Replies and Reflections

Nicholas Maxwell

I am absolutely delighted by the contributions to this volume. The variety of topics discussed is astonishing. I am immensely grateful to all the contributors for having taken the trouble to write these fascinating and challenging essays. I am enormously grateful to Leemon McHenry for all the work he has put into editing the book, and grateful too to Michael Krausz for having suggested the book in the first place, and initiated the project to produce it.

The philosophical problem which first came to haunt me was the problem of how we can understand our human world embedded in the physical universe, aspects of which are discussed by Jeremy Shearmur, Mat Iredale and David Hodgson in Part II. Then I became embroiled in trying to understand how we could make rational sense of science, the topic of essays by Karl Rogers and Leemon McHenry in Part III. Once I had arrived at the idea that, because the basic aims of science are profoundly problematic, science needs to try to improve its aims and methods as it proceeds, I was led to generalize this idea to all of academic inquiry and, in a way, to all of life. Thus did I stumble across my "from knowledge to wisdom" argument, which is discussed in diverse ways by Copthorne Macdonald, Steve Fuller, John Stewart, Joseph Agassi, Margaret Boden and Donald Gillies in Part I. I propose to discuss the essays in this order, as it seems to be a reasonably coherent way of unfolding the themes under scrutiny.

But first I want to pay tribute to Alan Nordstrom's marvellous sonnets on wisdom. How extraordinary to find what I have been labouring to say in tome after tome expressed here so lucidly and succinctly, and with such eloquence. I am reminded of the poetry of George Herbert. Nordstrom writes with absolute seriousness from the heart: he is one of a handful of people scattered across the world who actively strive to put what I would call "wisdom-inquiry" into practice in their teaching, as far as bureaucracies and current orthodoxies permit. He has even, heroically, attempted to get his university to devote itself to wisdom.

The Human World in the Physical Universe

Jeremy Shearmur

I am enormously grateful to Jeremy Shearmur for the very generous things he has to say about my work. I too remember vividly those wonderful times we spent together with Larry Briskman in the late 60s, arguing fiercely and roaring with laughter. We thought, in those far off days that, in the fullness of time we would come together to create a university department which would do philosophy as it ought to be done: serious concern with fundamental problems at the outset; physics, cosmology, biology, economics and politics on the curriculum as well as "philosophy" as it is mostly understood these days; imaginative speculation and fierce criticism; concern for problems of living, for global problems; and lots of laughter. It never happened.

Shearmur criticizes my approach to the human world/physical universe problem, and argues in support of Popper's "three-worlds" view. Shearmur makes it all sound very reasonable. He writes: "we should initially be as metaphysically expansive as seems adequate to us to do justice to the phenomena with which we are dealing. We then attempt scientific reductions." We do not *presuppose*, in other words, that the mind can be reduced to the brain, biology to chemistry and chemistry to physics. We *attempt* to explain the mind in terms of the brain, biology in terms of chemistry, and chemistry in terms of physics, and when we are successful it is a great scientific triumph – but we don't prejudge the matter. The proper scientific attitude is not to pretend to success that has not been achieved.

Actually, as Shearmur recognizes, the issue between Popper and me does not concern reductionism. I am as much an anti-reductionist (even in principle) of the experiential to the physical as Popper is. What is at issue, rather, is that I reject Popper's interactionist, quasi-Platonic, three-words view, and the chief *argument* which Popper deploys in support of it.

Popper holds that there are three worlds: the physical universe; the psychological world of conscious minds; and a quasi-Platonic world of the *content* of theories, problems, arguments, ideas, and works of art. This last world 3 interacts with world 1 (the physical universe) via world

2 (the psychological world).[1] And in support of the existence, the reality, of this interaction, Popper argues that a world 1 event, such as the atomic explosion over Nagasaki on the 9th August 1945, cannot be explained and understood without reference to scientific theories, in particular theories of nuclear physics. The explosion would not have occurred if certain theories in nuclear physics had not been previously discovered. Here, it seems, is a decisive case of a world 3 idea influencing a world 1 event via world 2. In order to *explain* the explosion, it will be necessary to refer to world 3 entities – theories of nuclear physics – which means no purely physical explanation, referring to the physical universe only, could conceivably, even in principle, suffice.[2]

But this is not quite the knock-down argument that Popper takes it to be. Instead of refuting, it just ignores an alternative account which makes no reference whatsoever to quasi-Platonic "ideas" which, supposedly, interact with the minds of people. We may adopt *experiential physicalism*, the anti-reductionist view I have expounded in section 6 of chapter one, and have argued for in some detail elsewhere.[3] Having adopted experiential physicalism, we may take the view that two quite different *kinds* of explanation of the Nagasaki explosion are (in principle) possible. On the one hand there is the physical explanation: the true theory of everything, T, plus a precise specification of the

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[1] See K. Popper, *Objective Knowledge* (Oxford: Clarendon Press, 1972), chs. 3 and 4; K. Popper and J. Eccles, *The Self and Its Brain* (London: Springer, 1977), Part I, chs. P2 and P3.

[2] I heard Popper spell out this specific version of the argument in a lecture he gave at Imperial College London. Other versions of the argument can be found in the works cited in note 1. The atomic bomb is mentioned in this context in K. Popper and J Eccles, *The Self and Its Brain, op. cit.*, p. 47.

[3] Experiential physicalism was expounded in my first three published papers, "Physics and Common Sense", *British Journal for the Philosophy of Science* 16 (1966), pp. 295-311; "Can there be Necessary Connections between Successive Events?", *British Journal for the Philosophy of Science* 19 (1968), pp. 1-25; and "Understanding Sensations", *Australasian Journal of Philosophy* 46 (1968), pp. 127-146. It was further developed in my *From Knowledge to Wisdom*, 1st ed. (Oxford: Blackwell, 1984), pp, 181-189, 201-205 and ch. 10; 2nd ed. (London: Pentire Press, 2007), pp. 205-213, 224-227 and ch. 10. See also my *The Human World in the Physical Universe* ((Lanham, Maryland: Rowman and Littlefield, 2001), pp. 88-89 and chs. 5-8; and especially my "Popper's Paradoxical Pursuit of Natural Philosophy", in J. Shearmur and G. Stokes (eds.) *Cambridge Companion to Popper* (Cambridge: Cambridge University Press, 2008), section 7.

physical state of the earth and its environment at some time prior to the explosion, together entail a specification of the Nagasaki explosion and, in this way, explain the explosion. This provides a comprehensive, detailed explanation of all the physical phenomena associated with the explosion, but leaves everything *experiential* and *human* associated with these phenomena undescribed and unexplained (except as physical processes going on in conscious brains). For this we need to resort to *personalistic* descriptions and explanations, couched in terms of intentions, plans, desires, ideas, perceptions, feelings and actions of people. A personalistic explanation enables one to know what it would be like to *be* the other person, seeing, experiencing, thinking, feeling what the other person sees, experiences, etc. The historical account of the exploding of the bomb – discovery of the nuclear chain reaction, the Manhattan Project, the war, the political decision to drop the bomb – would be made up of, and would presuppose, a multitude of personalistic explanations. It would render intelligible the dropping of the bomb (in so far as it can be rendered intelligible) in human terms, in terms of the plans, motives and actions of people, in a way in which the purely physical explanation, however complete in its own terms, could not. This historical, personalistic account would refer to apparent world 3 entities, such as theories, propositions, and arguments, but would not require anything to exist other than what can be accommodated within experiential physicalism.[4] There are, in other words, on this view, no Popperian world 3 entities that interact with world 1 via world 2.

The crucial feature of this view is that personalistic explanations are intellectually genuine, and compatible with, but irreducible to, even in principle, physical explanations. They are irreducible to physical explanations, not because they explain physical events that cannot be (fully) explained physically, but because they explain, or render comprehensible *in a way different* from the way physical explanations explain.

Popper argues that, in order to make sense of world 1 events, such as explosions of atomic bombs, we have to acknowledge that world 3 entities – physical theories – interact with world 1 via world 2. The physical universe is, in other words, "causally open" as Popper and Shearmur put it. What this argument ignores is that the physical universe may be closed causally but *open explanatorily*. Granted experiential physicalism, physical explanations explain everything in

[4] See my "Popper's Paradoxical Pursuit of Natural Philosophy", *op. cit.*, section 7.

only a highly restricted, specific kind of way. In particular, no physical explanation of the Nagasaki explosion, however complete, could even in principle render this horrific event intelligible in human or personalistic terms. But this does not mean, as Popper supposes, that world 3 entities exist and interact with world 1 via world 2.

At one point Shearmur asserts that, according to my view "what happens in the physical world is … to be understood *purely* in physical terms". No. Physical events caused in part by human intervention could be explained in only a restricted kind of way by physical theory; in order to make human sense of such events, personalistic explanations are required, which are compatible with but not reducible to physical explanation.

It is important to note that, in order to demolish Popper's *argument*, all that is needed is the counter example of experiential physicalism as a *possibility*. It is not required to establish that experiential physicalism is *true*.

We have before us, then, two rival views.[5] Both face problems. As Shearmur points out, experiential physicalism faces the problem: How can there be authentic free will if everything occurs in accordance with physical law? Popper's view faces the severe problem: How can Darwinian evolution result in (a) the creation of a whole new realm of world 3 entities, quite distinct from the physical universe, which (b) interact with the physical universe via conscious minds? Interactionism amounts to postulating poltergeistic events in the brain, more discreet than those which hurl furniture about in horror films, but otherwise not different in kind, and just as implausible when viewed from the standpoint of modern science.

Shearmur rather glosses over the very severe difficulties that Popper's view faces, but pounces on the chief difficulty faced by my view. He says, for example "as for ourselves and our activities, and the impact that we make on the physical world, everything would have been the same, had we been zombies". My reply is that unconscious zombies that imitate human actions are, perhaps, a logical possibility, but they are not possible in fact in our world. Any beings with brains sufficiently sophisticated to create, not just atomic bombs, but the marvels that we create – great works of art, science, loving relationships, liberal democracies – must be, in our world, conscious. Purely physical

[5] I ignore, in the present context, all the other rival possible views. These are critically assessed in my *The Human World in the Physical Universe, op. cit.*, ch. 4.

explanations of these things will refer to human intentions, plans, imaginings, but only as *physical phenomena*, the physical aspects of mental or brain processes. However, I defer responding properly to Shearmur's charge that experiential physicalism cannot do justice to free will till I come to discuss free will in some detail in connection with David Hodgson's contribution below. My response to Hodgson will also be my response to Shearmur.

Despite Shearmur's critique, experiential physicalism still seems to me to be overwhelmingly superior to Popper's three worlds view.

But what are we to make of Shearmur's apparently modest proposal that we should, initially, be as metaphysically expansive as we seem to need to be, to do justice to the phenomena, and then attempt scientific reductions (thus not presupposing such reductions will succeed)? Is not this open-minded attitude eminently reasonable? I have two comments to make.

First, much depends on whether one adopts standard empiricism (SE) or aim-oriented empiricism (AOE). AOE implies that physicalism is a relatively secure part of current theoretical (conjectural) scientific knowledge. AOE thus implies that we have good theoretical scientific grounds for *not* being metaphysically, or ontologically, expansive (except in the realm of physical entities perhaps). There are, according to AOE and physicalism, no non-physical entities knocking about in the universe interacting with the physical world. SE, on the other hand, implies that physicalism is *not* a part of current scientific knowledge (since it is a metaphysical thesis that is incompatible with current accepted physical theory). Accept SE, and science becomes much more tolerant about the kinds of entity that may exist in the world.

Second, whatever scientific *theory* tells us, we must, of course, continue to be highly *critical* of it. The falsifiable, and the metaphysical (i.e. unfalsifiable) accepted theories of science must be subjected to sustained attempted criticism and falsification. This may well involve entertaining the possibility that entities exist that are prohibited by current theory. In other words, a healthy Popperian *critical* attitude can do full justice to Shearmur's – and Popper's – plea for metaphysical expansiveness, and lack of prejudgement. (This is to use Popper to refute Popper.)

