Arnis Vilks [1991] has offered some rather severe criticisms in response to a paper of mine that appeared in the December 1990 issue of Methodus [Jacobi 1990]. The criticisms fall into three categories which claim that the paper:

[1] "...grossly misrepresents (some of) those philosophical positions that (it) criticizes";

[2] fails to consider "all sorts of philosophical problems" raised by the metaphilosophy of modern realism; and

[3] "...fails to adequately characterize the role of basic axioms for deductive economic theory."

In support of these general opinions, Vilks then itemizes a number of problems that he sees arising in the paper. It is perhaps appropriate to examine each of these arguments and reflect upon how they support the general claims.

The first point (v90) made is that my entire argument is based upon what Vilks terms 'the realist's credo' (actually, the protorealist's credo): the primitive assumption that the totality of existence is not exhausted by the contents of our phenomenological experience. He seems to be affronted by this assumption. My general response is that to deny it is to embrace some form of classical skeptical idealism with all of the problems which saw its demise in the 19th century as a serious framework for understanding the human condition, especially vis-à-vis the great progress in natural science. The practical implication of protorealism is that the properties of this 'other' are independent of our experiences and, in some unspecified way, are causally responsible for at least some of them. In a sense, my paper was directed to those who accept this assumption without quarrel; for the arguments to support it against its alternatives would lead to issues in classical philosophy far from the focus of interest of the paper. My intuition was that I would be communicating to the vast majority of those interested in issues in the philosophy of science — which was my intention — and not to convert the last remnants of skeptical idealism.

Vilks offered three arguments against accepting this 'credo'. The first was that the concept of an underlying or ultimate reality (hereafter UR) was meaningless since one could insert the concept of 'god' in its place. That is so, but it is no embarrassment. What is critical is what properties one attributes to the term UR or 'god'. UR is simply the most general and noncommittal token for indicating that our experiences are not self-generated. To use some other token, e.g. X or 'god', signifies nothing unless we attribute particular properties to those alternative tokens. The properties subsequently attributed to UR were general independence and causality (in the broadest construal of that term). I consider these to be minimal if a position other than skeptical idealism is to be entertained.

The second criticism was that my admission that this assumption cannot be proven true or false was somehow an attempt to exempt it from question. Clearly this interpretation is diametrically opposed to the spirit and intention as evidenced in the principle of radical fallibilism that denies the possibility of any foundations for human knowledge and any such exemption from question. It is not exemption from disproof that is at issue, but rather a confession that no conceivable method of proof or disproof exists. To deny this admission IS to embrace foundationalism. My position is that I can't prove or disprove 'the (proto) realist credo', nor can anyone else; so if certain readers find it impossible to accept, then this paper is not for them. Our topics for discussion will lie well outside the intended scope and purview of the paper.

The third criticism was that the question (as to whether anything exists other than the phenomenological contents of our experiences) can be passed over without making any commitment one way or another. One argument which supposedly supports this view is that the question is understandable only by English-speaking readers — an argument which I find ingenuous to the extreme — the question can obviously be translated into most languages, and into all languages with a cognitive philosophical tradition capable of supporting natural science. A second argument was that the question makes no sense to individuals not interested in philosophy — which, again seems to be a non sequitur. Surely, the implications of scientific theories extend...
beyond only those scientists who can understand the mathematical formalism in which the theory is presented. People who are uninterested or uneducated in quantum physics will still be governed by quantum mechanical laws. Nothing that Vilks or I wrote would be of interest or consequence to the vast majority of the population of the Earth — but presumably it is of significance within the community of scholars interested in these matters. A third argument was that Carnap had rejected the question on the grounds that the answer was of no consequence for science — an opinion which characterized the logical positivist metaphysics. Of course, he understood the question and its intention — it was simply that the question was considered to be of no epistemic interest given a belief that it was possible to isolate a pure category of experience, facts, to which logic could be applied to construct certain knowledge. The failure of the positivist program is almost universally recognized (I include ‘almost’, since Vilks seems to represent an exception). If one asked Carnap why he dismissed the question, it is almost certain he would have given the answer [1+] at the bottom of page 25 of [Jacobi 1991]. Had I been writing a PhD dissertation in analytic philosophy, I might have included comments as to the agnostic response; and I admit that it represents a logical lacuna. But I think that it has little significance for the point of the paper and in no way misrepresents or misconstrues the epistemological consequences of logical positivist epistemology. There is little cognitive distinction to be made between: “It is of no epistemic interest to ask if UR exists” and “Yes, I assume the UR exists but it is of no epistemic interest” or “No, only the phenomena of experience exist.” It is hardly a “gross misrepresentation” of the position. In fact, logical positivists have been interpreted as supporting each of the three positions.

