

problem-solving the primary test of a research program's success. An individual may choose to *pursue* a particular program because it solves conceptual problems better than its rivals, and may believe (in spite of the present state of the empirical evidence) that in the long run it will prove more empirically fruitful. But that same individual will also agree that the scientific box score primarily counts empirical problem-solutions.

So proponents of theistic science need to answer the charge that these programs *cannot* be empirically fruitful, since appeal to supernatural agencies precludes systematic investigation and testing. The most detailed response I have seen is by Stephen C. Meyer in "The Methodological Equivalence of Design & Descent," another essay in *The Creation Hypothesis*. Design hypotheses and Darwinian-style naturalistic hypotheses are "methodologically equivalent" in that "neither can meet standards of testability that depend on notions of repeatability" but "both can meet alternate standards . . . such as inference to the best explanation or 'consilience' that involve notions of comparative explanatory power" (Meyer in Moreland 1994: 89). The repeatability standards are appropriate for those sciences "that are concerned primarily with the discovery and replication of general phenomena," but not for "those concerned with past events and causes, such as evolutionary biology, historical geology, and archaeology" (Meyer in Moreland 1994: 88). In these *historical* sciences, comparative explanatory power is the proper standard of evaluation (and it would be wrong to build in a requirement that the explanations must be in terms of natural laws, since that is not the characteristic mode of explanation in historical sciences). In many of the historical sciences explanations invoking personal agents are routinely given – so there is nothing inherently unscientific about them – and there is no reason they shouldn't be used in historical geology or biology.

Here, of course, Meyer blandly glides by the methodological naturalists' point. Their objection is not to the appeal to personal agents in sciences like archaeology, but to the invocation in any science of *supernatural* persons and *universe-transcending* agents. Presumably Meyer thinks it arbitrary to allow one kind of person (humans) to appear in legitimate scientific explanations, but not another kind (God). Methodological naturalists say it isn't arbitrary, because appeals to divine agency provide an immediate easy explanation for anything – "the good Lord willed it so" – and render scientific inquiry otiose.

No, Meyer would say, the scientists needn't fear being put out of work. Nor is the proposing of "God's will" explanations such a free-wheeling exercise in uncontrolled speculation as the naturalist supposes –not for practitioners of theistic science who are working within the

framework of traditional theology. The traditional doctrine is that God intervenes in the natural order only rarely, and then only for purposes of a specific kind, namely "on behalf of human beings (*e.g.* creation or redemption)" (Meyer in Moreland 1994: 97). So *most* of the time, appeal to divine agency is inappropriate, and scientific inquiry can proceed in the customary ways. Only the really big, humanly-important phenomena like "the origin of life, human consciousness, and the universe" (Meyer, at Moreland 1994: 98) call for explanation in terms of divine purposes.

Two comments. First, it is noteworthy that Meyer's list of candidates for divine explanation leaves out one of the favorites of anti-Darwinian design theorists. I refer to the idea (which appears prominently in other chapters of *The Creation Hypothesis* and elsewhere in the "intelligent design" literature) that Darwinian theory can explain only microevolutionary phenomena – minor variations over time within created "kinds," which *might* include the emergence of new species as defined by contemporary biologists, but certainly not the origins of major taxa (*e.g.* phyla, classes; orders). Meyer is wise to omit this item, since it would be an unenviable task to have to explain, for each major taxon, what its emergence has to do with the creation or redemption of human beings. But many of his colleagues would be loath to give up this argument for design.

The second and crucial point is that while Meyer may have succeeded in dispelling nightmares about divine micromanagement rendering nature unpredictable and scientific investigation pointless, there is a fundamental problem he hasn't addressed: How are theistic scientists going to come up with *specific explanantia*? A personal-agency explanation must include specific statements of the agent's purposes and the agent's beliefs as to what means are available for the achievement of the purposes and what constraints must be met. Now it is hard to see what the notions of "means" and "constraints" actually come to in the case of an omnipotent Agent, and this disanalogy between divine and human agency makes divine-agency explanations hard to evaluate. If we leave that point aside, there remains the question: How can anyone say, with sufficient specificity, what the divine purposes are? As Darwin wrote in one of his early notebooks, "we know nothing of the will of the Deity" (Durant 1985: 46).[4]