Mathew Iredale

Mathew Iredale begins his lucid essay by attributing to me two points that I do indeed hold: first, that we should see philosophical problems concerning free will as a central part of the more general and complex problem of understanding how our human world, imbued with consciousness, free will, meaning and value, can be embedded in the physical universe; and second, that this latter "human world/physical universe" problem is perhaps *the* fundamental problem of philosophy, even our fundamental problem *per se*. He then expresses one or two doubts as to whether my proposed reformulation is so very different from the traditional free will/determinism problem, but goes on to consider implications that special relativity[6], evolution, neuroscience, genetics, psychological studies of rationality and manipulation (all aspects of science) have for our ideas about free will.

A few comments. In chapter 1 of this volume I suggest that our fundamental problem – philosophical, theoretical, *and* practical – might be put like this: *How can life of value best flourish in the real world?* I intend this to encompass both the practical problem "How can I, or we, realize what is of most value, potentially, in the circumstances of my, or our, life?", and the philosophical and theoretical problem of knowledge and understanding "How does our human world fit into the physical universe?". The latter is, perhaps, our fundamental problem of knowledge and understanding, but not quite our fundamental problem *per se*.[7]

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[6] Iredale says that "relativity does not appear to give the traditionalists any reason to abandon their formulation of the free will problem in terms of determinism". I would have thought one should draw exactly the opposite conclusion, for two reasons. First, in so far as relativity creates a problem for free will, this is an example of *science* creating a problem, relativity being a scientific theory. Second, relativity, in so far as it implies a space-time, "block universe" view, can be taken to imply that the future is fixed and determined; but this is what might be called "ontological determinism", quite different from the "predictive determinism" of orthodox discussions of the free will problem. Special relativity and ontological determinism are entirely compatible with the basic laws of the universe being probabilistic, predictive determinism being false.

[7] Iredale is quite right to quote me as saying, in a note, that the human world/physical universe problem is our fundamental problem. However, in the same note, I do say that it is our "fundamental problem of understanding": see my *The Human World in the Physical Universe: Consciousness, Free Will and Evolution*, *op. cit.*, p. 17, note 1.

In this age of almost lunatic specialization, it seems to me vital that philosophers – and indeed all of us – should persistently remind specialized academics that there are profoundly important general, fundamental problems that cut across conventional disciplinary boundaries, rationality requiring that there be a persistent interplay between specialized and fundamental problem-solving.[8] The primary task of philosophy is to keep alive an awareness of the existence of these fundamental problems – an awareness of the point that we cannot help but give answers to them, implicitly or explicitly, in our science and the way we think, in our institutions, our values and the way we live, our answers all being more or less inadequate and, as a result affecting adversely our thought and lives. If philosophers can also encourage imaginative and critical attempts to improve answers to our fundamental problems, so much the better. Philosophy betrays its central task – betrays reason and humanity – if it becomes, what it mostly is these days, yet another specialized discipline alongside other such disciplines.

A part of the reason for formulating the free will problem in such a way that it becomes an important component of the more general "human world/physical universe" problem is that this clarifies the nature of the problem, brings to the fore aspects of the problem that may otherwise be neglected, and may even be required as a first step towards solving the problem. Iredale, as we have seen, indicates a number of ways in which science has implications for our ideas about free will which have nothing to do with determinism – implications which may be missed if attention is restricted to determinism. But there are additional points to be made.

The two formulations we are concerned with are:

The determinism formulation: How can we have free will if determinism is true?

The science formulation: How can we have free will – a vital aspect of life of value – if what science tells us about the world is true?

Given the science formulation it is at once clear that a major part of the problem is to decide what we should take science to be telling us about the world. This will depend, in part, on what philosophy of science we accept. Given standard empiricism, it is not clear what science does tell us about the ultimate nature of physical reality.

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[8] This is the message of my "Science, Reason, Knowledge and Wisdom: A Criticism of Specialism", *Inquiry* 23 (1980), pp. 19-81.

Granted aim-oriented empiricism,[9] however, science tells us that we live in a *physicalistic* universe – physicalism being a rather secure part of scientific knowledge.[10] The free will problem becomes all the more severe – physicalism placing harsh constraints on the possibility of free will.

Again, given the science formulation, it is clear that a major part of the problem is to understand how there can be free will if the real explanation for all our thoughts, decisions and actions is a purely *scientific* one, indeed a purely *physical* explanation. If we are to have free will, surely rationalistic or personalistic explanations of what we decide and do must be true, must be couched in terms of our desires, thoughts, decisions, reasons for action, and must make these responsible for our actions in the kind of way required for free will. But how could such rationalistic, freedom-ascribing explanations of human actions be true if purely physical explanations suffice to predict and explain, in principle, everything that goes on? Could both kinds of explanation be true simultaneously? These issues concerning explanation become crucial to the free will problem.

Putting the free will problem into the context of the "human world/physical universe" problem brings two other matters to the fore. The first of these is that the free will problem cannot sensibly be tackled independently of the mind/body problem. The way the problem is formulated and the kind of answers to be sought will differ profoundly depending on whether some kind of dualist position is adopted, or a view which identifies the mind with the brain. The second point is that ideas about free will cannot be separated from ideas about what is of value. In part this is because free will is something we, correctly, judge to be inherently of value. Free will may be given a range of meanings, some perhaps more obviously compatible with determinism or physicalism than others. The question that really matters is this: In how *valuable* a sense of free will do we, or can we, have it given determinism, or physicalism? Values also arise in connection with

[9] For characterizations of standard and aim-oriented empiricism, see this volume, chapter 1, section 4.

[10] Physicalism, as understood here and in what follows, is the thesis that the universe is physically comprehensible, it being such that the yet-to-be-discovered true physical "theory of everything" is *unified*. Physicalism is of course a conjecture, like all items of theory in physics; it is, however, according to aim-oriented empiricism, more secure than even our best physical theories – quantum theory or general relativity.

questions about the authentic self. One way in which one's free will can be diminished is to have one's authentic self taken over by a false self as a result of brainwashing, indoctrination, or becoming in thrall to another. But how is the authentic self to be distinguished from a false self? At some point value judgements must be brought in to answer this question. A person is his real, authentic, true self if what is best, of most value, in this person's character, is in command. Formulating the free will problem as a part of the "human world/physical universe" problem rather naturally brings these value aspect to the fore, as this latter problem is centrally concerned with questions about how what is of value can exist (and flourish) embedded in the physical universe.

There is a further point. So far reasons have been given for reformulating the traditional free will/determinism problem so that "determinism" is replaced with "physicalism". But grounds can also be given for replacing "free will" with "wisdom" – conceived of as the capacity and active desire to realize what is of value in life, for oneself and others. This yields a stronger, more fundamental version of the problem, as long as we hold free will to be a necessary component of wisdom. It could of course be argued that what is of value may be achieved by someone acting compulsively, and thus without free will. A person might be a compulsive philanthropist, perhaps, or a compulsive creator of great art or science. But let us demand, by fiat as it were that, for wisdom, realization of value must be done freely. In this case, the wisdom/physicalism problem encompasses the free will/physicalism problem, in that solving the former solves the latter as well, but not *vice versa*. It is of course true that we may act freely but not wisely; nevertheless, if we can solve the problem of how wise acting is possible given physicalism, we can almost certainly solve the problem of how free, unwise acting is possible.[11] This wisdom reformulation is rather natural to adopt granted that one takes the "human world/physical universe" problem as fundamental.

The point is now this. Formulate the free will problem as an integral part of the more fundamental "human world/physical universe" problem,

[11] It might be objected that the free will/physicalism problem is already hard enough; why should we embed it in the even more difficult, severe problem of how wise acting is possible given physicalism? The answer is that we need to be clear about the problems that confront us, and sometimes embedding a problem in a more fundamental one, in this way, provides the key to solving the problem. I believe this is the case here.

and it is more or less obvious that the above considerations, having to do with the philosophy of science, the conflict between different explanations, the mind-body problem, values, and wisdom are all highly relevant to solving the problem. Formulate the problem as a question about free will and *determinism* however, and it is not clear that any of the above considerations are relevant.

Iredale makes the excellent point that a Darwinian perspective on free will implies that what free will we have today must have evolved gradually, its early manifestations existing, no doubt, in pre-human mammalian life. This Darwinian viewpoint speaks strongly on behalf of compatibilism. It is difficult to see how incompatibilist free will could have emerged gradually. The view would have to be, presumably, that early life was devoid of any hint of incompatibilist free will until, abruptly, at some instant, incompatibilist free will burst upon the scene – something of a miracle for ordinary Darwinian mechanisms of evolution to engineer!

Iredale also stresses that this gradualist Darwinian view implies that we possess only a certain amount of free will, it being possible, no doubt, for us to increase the degree that we possess. A fundamental problem may be that of discovering how we can learn to increase our free will. I would only add that there is here, in my view, an additional reason for concentrating on *wisdom* rather than *free will*. Enhancing wisdom increases – by definition – our capacity to realize what is of value, whereas enhancing free will does not. (What I take to be a really urgent and fundamental problem "*How can humanity learn a bit more wisdom?*" turns out to be much more closely related to the free will problem than one might at first sight realize.)

David Hodgson

I am immensely grateful to David Hodgson for the generous things he says about my book *The Human World in the Physical Universe*. Hodgson and I agree about much. But there is one fundamental point of disagreement: unlike me, Hodgson holds that free will and (experiential) physicalism are incompatible. Hodgson produces some searching criticisms of my compatibilist views which I shall do my best to rebut.

Hodgson argues that I have not shown that science has established the truth of physicalism. I agree. All theoretical knowledge in physics is conjectural. What I do claim to have shown is that physicalism is a basic tenet of scientific knowledge, more secure than any physical

theory (but still a conjecture). The grounds for accepting physicalism, in essence are, first, that persistent preference given to unified physical theories means that physics makes a persistent metaphysical assumption concerning unity and, second, given that *some* such assumption must be made, the best one to adopt is the one (a) which accords best with the thesis that the universe is perfectly comprehensible in some way or other, and (b) which is the most empirically fruitful, in the sense that it promises to support the most empirically progressive research programme, and has actually supported the most empirically progressive research programme.[12] I argue that the thesis which best satisfies (a) and (b) is physicalism.[13] Hodgson supports a rival dualist, interactionist metaphysical thesis which might be called physicalism(H). Hodgson argues that physicalism(H) accords just as well with modern science as physicalism does, the former differing from the latter only in asserting that some physical processes occurring in conscious brains are influenced by "the non-physical aspect of reality".

But physicalism(H) fails quite drastically to satisfy requirement (a) for acceptability. Any "theory of everything", T(H), compatible with physicalism(H), must be vastly more complex, disunified, and thus non-explanatory, than any "theory of everything", T, compatible with physicalism. For, unlike T, T(H) will have to specify extraordinarily complex physical states of affairs – states of conscious brains – that constitute the conditions under which the non-physical interacts with the physical. Or, put another way, in order to specify physical conditions under which that part of T(H) that constitutes a purely *physical* theory fails to yield correct predictions (because of non-physical interactions), extraordinarily complex physical states of affairs will have to be specified. This means that T(H) must be vastly less explanatory than T, and thus horribly at odds with the thesis that the universe is perfectly comprehensible *in some way or other*. Whereas T asserts that the same laws govern all phenomena everywhere, the physical part of T(H) asserts

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[12] For a more detailed explication of empirical fruitfulness along these lines see my *The Comprehensibility of the Universe* (Oxford: Oxford University Press, 1998), pp. 178-179; and my *Is Science Neurotic?* (London: Imperial College Press, 2004), p. 156.

[13] See my *From Knowledge to Wisdom*, 1st ed. (Oxford: Blackwell, 1984), pp. 218-230; 2nd ed. (London: Pentire Press, 2007), pp. 241-253, 358-360 and 400-430. See also my *The Comprehensibility of the Universe*, *op. cit.*, chs.3-5; and *Is Science Neurotic?*, *op. cit.*, appendix, section 6.

that the same laws govern all phenomena everywhere *except for some extremely complex phenomena occurring in conscious brains*.

It seems to me doubtful, too, that physicalism(H) satisfies (b) as well as physicalism does. Were we to allow that it does, we would have to allow, too, it would seem, that endlessly many other theses analogous to physicalism(H) satisfy (b) as well as physicalism does – theses which postulate that, in addition to the physical world, there is a non-physical one which interacts with physical states of affairs but in a way which has not yet been observed. This would include theses which postulate that such interactions occur in the future, and are so powerful that tables and chairs are pushed around, even mountains, planets and stars. Once we allow that views such as these are as empirically fruitful as physicalism (in the sense indicated above), the whole idea of *any* version of physicalism being empirically fruitful seems to disappear.