In this discussion, [91] Vilks notices two inaccuracies in my expression of ideas which suggest stronger conclusions than might be justified:

[a] “... all modern philosophical inquiry that attempts to understand the growth in scientific knowledge has accepted” protorealism, i.e. has rejected classical idealism; and

[b] “... an explicit commitment must be made since the answers provide the metaphysical structure necessary for the development of a coherent world view.”

Sentence [a] can be beneficially modified in two ways. First, a parenthetical comment might note the positivist agnosticism while noting that the position collapses to a common answer at [1+]. Second, ‘attempts’ is too inclusive: better would be to replace that word with the phrase “is currently considered to

be a viable attempt to understand ...(etc.)” Logical positivism was an attempt, but clearly not a viable one.

Sentence [b] is admittedly incorrect as it stands. It should read “a commitment either explicit or implicit is required for the development of a world view capable of providing a complete and coherent understanding of modern science.” In fact, most philosophical positions leave their metaphilosophy (‘first philosophy’) implicit, and their commitment must be deduced from subsequent assumptions or postulates. Of course, when the commitment is made it is made without guaranteed knowledge that it is correct — for that is the fundamental property of primitive assumptions. Modern realism would go on to say that no general principle or proposition can ever be stated with certain knowledge that it is correct.

Vilks also sees a number of problems for the metaphilosophy of modern realism. [91] The first seems to be with my semantics. It seems that my statement that the phenomenological contents of our experience exist as primitive data of our existence is beyond the understanding of mathematicians. Of course, I was not intending a mathematical seminar paper — and I assumed that most readers, being motivated by epistemic sincerity, would make an attempt and have little difficulty in understanding what I meant. The same comment also applies to my use of the terms UR, human mind, existence, closeness of homomorphism, experience. I assume most readers of Methodus will not require a glossary of these terms in order to follow the meaning of the article. The context in which they were used was reasonably clear.

Vilks also indulges in a bit of spurious metaphysics in suggesting that the UR—which for the purposes of the paper was simply the causal Other that underlies our experiences—might turn out to be a number of distinct URs, apparently on the grounds of the figure of speech that “everybody has his own reality”. The UR as defined has no properties specified, so I wonder on what basis it could possibly be subdivided into distinct subdomains. And certainly, given the astounding consensus that science has established for explaining human experience (observations, measurements), there is strong support for the belief in one very extensive UR which we all share. To deny the existence of the UR (i.e. any Other than the phenomena of experience) always leaves the skeptical idealist with this vast consensus to explain, without slipping into sophomoric solipsism.

The second problem imagined by Vilks for modern realism is that the semantic form of radical fallibilism—“there are no incorrigible grounds for
knowledge.."— when applied to itself seemingly leads to the self-referential paradox that it is fallible and incorrigible grounds for knowledge exist. This ‘problem’ is nothing more than an obfuscatory quibble. Radical fallibilism is not an axiom to be used in a deductive system—it is a guiding principle for constructing a world view and presents a judgment that we shouldn’t waste our time searching for incorrigible grounds for knowledge. Modern realism demonstrates that incorrigible foundations are not necessary to philosophy in general or epistemology in particular.

The third problem attributed to modern realism refers to a general interpretation of the principle of evolutionary naturalism—that epistemology and the knowledge it generates are important factors in the long-term success of species that depend upon cognitive strategies for adaptation: "successful societies and cultures will be those whose theories are closest to the truth." That is, epistemology is important for *Homo sapiens* because the closer our belief systems correspond to the way things really are (viz. UR) the better we will be able to develop successful strategies for adaptation. Vilks would question this principle by naively applying it to short run situations that have nothing to do with epistemology and adaptation (e.g. the counterfactual, if Hitler had won WW2 it would have proven his theories were superior) or by drawing the incorrect (perverse) conclusion that any successful epistemic strategy is necessarily successful because it is closest to the truth. Vilks’ example is that Western science has utilized the hypothetico-deductive (H-D) method, Western civilization is apparently successful, ergo the H-D method is closest to the truth. I find this reasoning incoherent in substance and petty in intent.

Evolutionary naturalism is a regulatory principle that is consistent with the apparent evolution of intelligence over the very long run in biological systems and suggests why epistemology and the search for knowledge is valuable. It is not an axiom of socio-political interaction. If one wants to dispute it, a more appropriate argument would be to offer an alternative principle capable of producing the same or better insights. Alternatively, one could argue that cognitive abilities and knowledge are not significant aspects of biological evolution, but simply curious coincidences—although this view would seem to lead to the same epistemic dead end that skeptical idealism entails.

The final section of Vilks’ criticism (91-93) is intended to show that the paper misrepresented the H-D method as it is employed in economics. It begins with the observation that modern realism embraces pragmatism with respect to the choice of primitive assumptions—which, of course, in no way entails that it also embraces a pragmatic epistemology. It then suggests that since it recommends that economics as a discipline should address the problems in society and generate theories to deal with those problems, that I have somehow supported conventional deductive economic theory. I confess to seeing no possible connection between these statements; nor did the ensuing description of how axiomatic systems are defined and developed provide any logical link.