Meyer and his colleagues would presumably reply that Darwin was just wrong about that; we do have significant knowledge of God's purposes, for they have been revealed to us. The trouble facing theistic-science research programs is, though, that what is traditionally supposed to have been revealed to us includes precious little information about God's purposes in relation to nature. We are told something about God's purposes for humanity, but as to nature we are told only that it is meant to be an

appropriate habitat for human beings, and to express God's wisdom and grandeur. But there are indefinitely many imaginable ways in which the divine glory and wisdom might be manifested, and indefinitely many possible worlds that include human beings. So why do we have this particular world, with these particular features? We have no access to the kind of information about divine purposes that would allow us to answer that question.

To this line of argument Meyer might retort that it misses the whole point of inference-to-the-best-explanation arguments. In the context of IBE arguments, you don't need, in order to be able to say that hypothesis H provides a better explanation than its rivals, to know in advance that it is *likelier*. That is the conclusion of the argument, not a premiss. "Better" here means that H provides (as Peter Lipton puts it) a "lovelier" potential explanation (Lipton 1991). So theistic scientists needn't know in advance what the relevant divine purposes are; they can conjecture freely, in search of lovely explanations.

I do not find this response convincing. In the first place, the problem is not just that we don't *know* what the divine purposes are; it will generally be difficult even to come up with a conjecture that is both specific enough to explain the phenomenon in question and general enough to explain a variety of others. (We don't want the trivial specificity of "God brought about this particular state of affairs because this state of affairs appeals to God.") Perhaps I underestimate the ingenuity of those who pursue theistic-science research programs. But then there is a second point. For IBE arguments to get off the ground, the set of competing hypotheses must be fairly small, and that means that each must reach a modest threshold level of *prior* probability. And so the problem recurs: how are we to judge which ingenious conjectures meet that standard?

Finally, let us suppose that these difficulties have been overcome somehow, and one or two divine-purpose explanations for a given phenomenon emerge as the most plausible of their kind, to compete with a few natural-cause explanations. We are then invited to choose the "loveliest," the one that would have the greatest explanatory force if true. How is this choice to be made, when the proposed explanations are of such different *kinds*? It would be like judging whether a swan is lovelier than a concerto. Judgments of relative "loveliness" would depend upon taste, or upon what one thinks the *desiderata* for a good explanation are – and opinions vary considerably on that point.

Well, what's wrong with that? What's wrong with a situation (not uncommon in the sciences) where some scientists think A is the best explanation, others B, and others C? Nothing – except that inference to the

best explanation was presented as a *test* procedure, according to which both Darwinian and divine-purpose hypotheses can be regarded as testable and thus scientific. But if a procedure is to function as a test, there has to be a good prospect that in the not-indefinitely-long run something close to a consensus will be reached as to the results. When it comes to judging the relative loveliness of natural-cause and divine-agency explanations, the prospects for consensus are not good at all. Neither, therefore, are the prospects for theistic science. The case for methodological naturalism remains strong.

Notes

1. All page references in citations of Johnson are to this book.

2. All page references in citations of Moreland and Meyer are to this book.

3. This is not a merely pedantic point. Johnson and others of similar persuasion sometimes seem to be suggesting that theists and naturalists will evaluate the *empirical* evidence differently, and legitimately so. Unfortunately, I have found no explication and defense of this claim. Perhaps something could be attempted along Bayesian lines. But I suspect that in most cases the argument would be similar to the "irregular argument" that Cleanthes offers in Part III of Hume's *Dialogues Concerning Natural Religion*. Compare his invitation to "anatomize the eye. . . and tell me if the idea of a contriver does not immediately flow in upon you with a force like that of sensation" with Johnson's assertion that the fact of design "is evident to all who do not cloud their minds with naturalistic philosophy or some comparable drug" (p.108).

4. Quoted by John Hedley Brooke, "Darwin's Science and his Religion," in John Durant, ed. 1985. *Darwinism and Divinity*. Oxford: Blackwell. See p. 46.

References

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