This point can be made in a slightly different way as follows. Once it is regarded as scientifically acceptable to hold that there is a non-physical world interacting with the physical one then, whenever physical phenomena are discovered that are inexplicable on the basis of current physical theory, it would always be scientifically acceptable to hold that, here, the non-physical world is interacting with the physical world. This would stultify the search for *physical* explanations for the recalcitrant phenomena. What this shows, decisively in my view, is that Hodgson's view – physicalism(H) – is *not* as scientifically acceptable as physicalism. Far from being scientifically fruitful, if accepted it would render equally acceptable analogous views which would stultify scientific progress. (A number of views analogous to physicalism(H) – such as versions of vitalism – have been put forward at various times: none have proved empirically fruitful.)

I might add that *if* physicalism is true, then all dynamical theories which apply only to restricted ranges of phenomena must be false. It follows at once that if physics proceeds by developing a succession of theories, each of which applies to a greater range of phenomena than its predecessors, but none of which applies to all phenomena, then physics will proceed from one false theory to another. That physics has developed in this way does not in any way undermine the idea that physicalism may be true (as Hodgson seems to suggest at one point). On the contrary, this is just the way physics ought to make progress, if physicalism is true.

Hodgson goes on to argue that there are grounds for holding that physicalism is false. His argument, if I have understood it, might be boiled down to this. Reasoning involved in doing physics is not algorithmic. But if physicalism is true,[14] it must be algorithmic, since thought processes involved in developing a new physical theory would all occur in accordance with precise physical law. Hence physicalism is false.

What is wrong with this argument is the idea that if physics is done in a physicalistic universe then all reasoning involved must be algorithmic. Consider Einstein pondering the problems that led him to formulate special relativity. We might simplify things drastically, and assume it took him two hours to work it out (actually he pondered the issues for some ten years). And again to simplify things, let us assume deterministic physicalism. Then, given a precise specification of the relevant initial conditions, IC, plus the true deterministic physical "theory of everything", T, a physical specification of the marks on paper produced when Einstein wrote down the special relativity follow logically. Does this mean that the discovery of special relativity can be specified by means of an algorithm? Not at all. This predictive task would, in practice, be utterly impossible to perform. First, it would be quite impossible to obtain IC – which would have to include a specification of the physical state of Einstein's brain and body, and his environment (strictly speaking including a region of two light hours distance). IC would have to be obtained with absolute precision; the slightest inaccuracy in the specification of the state of a molecule in Einstein's brain would be liable to throw the whole prediction out. The instantaneous states of even the simplest physical systems cannot be known with absolute precision. Specifying the state of Einstein's brain is entirely out of the question. Secondly, solving the equations of T for such a system is forever out of the question. It is very likely that, if ever we do discover the true theory of everything, we will only be able to arrive at approximate solutions to the equations for even the simplest of

[14] Hodgson argues that if physicalism is true there can be no free will. Elsewhere I have argued almost the opposite: if physicalism is true, we have strong grounds for holding we do have free will. For if physicalism is true, then the highly worthwhile project of theoretical physics has met with great success. But we should think of free will as the capacity to achieve what is of value. But if physicalism is true, we have achieved something of great value, what has been achieved in physics: hence we must have free will. See my *From Knowledge to Wisdom*, *op. cit.*, 1st ed., pp. 273-274; 2nd ed., pp. 294-295.

systems. As someone wittily observed, given Newtonian theory there is no general solution to the three body problem, given classical electrodynamics the one body problem poses problems, and for quantum electrodynamics even the zero body problem cannot be precisely solved. As physical theories have encompassed broader and broader ranges of phenomena, their equations have become ever more nearly impossible to solve.

An algorithm is a mechanical procedure for solving a problem. And a mechanical procedure is something which "can be given in a finite set of instructions which are executed in a stepwise manner, without appeal to random processes or ingenuity".[15] Not by the remotest stretch of the imagination could processes involved in predicting Einstein creating special relativity be regarded as algorithmic. No finite set of instructions are possible which would suffice to perform the predictive task. Endless infinities would be involved in obtaining the precise state of a molecule, let alone the state of a neuron, or of Einstein's brain. Endless infinities would be involved, too, in solving the relevant equations. Any attempt to get information about the state of Einstein's brain would disrupt and damage what was being sought. And in addition, the physical prediction, if it could be made, would predict the marks made by Einstein on paper, but not what these marks can be interpreted to assert, namely special relativity. There are, in short, endless reasons for holding that predicting Einstein discovering special relativity, given deterministic physicalism, is not algorithmic.

Hodgson does, however, have another argument. Doing physics requires consciousness. But if physicalism is true, "conscious experiences can't make a positive contribution to determining what happens, beyond that made by associated physical events". Hence physicalism must be false.

But what this argument fails to take into account is that (according to the view I uphold) relevant physical processes going on in Einstein's brain *are* (contingently identical to) Einstein's conscious experiences.[16] Special relativity requires consciousness for its production (let us assume). Some of the physical processes that lead to marks on paper

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[15] T. Honderich, ed., *The Oxford Companion to Philosophy* (Oxford: Oxford University Press, 1995), p. 21.

[16] See my *The Human World in the Physical Universe, op. cit.*, appendix 2, for a refutation of Saul Kripke's claim that such a contingent identity (with rigid designators) is not possible.

that can be correctly interpreted to constitute the formulation and discovery of special relativity are conscious experiences, and if they were not, special relativity would not have resulted.

But – Hodgson may protest – if it is the physical processes that result in the writing down of special relativity, what possible role can consciousness have? Consciousness can add nothing to the physics!

To this I reply: but the physics, structured and functioning in the way that it is in Einstein's brain, *is* consciousness. Hodgson's objection may well be lethal when directed against epiphenomenalism, since that view distinguishes brain processes and conscious experiences and gives no causal role to the latter. But that is not my view. I defend a version of the brain process theory according to which conscious experiences *are* (contingently identical to) brain processes, and not something distinct from them. What we are aware of, when we are aware of our inner experiences, is what might be termed the *control* aspect of processes going on in our brains – the role these processes have in controlling, or guiding, our actions. The control aspect operates by means of the physical or causal properties of these processes, and the causal and structural properties of the brain in which they occur. Einstein's perceptions, thoughts, desires, intentions, beliefs, decisions to act, are all brain processes – ultimately *physical* processes: what he is aware of is the *control* aspect of these processes – the capacity of these processes to produce his actions. Thus Einstein's conscious experiences, being physical processes, have a direct, causal role in producing special relativity, and Einstein is, in a sense, aware of this causal role, in a somewhat opaque way, in being aware of the control aspect of the relevant brain processes.[17]

Finally, Hodgson argues that experiential physicalism (the view I defend) faces a fatal difficulty when it comes to evolution – a difficulty his interactionist view effortlessly overcomes. Actually, I think the matter is all the other way round. It is Hodgson's view, not mine, that faces a severe problem when confronted by evolution.

Natural selection operates on the capacity to survive and reproduce. This capacity, as far as animals are concerned, has a lot to do with the way animals *act* in their given environment. But if physicalism is correct, action is produced by physical processes occurring in the brain. It is this that natural selection would operate on, not any subjective sensations, desires or feelings that may accompany these physical

[17] See *ibid*., chs. 5, 6 and 8. See also my *Cutting God in Half* (forthcoming), ch. 7.

processes. Hence there can be no Darwinian explanation for the evolution of sentience and consciousness.

From my point of view, this argument fails for the same reason that Hodgson's argument above fails. It might be successful against epiphenomenalism, but it does not work against the version of the brain process theory I wish to defend. Natural selection operates on the *control* aspects of the brain: this is what matters, in that it is this which produces and guides *action*. But sentient and conscious aspects of brain processes *are* control aspects. They are control aspects that have acquired these amazing features of sentience and consciousness – but in becoming sentient and conscious they do not in any way whatsoever lose their control status or character. Thus natural selection, in operating on control aspects of brain processes operates too on those control aspects that happen to be sentient and conscious. Once one appreciates that conscious inner experiences *are* (contingently identical to) brain processes, the difficulty Hodgson sees disappears. I must add, however that, in my view, something like a Darwinian explanation of the evolution of sentience and consciousness is only possible if Darwinian theory is modified to include the evolution of the *mechanisms of evolution*, so that, as evolution proceeds, purposiveness is gradually incorporated into these mechanisms.[18]

The interactionist view that Hodgson upholds does, however, in my view, face a serious problem in connection with evolution – as I have already indicated in my response to Iredale's essay.

I want to conclude my discussion of Hodgson's contribution by emphasizing again that, despite our sharp disagreement about free will, there is much that we agree about, over a wide range of issues. I am highly appreciative of the clarity and generosity of his exposition of my views, and the cogency of his criticisms, which have forced me to think again about how free will is possible if physicalism is true.

Philosophy of Science

Karl Rogers

Karl Rogers gives a terrific brief account of my "philosophy *for* science", as he puts it, but then makes a number of remarks that I find puzzling.

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[18] See *ibid*., ch. 7.

At one point he suggests that, for me, the task of philosophy of science is to explain "scientific success and progress". This is too strong. Rather, I take the task to be – as Rogers also reports – to explain how scientific progress is *possible*. (It seems impossible either because we demand the impossible of science – such as that it delivers verified knowledge – or because we assume unworkable methods for science – such as that theories be judged solely on the basis of empirical success and failure.) In my first work on scientific method (published long ago in 1972), I formulated the task of the methodologist like this. He must "specify (a) a fundamental aim, or group of aims, for science, and (b) a set of methodological rules …[and] then assert:-
(1) The specified aim is the most worthwhile aim for science that is, as far as we know, in principle realizable.
(2) The specified methodological rules give us the best hope of realizing the specified aim."[19]

This could be improved but is on the right lines. Even if aims and methods are specified that lead to acceptable propositions (1) and (2), this falls short of explaining "scientific success and progress".

Rogers goes on to refer to my "uncritical acceptance of 'the empirical success' of science". I balk a little at "uncritical", having launched, over the years, a number of criticisms of science, from neuroscience,[20] aspects of the whole endeavour[21], to orthodox quantum theory.[22] The latter, I have argued, is not just false; it is unacceptable as a theory, because of its severe disunity. I have argued that we have good grounds for holding all fundamental physical theories are false (as I have already indicated in

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[19] N. Maxwell, "A Critique of Popper's Views on Scientific Method", *Philosophy of Science* 39 (1972), p. 133. For an improved formulation see my "The Rationality of Scientific Discovery", *Philosophy of Science* 41 (1974), pp. 139-140.

[20] See my "Methodological Problems of Neuroscience", in D. Rose and V.G. Dobson (eds.), *Models of the Visual Cortex* (Chichester: Wiley and Sons, 1985), pp. 11-21.

[21] See my *What's Wrong With Science?* (Frome: Bran's Head Books, 1976); *From Knowledge to Wisdom, op. cit.; Is Science Neurotic?, op. cit.*

[22] Papers of mine that criticize orthodox quantum theory (and seek to develop a better version of the theory that differs empirically from the orthodox version) stretch over 36 years, from the first, "A New Look at the Quantum Mechanical Problem of Measurement", *American Journal of Physics* 40, 1972, pp. 1431-1435, to the most recent, "Is the Quantum World Composed of Propensitons?", in M. Suárez (ed.) *Probabilities, Causes and Propensities in Physics,* (Boston: Synthese Library, 2008).

my response to Hodgson). I hold that all scientific knowledge is irredeemably conjectural in character. And I have been highly critical of what "the empirical" should mean, at the most fundamental level, in social science, in that I hold social inquiry should take human experience, what we enjoy and suffer, as basic in assessing proposals for action. So I don't think it is quite right to say I accept that science is empirically successful *uncritically*, or that I "preclude any possibility of criticism of the nature of 'the empirical' and the criteria for 'empirical success'".

I am puzzled, too, by Rogers' assertion that I assume standard empiricism in my "interpretation of the nature of 'the empirical'". Quite to the contrary, I have made it abundantly clear, in a number of places that, in my view, metaphysical assumptions are implicit in empirical assertions of science (which contradicts standard empiricism). For example, the point is stated quite clearly on pages 209-210, and on page 270, note 8, of my *The Comprehensibility of the Universe*. Elsewhere I declare "Even humble particular statements about our immediate surroundings contain presuppositions about the entire cosmos".[23]

These misunderstandings vitiate Rogers' subsequent criticisms of my views.