Vilks evidently believes that the H-D method is both necessary and sufficient for epistemology. He conveniently defines a deductive theory as any theory that contains within it logico-mathematical axioms that are accepted "without proof." It is not clear from this definition how empirical statements or general inductive propositions fit into such theories; for it is certainly the case that most of the complex theories of modern physics include axiomatic foundations as well as inductive principles and empirical generalities (not to mention free parameters that are selected on a pragmatic basis as in quantum chromodynamic theory). If these theories are to be termed 'deductive' simply because they employ logic and set theory, then Vilks and I are speaking a very different language.

In his discussion Vilks provides some basic mathematical definitions of axioms and their difference from ‘axioms’ that are actually assumptions—a distinction which was not included in the paper as it was not considered crucial the paper was not directed toward the philosophy of mathematics—rather it was directed toward economics where a distinction between postulates and assumptions was indicated and the axioms of logic which underwrite the use of deduction were simply taken for granted. In passing it might be noted that there is in Vilks’ discussion here a suggestion that the axioms of classical logic and set theory are foundational and incorrigible. This position is certainly not unanimously accepted amongst either philosophers of science or mathematics given the alternative logico-algebraic structure required for quantum mechanics and the plausible interpretation that logic is empirical, e.g.[Putnam 1969].

Since Vilks seems to misconstrue my intended meaning at almost every turn, I should hasten to observe that I have never denied—and certainly not in the paper—the efficacy of the basic logico-mathematical axioms that underwrite the predicate calculus. Nor have I ever doubted that the hypothetico deductive method has an important part to play in epistemology. Contrary to Vilks reading, I do not think that I have argued "against logic and set theory."
anywhere in the paper. (My oblique reference to Gödel’s theorem was simply a warning to those arch scientific realists who might imagine that logicomathematical structure is adequate to represent all that is epistemically significant.) My main point in the discussion of the H-D method in economics was that other valid methods (methodologies) are available; and that economics has tended to restrict itself to the H-D method employing mathematically convenient postulates and assumptions. The result has been the development of a dense theorem space with acknowledged application to certain domains of economic interest—but that there are economic problems that lie outside that domain which are being ignored through slavish devotion to the H-D method. I am uncertain what conclusions are to be drawn from the quotations of Robbins and Hahn (:93) relative to Vilks’ criticism of the paper. But it would appear they are intended to support Vilks’ general and, I believe, seriously mistaken belief that the H-D method is the only method permissible in epistemology.

Vilks then argues that the use of tautologies is to be recommended in epistemology on the grounds that in mathematics interesting conclusions can result from manipulating such definitional assumptions. My position is that the proper interest of epistemology is to match our interesting conclusions with UR, that is, to strive to develop theories that are not just logically true, but which can provide understanding and explanatory power of the real world. The axiom of rationality provides neither. Vilks seems to believe that this common-sense tautology provides all that is required to underwrite modern economics. Patently, this belief is misguided. Many other postulates and assumptions are required to get the theorem-production going, e.g. the assumption that individual utility functions are independent (else one loses access to linear algebra and the neat theorems which constitute microeconomics).

Vilks opines that the combination of logic, set theory and the axiom of rationality have produced “a theory one may wish to call economics.” (:93) First, I would argue that what is produced from this combination is theorem space, not a theory or body of theories. Second, I would note that there are indications of some deep problems in the epistemology which has resulted from this method:

[1] Contradictory theories proliferate (through using alternative assumptions or postulates) with no apparent means of arbitrating amongst them.

[2] There is a noticeable absence of any general or universal laws concerning human economic behavior that have not already been built into the assumptions, simply to be ‘mined’ by deduction.

[3] The inability of economics to produce possiblity structures for its domains of interest, i.e. counterfactual conditionals which provide explanations of both what is possible and impossible under given initial and boundary conditions.

Vilks is obviously sanguine about the above; my view is that they may be an artifact of slavish devotion to the H-D method and that a realist perspective that attempts to explain human economic behavior rather than merely define it may offer opportunities for economics to expand its scope and significance.

In conclusion, there appears to be nothing in Vilks’ detailed discussion which supports the strong hyperbole of his introductory general criticisms. Other than the cautionary comments on inaccuracies in the expression of a few general statements, there is little of substance in the attempts to discredit protorealism, modern realism, or its suggestions that H-D is not the only possible methodology to be considered for economics.

Notes
1. ‘On “Metaphilosophy and Methodology in Economics”’.
Methodus v.3 n.1:90-94. All references to this paper will hereafter be indicated by page numbers within parenthesis, e.g. (:2).

References