Rogers correctly says that I agree with Duhem and Popper in holding that laws and theory are implicit in the interpretation of observational and experimental results. What he does not say, however, is that I have also criticized Popper on just this point.[24] It is important to appreciate that we always have the option of weakening the theoretical or metaphysical assumptions implicit in empirical assertions. "This is copper wire" presupposes that this object has certain dispositional properties (being a good conductor or electricity, etc.) but does not necessarily have to assume universal laws to the effect that all pieces of copper everywhere, at all times, have these properties. In testing laws and theories it is important that we can weaken theoretical, metaphysical

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[23] See my *Is Science Neurotic?, op. cit.,* p. 217. I there declare "'I can walk across a room' presupposes that nowhere in the entire universe is an explosion even now occurring of unprecedented force which will spread with nearly infinite speed to engulf the room before I can take a step". I point out that similar considerations apply to the assertion "This piece of copper wire will continue to behave as copper wire for the next few minutes", the kind of assertion all scientific experiments have to assume to be true.

[24] See my "A Critique of Popper's Views on Scientific Method", *op. cit.*, pp. 143-145;

and cosmological presuppositions of empirical assertions, so that we do not presuppose the very law or theory we are seeking to test, or more than is necessary. Rogers argues that experimentation in physics presupposes the seven points of what he calls "mechanical realism". This strikes me as unacceptability substantial, and indeed close to physicalism. (I am not sure how physicalism could be false but mechanical realism true.) A quite basic idea behind aim-oriented empiricism is that it is important that we have available increasingly *insubstantial* presuppositions, so that more substantial ones can be critically assessed, and not just dogmatically presupposed. This is important because our more specific presuppositions are quite likely to be false, and in need of modification. Experimental physics becomes somewhat irrational and dogmatic if it must presuppose, and cannot question, mechanical realism.

Rogers is probably correct in holding that much physical theory, such as nuclear physics, is most severely tested in the laboratory, but it is important to appreciate that even nuclear physics can be tested by, for example, astronomical observation. A recent example was the observation that the sun seemed to be emitting too few neutrinos, which led some physicists to question whether the relevant physics is correct, but which was eventually resolved by the discovery that neutrinos have mass.[25] But even if most physical theory is primarily tested and corroborated in the laboratory, this does not mean that special assumptions, such as mechanical realism, are required to apply physical theory to phenomena beyond the laboratory – in addition, that is, to the hierarchy of assumptions of aim-oriented empiricism involved in the rejection of empirically successful, *ad hoc* variants of the theories we accept.

Rogers goes on to declare that I "should recognise that different sciences should have different conceptions of 'the empirical' and, correspondingly, explore different phenomena in accordance with different conceptions of theory and practice. [I] should recognise that social sciences have developed distinct methodologies, often independently of those of the natural sciences, including historical descriptions and explanations, with their phenomenological, sociological, and hermeneutic dimensions. Even in the natural sciences

[25] Because they have mass, neutrinos, on their way to earth from the sun, oscillate from one type to another, unobservable type: hence the low observed flow of neutrinos from the sun.

there are differing presuppositions about the nature of 'the physical' and 'the empirical'". I cannot help but think that I do recognize these things, and have said them in my publications. Thus, in *Is Science Neurotic?* I stress that different branches of the natural sciences have different, problematic aims, assumptions and methods, and thus diverse characters, all of which can, however, be encompassed within the broad framework of aim-oriented empiricism: see pages 41-47. I have long argued that the social sciences urgently need to be transformed so that they cease to be, primarily, sciences or disciplines seeking knowledge, and become instead, with the humanities, that branch of inquiry concerned to help humanity tackle its problems of living in increasingly cooperatively rational ways. This would involve, too, as a long-term project, helping humanity build aim-oriented rationality into our social world, into institutions other than science. The pursuit of knowledge within social inquiry would become a secondary matter. All this would involve a dramatic change in the nature of social inquiry – and a change, too, in what "experience", or the "empirical" means, as I have already mentioned. Social inquiry, instead of being, primarily, science, or the pursuit of knowledge, would become *social methodology*, or *social philosophy* (although this would, of course, include acquisition of relevant knowledge and understanding).

In the last paragraph of his essay, Rogers casts doubts on the possibility of there being a unified conception of inquiry, in view of "incommensurable standards between different specialisations of natural or social sciences" and "in the absence of universal agreement on how to resolve questions of value". But here, in my view, Rogers seriously underestimates the capacity of the meta-methodological framework of aim-oriented rationality and wisdom-inquiry to accommodate, and help resolve conflicts between, rival views about aims and methods, facts, values, philosophies of life. The whole idea of aim-oriented rationality is to create a framework of relatively unspecific, unproblematic aims and methods within which rival, much more specific, problematic aims and methods may be assessed and improved, as we act and live. Far from being "totalitarian", as Rogers suggests, on the contrary aim-oriented rationality provides us with the best hope of resolving our conflicts in just and cooperative ways.[26]

[26] For discussion of the vital role wisdom-inquiry might have in helping to resolve conflicts in just and cooperative ways see my *From Knowledge to Wisdom, op. cit*.,

Leemon McHenry

Leemon McHenry, in his interesting and provocative essay, sets out to assess the relative merits of Popper's philosophy of science, and mine. He argues that the difference between Popper's *falsificationism*, and my *aim-oriented empiricism* is that, whereas Popper holds that metaphysical assumptions are "held unconsciously" in the minds of scientists, I hold they form an important part of scientific knowledge. McHenry says that I am "correct in [my] assessment of the tension in Popper's late theory, but this alone does not mean that [Popper] has failed to say what progress is or means". McHenry acknowledges that, unlike Popper, I go on to argue that values play a role in science, and science should contribute to rational inquiry devoted to promoting wisdom, but then wonders how these further aspects of my views are related to my philosophy of science of aim-oriented empiricism (AOE). I take these points in turn.

I am not altogether happy with the way McHenry draws the distinction between Popper's philosophy of science and mine. Much that he says is correct, but he does not pin down the exact point where Popper and I part company, and he does not spell out adequately all the differences that arise as a result, between Popper's view and mine.

McHenry quotes Popper as holding that "the aim of science [is] to find satisfactory explanations". One might think that this makes Popper's view rather close to mine, in that I too hold that science seeks explanatory truth. But there is, here, a fundamental difference.[27] Whereas I hold that, if we are honest, we have to see science as seeking truth *presupposed to be* (more or less) *explanatory*, there being a substantial, highly problematic metaphysical thesis concerning the

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1st ed., pp. 81-91, 109-110 and 189-199; 2nd ed., pp. 95-104, 121-122, 149-153, and 213-222.

[27] It is vital to appreciate that "aims of science" is highly ambiguous. It may refer to aims *in the context of discovery*, or to aims *in the* (so-called) *context of justification* – the context in which decisions are reached as to what does, and does not, constitute scientific knowledge. In what follows it is always "aims in the context of justification" that is being discussed – although when Popper declares "explanation" to be the aim of science it is not quite clear which context he has in mind. An assumption made in the context of discovery is not a part of scientific knowledge, whereas one made in the context of justification *is*. The distinction between the two contexts is usually attributed to Reichenbach, although Popper made the distinction before him: see my *From Knowledge to Wisdom*, *op. cit.*, 1st ed., pp. 22-23; 2nd ed., pp. 33-34

comprehensibility of the universe implicit in this basic aim of science, Popper would reject this entirely. There can be, for Popper, no metaphysical assumptions, implicit or explicit, in the aims of science. Right to the end of his life, Popper held onto his "principle of demarcation" which excludes metaphysics[28] from science.

Popper always held, as I do, that *two* considerations govern choice of theory in science, having to do with (1) empirical success or failure, and (2) simplicity. In chapter VII of *The Logic of Scientific Discovery* Popper identifies simplicity with falsifiability. If this identification was valid then, in persistently accepting *simple*, or *explanatory*, theories scientists would merely be accepting theories of high *falsifiability*. In this case, the aim of seeking *explanatory truth* makes no presuppositions at all; the aim reduces to that of seeking *highly falsifiable truth* or *truth per se*. But simplicity cannot be identified with falsifiability. One can easily increase the falsifiability of a theory by adding on independently testable hypotheses otherwise unrelated to the theory, in this way drastically decreasing the unity, the simplicity, of the overall theory.

Subsequently, and perhaps because he realized his earlier view is untenable, Popper made a substantial addition to this account of simplicity. As McHenry points out, Popper proposed that, in addition to being highly falsifiable, a "new theory should proceed from some *simple, new, and powerful, unifying idea* about some connection or relation … between hitherto unconnected things or facts … or new theoretical entities". As I shall explain in a moment, my view is that, in only accepting theories that satisfy this "requirement of simplicity" (as Popper calls it), even though endlessly many rival theories can always be concocted which satisfy purely *empirical* considerations much better, but which fail to satisfy this requirement of simplicity, scientists in effect make a big, implicit (metaphysical) assumption about the nature of the universe. But Popper does not draw this conclusion. And nor can he without destroying his whole philosophy of science, founded as it is on the principle of demarcation. Thus even after enunciating the new "principle of simplicity", Popper continues to hold that science seeks truth *without making any presupposition*. Seeking "satisfactory explanations" does not mean, for Popper, that science presupposes that explanations exist to be found. Adopting the new "principle of simplicity" as a methodological principle, and yet denying that this

[28] Here, as elsewhere, I follow Popper in taking "metaphysical" to mean "empirically unfalsifiable".

means science makes some kind of presupposition concerning the simplicity of nature, is the "tension" that McHenry says I have disclosed in Popper's later work.

McHenry declares in the second paragraph of his essay that I hold that we need to see science as making "a commitment to metaphysical principles underlying our notion that the universe is comprehensible". But this rather puts the cart before the horse. What is crucial, for me, is the argument, alluded to above, in support of this contention. If physics only accepts theories that are *unified*, even though endlessly many empirically more successful *disunified* rivals can always be concocted, then physics must be making a persistent, substantial assumption, at least to the effect that the universe is such that all *disunified* theories are false. Physics must be assuming, implicitly, that there is some kind of underlying *unity* in nature.

What McHenry never quite gets round to saying is that it is this *argument*[29] which, fundamentally, divides me from Popper. I accept the argument as valid; Popper never did.

One immediate implication of taking the argument to be valid is that Popper's demarcation requirement must be abandoned. Metaphysics is an integral, central, fundamental part of scientific knowledge. McHenry is quite clear on this point. What he does not mention, however, is that there are important further implications, for the philosophy of science, and for science itself. What emerges differs from Popper in at least the following sixteen respects.[30]

(1) A basic aim of science is transformed, and becomes deeply problematic. Instead of seeking truth *per se*, in the so-called "context of justification", science seeks truth *presupposed to be explanatory*. A metaphysical thesis asserting that the universe is (more or less) comprehensible, in some way or other, is implicit in this aim. But this

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[29] The argument was first spelled out by me in 1974 in my "The Rationality of Scientific Discovery: Part I", *Philosophy of Science* 41 (1974) pp. 127-136. It has been reformulated and amplified many times since: see ref. 30.

[30] These 16 features of AOE have been spelled out by me in a number of publications over the years: see my "The Rationality of Scientific Discovery: Parts I and II", *Philosophy of Science* 41 (1974), pp. 123-153 and 247-295; *What's Wrong With Science?* (Hayes: Bran's Head Books, 1976); *From Knowledge to Wisdom*, *op. cit.*, 1st ed., chs. 5 and 9; 2nd ed., chs. 5, 9 and 14; "Induction and Scientific Realism", *British Journal for the Philosophy of Science 44*, 1993, pp. 61-79, 81-101 and 275-305; *The Comprehensibility of the Universe*, op. cit.; *Is Science Neurotic?*, *op. cit.* chs. 1 and 2, and appendix.

thesis is almost bound to be false. It is vital (for good Popperian reasons) that the thesis is made explicit within the context of science so that it can be criticized, so that alternatives can be developed and assessed, in the hope of improving the thesis. We may hope that this basic aim, being problematic, will be improved, as science proceeds. (For Popper, the basic aim is fixed, and does not evolve.)

(2) As the aim, and associated metaphysical assumption, is improved, so associated methods (specifying non-empirical simplicity and unity considerations) improve as well. The aims and methods of science *evolve* with evolving knowledge. (For Popper, aims and methods are fixed.[31])

(3) There is a two-way interplay between improving scientific knowledge, and improving aims and methods. The task of improving aims and methods becomes an integral part of science itself. (For Popper, ideas about aims and methods are not falsifiable, and thus not a part of science. They do not interact and evolve with science.)

(4) Philosophy of science, in so far as it is about aims and methods, is an integral, influential part of science itself. (For Popper, philosophy of science, not being falsifiable, is not a part of science.)

(5) Not only do the methods of science evolve over time with evolving aims, and evolving assumptions implicit in these aims. Furthermore, according to AOE, different natural sciences, having different specific aims and assumptions at any given time, quite properly and rationally have different specific methods, within the overall framework of AOE. (For Popper, because basic aims are fixed, methods remain unchanged through time and across scientific disciplines.)

(6) Science, as a result of including metaphysics, aims and methods and philosophy of science, becomes rather more like natural philosophy, as in the time of Newton, rather than science today. (Despite enthusiastically supporting natural philosophy, Popper's main

[31] But is this correct? In his contribution to this volume, Shearmur quotes Popper as writing, in the Preface to the 2nd edition of his *Conjectures and Refutations*, that "our system of aims not only *changes*, but it can also *grow* in a way closely similar to the way in which our knowledge grows". I possess only a very battered first edition of the book. Intrigued, I rushed out to check. Popper is here speaking of aims in general, not specifically of *scientific* aims. He could not acknowledge that the aims of science (in the context of "justification") change in the way specified by AOE because that would involve acknowledging that scientific knowledge includes metaphysical theses, which in turn demands the abandonment of Popper's demarcation criterion – something Popper was never prepared to do.

contribution, his demarcation requirement, splits off science from metaphysics, etc., and thus serves to sabotage natural philosophy, and turn it into *science*.[32])

(7) As scientific knowledge improves, our knowledge about how to improve knowledge (our view about aims and methods) improves as well. There is something like positive feedback between improving knowledge and improving knowledge about how to improve knowledge – something which helps to account for the explosive growth in scientific knowledge. This positive feedback feature may well be regarded as constituting the nub of scientific rationality. (For Popper, there is no such positive feedback: scientific rationality is understood in terms of the Popperian principles of conjecture and refutation.)

(8) Scientific knowledge and discussion takes place at least three levels: (i) the empirical, (ii) theory and (iii) aims and metaphysical assumptions. (For Popper, there are only the two levels, (i) and (ii).)

(9) In order to facilitate criticism and improvement of metaphysical theses, they can be represented in the form of a hierarchy, theses becoming less and less substantial, and more nearly such as to be required to be true if science, or the pursuit of knowledge, is to be possible at all, as one goes up the hierarchy. In this way one creates a framework of relatively unproblematic aims and methods, assumptions and methods, not likely to need revision, within which much more problematic aims and methods, much more substantial assumptions and methods, can be criticized and improved, as science proceeds. The *methods* of science evolve with evolving aims, assumptions, and knowledge, but what may be termed the *meta-methods* of science, associated with higher level, relatively unproblematic and fixed aims and methods, and required to assess *methods*, do not change.[33] This hierarchical structure of AOE is depicted in diagram 2 of chapter 1. (Nothing like this aim-oriented empiricist picture of science – or natural philosophy – exists for Popper. This is true even of his conception of "metaphysical research programmes".)

(10) There is a (fallible) method of discovery of new theories, even in physics. This involves modifying existing theoretical and metaphysical

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[32] For a sustained discussion of this point see my "Popper's Paradoxical Pursuit of Natural Philosophy" in J. Shearmur and G. Stokes (eds.) *Cambridge Companion to Popper*, (Cambridge: Cambridge University Press, 2009).

[33] This hierarchical structure of AOE effortlessly solves the problem of reconciling naturalist and normative conceptions of methodology and epistemology.

ideas in order to resolve conflicts between levels indicated in (8). (For Popper, there is no rational method of discovery.)

(11) AOE implies that there is a great deal of *continuity of theory* across scientific revolutions. Metaphysical theses at and above level 4 in the hierarchy of theses of AOE are likely to persist through revolutions, even if theories and theses at levels 2 and 3 change quite dramatically. (Such continuity is even more dependable above level 4.) This all-important theoretical continuity across revolutions is what makes rational discovery possible. (For Popper, like Kuhn, there is no basis for holding that theoretical ideas will persist across revolutions.)

(12) As a result of acknowledging and making explicit the metaphysical dimension of science there is a very considerable increase in the scope of scientific knowledge. Conjectural scientific knowledge includes a thesis about the ultimate nature of the physical universe, namely *physicalism* at level 4 of AOE. (For Popper, there is no such thesis in science.)

(13) AOE predicts that physics, if it is making genuine progress, will advance from one false theory to another – until, perhaps, a theory of everything is formulated. For AOE includes physicalism at level 4, and if physicalism is true, all dynamical physical theories about a restricted range of phenomena only (which cannot immediately be generalized to apply to all phenomena) are false. That physics has advanced in this way, from one false theory to another, is thus good news. It is just the way physics would advance were it to be making progress towards capturing physicalism in the form of the true theory of everything. (Nothing comparable holds granted Popper's philosophy of science, or standard empiricism more generally. Advancing from one false theory to another can only be bad news, from the standpoint of scientific progress. It has led some to speak of "the pessimistic induction".)[34]

(14) That physics advances from one false theory to another poses the problem: In that case, what does scientific progress mean? The problem

[34] McHenry asserts at one point that "the truth value of any scientific theory must be regarded as false". This represents a characteristic Popperian or standard empiricist attitude, and is too strong. Granted AOE, it is in *physics* that we should expect all dynamical theories to be false (until we formulate a theory of everything). Elsewhere in natural science mistakes may be made, but this does not mean we should expect all well corroborated theories to turn out to be false. For example, it is reasonable to hold that Harvey's theory concerning the heart is, quite straightforwardly, *true*.

is readily solved within the framework of AOE.[35] (It is much to Popper's credit that he first formulated this "problem of verisimilitude". But, as McHenry indicates, Popper's attempted solution is untenable. Furthermore, the solution to the problem makes clear that it cannot be solved if standard empiricism is presupposed. This is because, in order to solve the problem, one needs to refer to the true theory of everything – which may, or may not, be unified. This is hardly within the spirit of standard empiricism.)

(15) The notorious problem of what it is for a physical theory to be simple or unified has been solved within the framework of AOE.[36] (Popper failed to solve this problem. His attempt at solving the problem in *The Logic of Scientific Discovery* fails, for reasons I have already indicated, and his later, additional "requirement of simplicity" does not succeed either, as Popper himself acknowledges.)

(16) AOE solves the three parts of the problem of induction – an enormous success for the view which, so far, has received no comment whatsoever.[37] (Popper claimed to solve the problem of induction but failed to solve even the *methodological* part of the problem – the problem of specifying methodological rules governing acceptance and rejection of theories in science.)

McHenry says at one point that Popper has not "failed to say what progress is or means". But in order to do this one needs to specify correctly what the progress-achieving methods of natural science are, and one needs to solve the problems of verisimilitude, simplicity and induction. Popper, having failed on all these points, cannot in my view be held to say – in detail at least – what scientific progress is or means. McHenry is led to hold there is no difference between Popper and me in this respect because he ignores most of the differences between our two views, spelled out in (1) to (16) above.

There is an important additional point about scientific progress which Popper failed to make, as far as I know. What scientific progress means, and how successful we judge science to be in achieving it, may well

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[35] See my *From Knowledge to Wisdom,* 2nd edition, *op. cit.*, pp. 393-400 and 430-433. This improves on an earlier attempt at solving the problem to be found in *The Comprehensibility of the Universe, op. cit.*, pp. 211-217.

[36] See my *The Comprehensibility of the Universe, op. cit.*, chs. 3 and 4; *Is Science Neurotic?, op. cit.*, pp. 160-174; *From Knowledge to Wisdom,* 2nd edition, *op. cit.*, pp. 373-386.

[37] See *ibid*, pp. 400-430. See also my *The Comprehensibility of the Universe, op. cit.*, ch. 5; *Is Science Neurotic?, op. cit.*, appendix, section 6.

depend on what we take the *aim* of science to be. If we take the aim to be to increase human knowledge, and by "human knowledge" we mean "knowledge of experts", there can be little doubt that modern science has been astonishingly successful. But if "human knowledge" is interpreted to mean "scientific knowledge and understanding of the 6 billion people alive today", modern science must be judged to be very much less successful, given widespread ignorance of even the overall scientific picture of the world. If we take the aim of science to be "to help promote human welfare by scientific and technological means", the success of science must be judged to be mixed. Many have benefited immensely, but many others (the billion or so poor of the planet) have not. And there are the global problems science has made possible which serve to undermine human welfare: modern armaments and the lethal character of modern war; population explosion; destruction of natural habitats and extinction of species; pollution of land, sea and air; global warming.[38]

A few further remarks, now, about the different attitudes Popper and I hold towards metaphysics. We both hold some metaphysical ideas to be of great importance to science in the context of discovery. Popper denies that metaphysics is a part of scientific knowledge, in the (so-called) context of justification, whereas I hold that metaphysical theses asserting the knowability and comprehensibility of the universe are a central and fundamental component of scientific knowledge. For Popper, metaphysical ideas are "indispensable for science", and "give science its problems, its purposes, and its inspiration", but nevertheless are "more of the nature of myths, or of dreams, than of science".[39] Popper argues that some metaphysical theses – for example the thesis that nature is uniform – should be turned into methodological rules.[40] I hold almost the opposite. I argue that corresponding to methodological rules concerning unity or simplicity there are metaphysical theses which need to be made explicit within science, so that they can be critically assessed and *improved*, this leading to the improvement of the

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[38] See my "Can Humanity Learn to become Civilized? The Crisis of Science without Civilization", *Journal of Applied Philosophy* 17 (2000), pp. 29-44; *Is Science Neurotic?*, *op. cit.*, pp. 68-71.

[39] K. Popper, *Quantum Theory and the Schism in Physics* (London: Hutchinson, 1982), p. 165

[40] K. Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1959), pp. 252-253.

corresponding methodological rules. There is, in Popper, nothing like the *hierarchy* of metaphysical theses (and associated meta-methodological rules) that is such an important feature of AOE, facilitating, as it does, the critical assessment and improvement of metaphysical theses, and corresponding methods.[41]

The really big differences between Popper's views and mine arise, not so much in connection with natural science, as with the nature of social inquiry and the humanities, and academic inquiry as a whole, matters to which I now turn.[42]

Towards the end of his essay, McHenry asks what relevance AOE has for science devoted to the promotion of human welfare, or for wisdom-inquiry – the kind of inquiry I argue for, devoted to seeking and promoting wisdom. How, he asks, does AOE contribute "to the more general goal of solving problems of living or the achievement of global wisdom"? AOE "cannot be just another version of what" I call "the philosophy of knowledge".

To take the last point first, AOE might well be a part of knowledge-inquiry (as I tend to call "the philosophy of knowledge" these days). But this would miss the full import of AOE.

The basic idea of AOE is that the aim for science of explanatory truth (truth presupposed to be explanatory) is deeply problematic: physics must accept some assumption about how the universe is comprehensible, but this assumption is almost bound to be false. It is vital, therefore, that science seeks to improve this assumption and associated methods, its aim and methods, as it proceeds, as an integral part of scientific inquiry.

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[41] For further details concerning differences between Popper's and my views concerning natural science, its methods and philosophy, and the role of metaphysics, see my "Popper, Kuhn, Lakatos and Aim-Oriented Empiricism", P*hilosophia* 32/1-4, 2005, pp. 181-239; and my "Popper's Paradoxical Pursuit of Natural Philosophy", *op. cit.*

[42] For discussion of these points see my *From Knowledge to Wisdom*, *op. cit.*, 1st ed., pp. 189-198; 2nd ed., *op. cit.*, pp. 213-220; *Is Science Neurotic?*, *op. cit.*, pp. 71-99; "The Enlightenment Programme and Karl Popper", in . I. Jarvie, K. Milford and D. Miller (eds.) *Karl Popper: A Centenary Assessment. Volume 1: Life and Times, Values in a World of Facts*, (London: Ashgate, 2006), pp. 177-90; "The Enlightenment, Popper and Einstein", in Y. Shi *et al.* (eds.) *Knowledge and Wisdom: Advances in Multiple Criteria Decision Making and Human Systems Management* (Amsterdam: IOS Press, 2007), pp. 131-148.

This elementary idea of AOE that science needs to try to improve its problematic aim and methods as it proceeds can be generalized in three ways.

(1) It can be generalized to apply to other aspects of science that have problematic aims.

(2) It can be generalized so as to apply to other branches of academic inquiry, and indeed to academic inquiry as a whole, in so far as they have problematic aims.

(3) It can be generalized so as to apply to *all* worthwhile human endeavours, personal, social, institutional and global, in so far as they have problematic aims.

Let me take these points, briefly, in turn.

(1) In my view (as McHenry acknowledges), the aim of seeking *explanatory truth*, is a special case of the more general scientific aim of seeking *valuable truth*. This latter aim is, if anything, even more problematic. Of value to whom? In what way? When? The argument for AOE is that science is more rigorous and objective, and likely to be more successful, if problematic *metaphysical* assumptions inherent in the aim of science are made explicit, so they can be critically assessed and, we may hope, improved. Essentially the same argument applies to science taken to seek valuable truth. Science is more rigorous and objective, and likely to be more successful, if problematic *value* assumptions inherent in the aims of science are made explicit, so they can be critically assessed and, we may hope, improved. The aim of seeking knowledge of valuable truth is, however, a means to such knowledge being *used* by people in life, culturally or practically, to achieve diverse human goals deemed to be of value. There is, in short, a social, humanitarian or *political* dimension to science. This is, if anything, even more problematic. But, as before, science is more rigorous and objective, and likely to be more successful, if problematic *social* or *political* assumptions inherent in the aims of science are made explicit, so they can be critically assessed and, we may hope, improved. AOE, I have argued, needs to be generalized, to become "humane AOE", and "person-centred science" in turn, in recognition of these broader, highly problematic scientific aims.

(2) But it is not just science that has problematic aims. Other, non-scientific branches of inquiry have problematic aims. Indeed, inquiry as a whole – in so far as it seeks to produce that which is of value to humanity – has a profoundly problematic aim.

(3) Furthermore, many of our individual, social, institutional and global endeavours have profoundly problematic aims. Almost all our current global problems – from global warming, population growth, the lethal character of modern warfare, destruction of natural habitats and rapid extinction of species, to impending shortage of food, water and oil, and even the AIDS epidemic (aids being spread by modern methods of travel) – are consequences of economic and industrial progress made possible by modern science and technology. They are the unforeseen consequences of the social goals we have pursued. In personal, social, institutional and global life we need, whenever aims are problematic (as they mostly are) to make implicit implications of our aims and actions explicit, so that we can criticize and try to improve them. AOE needs to be generalize to become *aim-oriented rationality* (AOR), potentially fruitful, and in many cases absolutely vital, whenever our aims our problematic, whatever we may be doing.

Social inquiry and the humanities, I have argued, ought to take, as a basic task, to help humanity build into the fabric of social, institutional and global life the hierarchical structure of AOR, copied from AOE, so that we may, in life, improve our aims and methods as we live, somewhat as physicists would do were they to implement AOE explicitly in doing physics. Social inquiry, on this view, is not primarily social *science*, or the pursuit of *knowledge*. Rather it is social *methodology* or social *philosophy*.[43] What the philosophy of science is to science, according to AOE, so social inquiry ought to be to the social world. In particular, on this view, that fragment of social inquiry concerned with the institutional endeavour of science, namely the sociology of science, ought to be identical to the philosophy of science.

McHenry asks "Would aim-oriented rationalism put into effect have altered the course of physics in the twentieth century such that our current theories would look entirely different and serve humanity in a way in which they are irrelevant at present?". There would certainly be differences for *physics*,[44] but these would not be such as to make physics

[43] This differs dramatically from Popper's conception of social science as spelled out, for example, in his *The Poverty of Historicism* (London: Routledge, 1961). Wisdom-inquiry, again, differs radically from the version of knowledge-inquiry advocated by Popper.

[44] I have argued in some detail that if physicists ceased to pay lip service to standard empiricism and put AOE explicitly into scientific practice instead, this would have widespread implications for physics itself: see my *The Comprehensibility of the Universe, op. cit.*, pp. 23-33 and ch. 7; *Is Science Neurotic?, op. cit.*, ch. 3; "Do We

more relevant to practical problems of living than it is at present. It is not the *physics* of AOE or AOR that has wide import (as McHenry provocatively suggests) but the *methodology*. It is adoption of AOR throughout academia, so that academia may come to help humanity adopt AOR in personal, social and global life, that is so urgently needed. If we are to have a future we need to develop the capacity to *modify*, to *improve*, our aims when we see, as we do at present, that we are heading towards catastrophe. We have known about global warming for a long time, but have so far been unable to do what needs to be done to avert the worst possible futures unfolding for humanity. If AOR and wisdom-inquiry had been put into academic practice 50 years ago, so that problems of living, and the task of improving problematic aims and methods in life, had been given the importance that they deserve, we would now, in my view, be taking effective action to deal with the crisis of global warming. Bringing about the revolution from knowledge to wisdom is not just an academic matter: the future of civilization may depend on it.

From Knowledge to Wisdom

Copthorne Macdonald

In his wonderfully lucid and optimistic essay, Copthorne Macdonald describes recent initiatives in universities and education that can indeed be regarded as steps towards the implementation of what I have called *wisdom-inquiry* – inquiry rationally devoted to helping people realize what is of value in life. There are programmes of education and research devoted to environmental problems, elimination of poverty, conflict resolution, a more equable relationship between men and women. There is a flowering, in recent decades, of research into wisdom, and education devoted to promoting wisdom in schools and universities. Macdonald concludes that the transformation I have argued for, from knowledge-inquiry to wisdom-inquiry, is quietly underway, in a multiplicity of respects, even if we still have some way to go before universities fully devote themselves to the rational pursuit of wisdom.

With reservations, I am inclined to agree. In the second edition of *From Knowledge to Wisdom* I described a number of changes that have been made in recent years to aspects of science, and of academic inquiry

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Need a Scientific Revolution?", *Journal of Biological Physics and Chemistry* 8/3, (2008).

more generally which, I said, amount to first steps towards putting wisdom-inquiry into practice.[45] What is remarkable is that Macdonald and I refer to the same broad trends – environment, policy, peace studies, wisdom research and education – and then go on to give different specific examples of initiatives exemplifying these trends (apart from a few we both mention).[46] Without doubt, changes are underway which can be regarded as first steps towards wisdom-inquiry.

Why, then, do I have reservations? Why do I lack Macdonald's optimism?

It could be that this is a purely personal matter. Here I have been, for over 30 years, shouting at the top of my voice "We urgently need to bring about a revolution in academia so that it comes to help humanity learn how to create a better world!" and my cries have been ignored. Even my immediate academic colleagues, my fellow philosophers and philosophers of science – or rather, especially my immediate colleagues – have ignored what I have been shouting all these years (apart from those few who have tirelessly supported my apparently hopeless campaign). Now I discover that, entirely independently of me, some aspects of what I have been shouting about all these years are beginning to be put into practice. But it all seems to be so slow, so piecemeal, and in some respects, in connection with global warming especially, so late in the day as to be almost too late. It could be, however, that my sense of frustration and despair comes simply from hurt vanity: I am disappointed that my work has turned out to be so little known, so

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[45] See *From Knowledge to Wisdom*, 2nd ed., *op. cit.*, pp. 311-315 and 321-325. Even in the first edition, I indicated then recent publications that could be regarded as pointing towards wisdom-inquiry, in the last chapter entitled, optimistically, "The Revolution is Under Way". See also M. Iredale, "From knowledge-inquiry to wisdom-inquiry: is the revolution underway?", *London Review of Education*, 5/2, 2007, pp. 117-129, reprinted in R. Barnett and N. Maxwell (eds.) *Wisdom in the University* (London: Routledge, 2008), pp. 22-33.

[46] I think the most important factor that has led scientists and others in recent years to see that academia needs to change is global warming. There is a growing appreciation of the need for multi-disciplinary approaches to problems, and for scientists to interact with government, industry, the media, and the public. I should add that my own university – University College London – is at present introducing changes designed to promote contact between disciplines, and enhance research devoted to helping solve global problems. It is as if the university is now putting into practice that first step towards wisdom-inquiry I argued for in "Science, Reason, Knowledge and Wisdom: A Critique of Specialism", *Inquiry* 23 (1980), pp. 19-81.

ineffective, so unavailable in practice, so *unused*. It could be that I am not the right person to make an objective judgement of the significance of what is going on.

Without denying this as a possibility, let me, nevertheless, attempt an objective appraisal – one which, it will turn out, is not quite as optimistic as Macdonald's.

There are, to begin with, rather obvious developments in academia that have taken place since 1976 (when I first spelled out the case for the revolution in *What's Wrong With Science?*) which, if anything, amount to steps away from wisdom-inquiry, or which make it that much more difficult to put it into practice. In the UK at least, the whole question of funds has become far more important. In the old days what mattered was the quality of your research; now what matters is the amount of money you bring in to your university. The bureaucracy has become much more powerful and intrusive: instead of serving teaching and research, it tends now to run the show and make all the important decisions. Every academic complains bitterly about the amount of time that goes into administration. Careerism seems even more rampant than it used to be. The research assessment exercise (discussed by Donald Gillies in chapter 8) has the effect of repressing slow-developing, really original and fundamental research, and makes discussion and implementation of wisdom-inquiry much more difficult. These institutional changes have all but destroyed the liberal university, based on the idea that people are employed who are as good as can be at teaching and research, and then are given as much freedom and independence as possible, in the hope that what they choose to do will be worthwhile. Finally, the flourishing of various anti-rationalist doctrines and movements in universities, satirized and lambasted by Alan Sokal and others,[47] has all but drowned the idea that the fault with the status quo might be not an *excess* of reason, but quite the opposite, not enough. Both sides in the so-called Science Wars missed the point; and the battle

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[47] See A. Sokal and J.Bricmont, *Intellectual Impostures* (London: Profile Books, 1998); A. Sokal, *Beyond the Hoax* (Oxford: Oxford University Press, 2008); N. Koertge, ed., *A House Built on Sand: Exposing Postmodernist Myths about Science* (Oxford: Oxford University Press, 1998); P. Gross and N. Levitt (1994) *Higher Superstition: The Academic Left and Its Quarrels with Science* (Baltimore: John Hopkins University Press, 1994); P. Gross, N. Levitt and M. Lewis, eds., (1996) *The Flight from Science and Reason* (Baltimore: John Hopkins University Press, 1996).

served to obscure it. (Some of the positive trends Macdonald describes have been infected and subverted by these anti-rationalist views.)

These negative developments are important, but are not, perhaps, my main reason for being pessimistic. My fear is that what I call *knowledge-inquiry* is deeply rooted into the institutional structure of academia, and deeply rooted into the psyches of many academics. It is at present, in one or other of its forms, the only widely known idea as to what constitutes rational inquiry. Very powerful forces, having to do with ambition, careers, prizes, status, ingrained habits of thought and practice, keep it in place. As long as it continues to be, in academia, the dominant *ideology* (as it might be called), it will restrict and frustrate attempts to develop a more rational and humanly valuable kind of inquiry, devoted explicitly to helping us create a better world.

Knowledge-inquiry is a multifaceted and quite flexible beast. It does not restrict *what* one seeks to acquire knowledge about. Wisdom, peace, conflict, gender, development, poverty, environmental degradation, climate change, world health, extinction of species, inequality, population growth, well being: it readily permits all these to be studied. Knowledge-inquiry positively encourages and promotes the *application* of knowledge to the task of solving social problems. It even permits policy studies. And of course in academic practice, much goes on at the fringes, as it were, which violates the edicts of knowledge-inquiry – which rejects and mocks it. I have in mind anti-rationalist movements briefly referred to above, movements such as deconstructionism, post-modernism, social constructivism, and the so-called "strong programme" in the sociology of science.

The positive developments to which Macdonald refers can all be regarded as developments *within* the general framework of knowledge-inquiry, and not as developments which seriously challenge or threaten it.[48] That is the reason for my reservations, my pessimism. I am not sure that the positive developments Macdonald describes really go the heart of the matter – namely the overthrow of knowledge-inquiry and its

[48] This is not quite true of those trends which are explicitly and stridently *anti-rationalist*; but these merely provoke a backlash from self-styled "rationalists" who take knowledge-inquiry for granted, and the net result is that wisdom-inquiry is not advanced, and it becomes, if anything, even more difficult to get a hearing for the view. The all-important point that is invariably overlooked by critics of the status quo (and its supporters of course) is that knowledge-inquiry is defective, not because it has too much reason, but because it does not have enough.

replacement with something more like wisdom-inquiry. My complaint is not simply that the changes are taking place too slowly, and in too piecemeal a fashion (although that is a part of it). Rather, my fear is that *all* the changes we have seen so far can be accommodated within the framework of knowledge-inquiry, and thus do not serve to undermine that framework at all. And as long as the beast of knowledge-inquiry continues to sit astride our institutions of learning and dominate them, we will not be able to make the changes that we so urgently need to make. So much will be permitted; and then we will slam into a brick wall.

What we urgently need is a kind of academic inquiry which takes its first priority to be to help humanity come to understand what our problems of living are – personal, social and global – and what we need to do about them. Academics need to speak with the public, and not just with each other. The primary task is public education – education about what our problems are, and what we need to do (or stop doing) in order to solve them. This public education must, of course, go in both directions. Ideas, arguments, experiences, proposals, solutions must both flow into and out of academia. Public engagement, not instruction, is what is needed. Academics have, of course, much to learn from non-academics. We must get away from the idea that you must have a PhD before you can be permitted to contribute to academic thought. The thinking that really matters – wisdom-inquiry thought at its most fundamental and important – is the thinking we do as we live, informing and guiding our actions. A basic task of academia must be to hoover up the best ideas, discoveries, solutions, wherever they are to be found in the social world, and make them available to everyone. Academia has the task of sifting out and developing the best ideas that there are. And the central, fundamental task is to come up with ideas as to what we should *do*, how we should *live.* It is to create imaginatively, and assess critically, policies, political programmes, ways of living, philosophies of life. These intellectually fundamental tasks are to be undertaken by social inquiry and the humanities. The less intellectually fundamental scientific pursuit of knowledge and technological know-how emerges out of, and feeds back into, thinking about problems of living. On this view (as I have already said), social inquiry is social philosophy, or social methodology not, primarily, social *science* or the pursuit of knowledge: it seeks to promote imaginative and critical cooperatively rational resolving of conflicts and problems of living in the world and, as

a long-term task, build aim-oriented rationality into the fabric of social reality.

If we do not discover soon how to resolve our problems and conflicts in rather more cooperatively rational ways than we have managed to do so far, we face catastrophe. We are very unlikely to learn how to do this if our institutions of learning haven't discovered how to make this their basic priority. But this requires the wholesale replacement of knowledge-inquiry by something resembling wisdom-inquiry. The changes that need to be made to academia, as I have tried to indicate in the paragraph above, are far more radical, wholesale and structural than the piecemeal changes Macdonald has described. Changes that take place within the general framework of knowledge-inquiry are not enough. Furthermore, piecemeal changes of the kind we have witnessed so far seem to me not enough to whittle away, even gradually, the knowledge-inquiry beast.

I cannot help but believe that what is required is a general recognition of the damaging structural *irrationality* of knowledge-inquiry, and a general appreciation of the vital need to create and pursue academia along the lines of wisdom-inquiry. Above all, there needs to be recognition of the fundamental importance of improving problematic aims as we act, as we live.

Our only hope is to tackle our problems democratically. But, if this is to be done well, it requires electorates to have a good understanding of what our problems are and what we need to do about them. We cannot expect democratic governments to be much more enlightened than their electorates. There is at present a lamentable lack of understanding of what our problems are and what we need to do about them: one only has to think of the USA re-electing Bush after the Iraq war, after his disastrous "war against terrorism", and after his denial of the realities of global warming. (The UK does not fare much better, re-electing Blair.) As I write, it even seems possible that the Republicans will be re-elected yet again, with a vice President who thinks we are not causing global warming. It is now a matter of desperate urgency that we transform our big, public institutions of learning; what we have done so far is in the right direction, but does not begin to bring about the overall changes that are needed.

We suffer from a *philosophical* blunder. Our problem is, in part, that no one thinks that it is remotely possible that we could be *suffering* from a philosophical blunder – a blunder in wholesale aims and methods, in

the *philosophy* – of inquiry, that is, of how we think. Academic philosophers have reduced philosophy to just another specialized discipline alongside others – and an especially sterile one at that. Even academia is seen as irrelevant, "academic" being another word for "beside the point". Rampant specialism within academia has resulted in a state of affairs where few attempt to take responsibility for the whole enterprise.[49] There are individuals out there in the world who feel some responsibility for the state of the world, and where it is going, but all too few academics who take responsibility for academia as a whole, and for its responsibility towards the state of the world. We urgently need a new Enlightenment.

We need a rather general recognition of the following elementary philosophical points. There are two rival philosophies of inquiry that need serious consideration: knowledge-inquiry and wisdom-inquiry. The first dominates science, and academic inquiry more generally, but is damagingly irrational. The second is vastly superior to the first and, if implemented, would (other things being equal) result in a kind of inquiry superior to what we have at present both intellectually and practically. It would help us make progress towards a good world. The course of our civilization – even perhaps its very survival – may depend on replacing knowledge-inquiry with wisdom-inquiry.

I think I should conclude my response to Macdonald's essay by saying that, although I have declared myself to be more pessimistic than he is, nevertheless, as I always used to say to my students, our situation is too desperate for us to be able to afford the luxury of pessimism.

Steve Fuller

In his delightfully witty and erudite essay, touching on themes from Kant, Bentham, Aristotle, Francis Bacon, Hegel, Gibbon, Hume, Plato, Hayek, Kuhn, Freud, Spengler, Edward Said, von Humboldt, Daniel Dennett and Habermas, Steve Fuller first expresses reservations about traditional conceptions of wisdom, and then goes on to point out that the Western tradition has long downgraded or abandoned the very notion. Fuller contrasts different attitudes to wisdom to be found in the East and West. Western religions put all the emphasis on *prophecy*, on aspirations for the future, on progress; Eastern religions by contrast emphasize *wisdom*, learning to endure what will be, restricting

[49] See my "Science, Reason, Knowledge and Wisdom: A Critique of Specialism", *op. cit.*

expectations to one's circumstances, and perhaps becoming at one with reality. Fuller then wonders where I fit in. In some respects I clearly belong to the Western tradition of progress, but in others I have something in common, perhaps, with Eastern attitudes.

I must confess, immediately, that what I wish to stress about my work is not concern for *wisdom*, but rather the *argument* that there is an urgent need to transform the overall aims and methods of academic inquiry. Having developed the argument, I required a word to stand for the new aim of the new kind of inquiry my argument had led me to uphold, and "wisdom" seemed to me the best available. However, when I first spelled the argument out in print in my first book, *What's Wrong With Science?*, I did not employ "wisdom" at all.[50] Instead, I spoke of a "people's rational science of delight and compassion" (taken from the subtitle), the idea being that delight and compassion speak to the two aspirations of science at its best, on the one hand intellectual exploration, the endeavour to see, to know, and to understand for their own sake, and on the other hand science used to help relieve human suffering. Only when I came to write *From Knowledge to Wisdom* did it occur to me that I might employ "wisdom" to stand for delight and compassion – for the aim of the new kind of inquiry I was arguing for. In declaring what I take wisdom to be (on page 66 in the first edition, and on page 79 in the second edition),[51] I am not so much specifying how I think wisdom should be defined as indicating how I think the fundamental aim of rational inquiry should be characterized – an aim that may conveniently be labelled as "wisdom". But even this is not quite right, for wisdom, understood in this way, is merely a means to the end of realizing what is of value, apprehending, experiencing, sustaining and creating what is of value, living a life of value. *That* is what inquiry is ultimately for, in my view.

I share some of Fuller's suspicion of traditional ideas about wisdom – its association with religion, prophets, gurus; its anti-rationalist connotations; its tendency, in some cases, to act as a smokescreen for the

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[50] Before that the argument was developed in a manuscript, written in 1972, which never saw the light of day, called *The Aims of Science*.

[51] This says in part "wisdom being understood here to be the desire, the active endeavour, and the capacity to discover and achieve what is desirable and of value in life, both for oneself and others", and I go on to say that wisdom includes knowledge and understanding, and can be understood in institutional and social as well as personal terms.

manipulation and exploitation of the gullible and vulnerable. On the other hand, in declaring the aim of rational inquiry to be to seek and promote wisdom I cannot altogether dissociate myself from traditional notions. Instead, I should perhaps confess that I am attempting to improve *both* science *and* traditional ideas about wisdom.

What of the idea that wisdom-inquiry might be a synthesis of West and East? I do certainly argue that wisdom-inquiry is a synthesis of, and a great improvement over, traditional Rationalism and Romanticism: see chapter 1, section 4.[52] But that debate lies wholly within the Western tradition. Fuller suggests that the difference between West and East is that, whereas the West seeks to change the environment so that it comes to satisfy our needs and desires, the East seeks to change the self so that its needs and desires can be satisfied in the given environment. Given this characterization of the difference, it is little more than common sense to hold that we need an appropriate admixture of both. Wisdom-inquiry, with its emphasis on science and technology, and on tackling problems of living, all within the framework of improving problematic aims, clearly provides the means to do justice to both West and East, when construed in such terms.[53] One may, of course, doubt that this way of contrasting West and East has much to do with realities, as Fuller hints when he mentions Edward Said. Modern China does not exactly seem to embody the spirit of the East, as just characterized.

At one point Fuller remarks that, for me, "the main problem with science is that it is not consistently put to humanly beneficial ends". This *is* a problem, but not, for me, the main one, which is rather that natural science suffers from a damaging, irrational, untenable philosophy of science of *standard empiricism*. This seriously misrepresents the highly problematic aims of science, and prevents explicit discussion and attempted improvement of aims as an integral part of science itself. All the defects of modern science that I discuss – intellectual, educational, moral, social, cultural – stem from acceptance and attempted implementation of standard empiricism.[54] I also argue that natural

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[52] See my *What's Wrong With Science?*, *op. cit.*, pp. 173-196; *From Knowledge to Wisdom, op. cit.*, chs. 5 and 7; Is Science Neurotic?, op. cit., chs. 3 and 4.

[53] I am reminded in this context of Schumacher's call for "Buddhist economics": see E. F. Schumacher, *Small is Beautiful* (London: Blond and Briggs, 1973), Part I, Ch. 4.

[54] This theme is to be found in all my books, and a number of my papers. For the most recent expression of the argument see my "Do We Need a Scientific Revolution?", *op. cit.*

science needs to become a part of wisdom-inquiry if it is to develop its full potential. And I also argue that the real intellectual and moral disaster in academia lies, not with natural science but with social inquiry and the humanities. Social science is fundamentally misconceived. It ought not to be, fundamentally, *science* or the pursuit of *knowledge*, in the first instance, at all. Rather, its proper task is to promote cooperatively rational tackling of conflicts and problems of living in the real world.

Fuller remarks that I hold that academia should be "turned into a civil service dedicated to researching and applying science to solve the public's needs". That is not what I have in mind. What I have argued for (again and again, I am afraid) is that we need to put problems of living at the heart of academia, the proper basic task being to articulate, and try to improve the articulation of, problems of living, and propose and critically assess possible solutions – possible actions, policies, political programmes, philosophies of life. On a more long term basis, a basic task of social inquiry is to help us build aim-oriented rationality into the fabric of social life. This is very different from "researching and applying science to solve the public needs".

Fuller goes on to say that in my view "there is a relatively sharp division of labour between the public (who supplies the ends) and the scientists (who supply the means)". This has no resemblance to what I have written whatsoever. In my early romantic phase I put the matter, at one point, like this:-

> With humane aim-oriented empiricism and aim-oriented rationalism before us … it becomes crystal clear that we can no longer conceive of science as something primarily pursued by *experts*, owned by experts, a product of the expert dissociated intellect or mind. Properly conceived, science is much too central and important a part of our lives to be thought of, and practiced, in such a way. In essence, science is *our* activity, *our* creation, the outcome of *our* concern. It is the outcome of our sharing of our concern for our world and for each other. It is a part of the expression of, and at the same the outcome of, our concern to improve our relationships with the world and each other. The essential things, one might say, are *me*, *you*, and *cosmos*: science is the adjusting of relationships between *you*, *me*, and *cosmos*, so that these relationships become less painful, less frustrating, less restricting, more knowledgeable, more

> understanding, more appreciative, freer, more sensitive, more honest, more harmonious, more enjoyable, more trusting and loving. Obviously experts are important: some technical matters need to be delegated to experts, who may be permitted to pursue there matters under our kindly, watchful gaze, and with our help. But the *essential* thing is far too important, for too intimately associated with the very stuff of our lives, and the very stuff of our personal identity, to be left to experts to decide upon. Science would not be helping us if *expert science* deprived us utterly of all free will, and was given a free hand to determine the very stuff, the very fabric, of our lives. There is no choice: we must say this: The centre of *gravity* of science … lies within our own hearts.[55]

Many other passages in *What's Wrong With Science?* spell out the theme of science created by and for people, from children onwards, with some delegation to experts under our watchful, non-expert gaze. The whole text seeks to move "towards a people's rational science of delight and compassion" (the subtitle of the book).

Eight years later, in my more sober, rationalist phase, I put the matter more soberly, for example in a passage like the following:-

> Far from serious, prestigious inquiry being primarily scientific or academic, it is according to the philosophy of wisdom, if anything, all the other way round: for each one of us the most important and fundamental inquiry is the thinking that we personally engage in (on our own or with others) in seeking to discover what is desirable in the circumstances of our life, and how it is to be realized. Institutionalized inquiry is simply a development of our personal and social thinking, having as its basic task to help us rationally develop our own personal and social thinking and problem-solving, so we may all the better realize what is of value to us in our personal and social lives. Whereas for the philosophy of knowledge the fundamental kind of rational learning is acquiring knowledge, for the philosophy of wisdom the fundamental kind of rational learning is learning how to live, learning how to see, to experience, to participate in and create what is of value in existence.[56]

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[55] *What's Wrong With Science?*, *op. cit.*, p. 67.

[56] *From Knowledge to Wisdom, op. cit.*, 1st ed., p. 66; 2nd ed., p. 79.

Aspects of this theme, again, are further developed in *From Knowledge to Wisdom*, and in any number of my other publications.[57]

Fuller concludes by remarking that my proposal, like others, does not "pay sufficient attention to the prospect that scientific inquiry may generate findings that provide grounds for the public to radically re-orient its aims. (Global climate change is an obvious candidate example.)". I am bemused. Not only have I, almost *ad nauseam*, stressed the seriousness of the impending crisis of global warming; central to my work is the theme that our *aims* in both science and life are likely to be profoundly problematic, in part because of unintended consequences of what we do, it being of fundamental importance to scrutinize our aims, possible unintended consequences of our actions. Science, of course, can have a vital role in alerting us to the need "to radically re-orient" our aims. I invariably emphasize that there needs to be a two-way interaction between science and attempts at solving problems of living. Equally, our long-standing failure to implement wisdom-inquiry – which gives intellectual priority to our problems of living – has much to do with our current inability to respond adequately to the discovery that if we continue as we are we will plunge into catastrophe – a point I have made explicitly.[58]

John Stewart

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[57] See, for example, "Science, Reason, Knowledge and Wisdom: A Critique of Specialism", *op. cit.*; *Is Science Neurotic?, op. cit.*, chs. 3 and 4; "Philosophy Seminars for Five-Year-Olds", *Learning for Democracy*, 1/2 (2005), pp. 71-77 [reprinted in *Gifted Education International*, 22/2-3 (2007), pp. 122-127]; "Popper's Paradoxical Pursuit of Natural Philosophy", *op. cit.*, especially sections 1 and 8. I also emphasize the need for wisdom-inquiry academia to take up, as a basic task, to educate the public about what our global problems are, and what we need to do about them – education going in both directions in the form of arguments, ideas, information, experiences: see for example "Can Humanity Learn to become Civilized? The Crisis of Science without Civilization", *op. cit.*: "The Disastrous War against Terrorism: Violence versus Enlightenment", in A. W. Merkidze (ed.) *Terrorism Issues* (New York: Nova Science Publishers, 2007).

[58] See my "Are Philosophers Responsible for Global Warming?", *Philosophy Now*, issue 65, (January/ February 2008), pp. 12-13.

After giving a lucid summary of my argument,[59] John Stewart goes on to ask "why *is* it so hard to move from knowledge to wisdom?". It is hard, in part because of vested interests, in part because pursuing wisdom is a *social* matter. It cannot be done by an individual alone. Stewart then considers what opposes wisdom-inquiry in the world, and takes us on a fascinating journey which includes: an account of Etienne de la Boétie's *Discourse on Voluntary Servitude* published 1553 (of which I had never heard): Max Weber's views on capitalism and its links with Calvinism; the way capitalism compels the business world to narrow its aims to nothing more than a demented pursuit of profit; Karl Marx's view that competition drives profit down, which in turn leads to an almost lunatic striving for innovation in an attempt to boost profits; J. Schumpeter's account of social havoc produced by such capitalist innovation; the Club of Rome's thesis that unrestrained economic development must lead to ecological disaster – a disaster that now stares us in the face in the form of global warming and other impending environmental crises. What opposes wisdom-inquiry in the world is the capitalist system, with its reckless, blind pursuit of profit – just that which threatens us with impending disaster. What we need to change is what prevents change. Stewart concludes by alluding to the possibility of the complete collapse of all our social institutions, but hopes we might find the means to avoid this disaster.

I agree with Stewart's assessment of the gravity of our situation. On the day I write these words in the autumn of 2008, it was announced on the BBC news that, despite all the efforts to reduce CO_2 emissions, global CO_2 emissions are increasing at a rate that is four times the rate in 1990.

As Stewart clearly realizes (even though he does not make the point explicitly), those who back wisdom-inquiry face three tasks:

(1) Getting a hearing for the argument that we urgently need to transform our institutions of learning so that they put wisdom-inquiry into practice.

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[59] One minor *caveat*. Stewart asserts at one point: "It is therefore an elementary requisite for rationality to give overriding priority to correctly identifying the aims to be achieved". I tend to emphasize the importance, for rationality, of trying to *improve* our aims as we proceed (it being quite likely we will fail to identify what is best in a problematic aim at our first attempt). It is certainly a mistake to hold that, for rationality, we must first identify our aim correctly *before* we proceed (not, incidentally, quite what Stewart asserts).

(2) Transforming academia so that it puts wisdom-inquiry into practice.
(3) Transforming our social world so that it puts aim-oriented rationality (the methodological key to wisdom-inquiry) into practice.

What really matters is (3). (2) is important to help bring (3) about, and (1) is important to help bring (2) about.

I have devoted over 30 years of my working life to step (1) – so far, without much success. I used to think that (1) was a necessary first step to bringing (2) about, and (2) a necessary first step to bringing (3) about. Now I am not so sure. Despite my reservations, I agree with Copthorne Macdonald that changes are underway in academia which can be regarded as piecemeal steps towards wisdom-inquiry (without any awareness of my work): see above and chapter 3. And the dire threat of catastrophic runaway climate change is now prompting many to do what they can to contribute to a reduction in energy consumption and an increase in energy production by renewable methods (which do not in turn do more harm than good) – at the same time doing what they can to spread awareness of the urgent need to do these things. Many people are battling away, in different contexts and in different ways, to bring about changes, in the social world, and in academia, which can be regarded as steps towards (3) and (2).

However, even if (1) is not *necessary* for (2), and (2) for (3), it is undoubtedly the case that (1) would help with (2), and (2) would help with (3).

Is Stewart correct in holding that the key factor which blocks steps (1), (2) and (3) is the capitalist system and the demented, destructive drive towards profit? Undoubtedly, it is *a* factor. As I write these words, in the autumn of 2008, the world's financial system is in crisis – a crisis brought about by banks pursuing profit in an insane fashion, with no thought for the future. Blinkered pursuit of profit is clearly a part of the problem.

But is it the *key* factor, the *only* factor? My answer is "No".

To begin with, different factors block steps (1), (2) and (3). It has not been capitalism which has blocked (1), but rather, as Stewart and I agree, vested interests and ingrained habits of thought of senior academics, the deplorable state of academic philosophy, social constructivism and anti rationalism among historians and sociologists of

science, and the distraction of the Science Wars.[60] There is also what I have called "the lobster pot effect" of standard empiricism and knowledge-inquiry: once these doctrines are accepted and institutionalised, they shield themselves from effective criticism. Thus standard empiricism demands of a potential contribution to science that, in order to enter the intellectual domain of science, it must be empirically testable. The demonstration that standard empiricism is untenable fails to satisfy this requirement, and is thus excluded from science. In a similar way, the argument that knowledge-inquiry is irrational is not exactly a contribution to knowledge, and so is excluded, by knowledge-inquiry, from the intellectual domain of academic inquiry. In addition, in order to publish in scientific or academic journals, you are obliged to cast your paper in a form which conforms to the edicts of these doctrines (even if, privately, you reject them). Thus what is published seems to bear out the truth of the doctrines; this serves to convince students and fledgling academics of the truth of the doctrines, and makes it seem all the more absurd to question their validity. In line with all this, I have found it extraordinarily difficult, over the years, to get my work published in academic journals, let alone in *scientific* journals – although, before I stumbled across this "knowledge to wisdom" argument, I had no difficulty whatsoever. I admit there may be other reasons for difficulty in getting my work published. I also acknowledge that it may be easier these days to publish an article in a scientific journal calling current aims and methods of science into question than it was ten or twenty years ago.

These are some of the factors which have blocked step (1), none of which have much to do with capitalism. When it comes to step (2), much depends on the battle for step (1) having been won. But it has not been, which provides sufficient grounds for the failure so far to win the battle for (2) as well. But if step (1) had been achieved, I am quite sure that there would still be resistance to (2) in scientific and scholarly quarters, for the sorts of reasons I have indicated, and for reasons famously discussed by Kuhn.[61] If the academic community began to be serious in seeking to implement wisdom-inquiry, opposition might well

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[60] This has led rationalists into combating anti-rationalists, and has distracted them from considering the damaging structural irrationality of what they take for granted, standard empiricism and knowledge-inquiry.

[61] T. S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962), ch. XII.

come, even in liberal democracies, from government, industry, the military, the media, and the public. The capitalist system might play a role in opposing a switch in funding of scientific and technological research away from the needs of domestic industry and towards the needs of the world's poor. But this would be just one factor amongst others.

What of step (3), the one that really matters? We need to distinguish two very different questions. Is the capitalist drive towards profit primarily what needs to be changed? Is it this which primarily prevents aim-oriented rationality from being implemented in the social world?

The capitalist pursuit of profit would be a factor in preventing the implementation of aim-oriented rationalism in the social world – supposing steps (1) and (2) had already been achieved – but not the only one. There are other institutions and aspects of life resistant to imaginative and critical exploration of problematic aims as well: government, the military, religion, the law, public opinion. Those who possess inordinate wealth, power or status are likely to resist serious scrutiny of aims of relevant institutions and social endeavours. Quite generally, whenever aims are seriously problematic, there is a tendency to misrepresent aims, and a reluctance to reconsider and scrutinize aims officially acknowledged.

I am inclined to think that there is an even deeper reason for our ineptitude when it comes to acknowledging, critically examining and revising problematic aims. This has its roots in our evolutionary past. At the end of chapter 1, in section 6, I indicated five reasons why our evolutionary past may well make it especially difficult for us to put aim-oriented rationality into practice in personal and social life – it being, as a result, all the more important that we strive to do just that.

Even if capitalism is not exclusively – or even primarily – what prevents change, it might still be what primarily needs to be changed. Is this the case?

I am in favour of speaking of "the free market system" rather than capitalism, to emphasize *both* that there is something good about the system, and that greedy capitalists are not alone responsible for what is bad about it. What is good about the system is that it is responsive to the needs and desires of people. This in turn ensures that consumers share responsibility for what is bad about the market system along with greedy capitalists – and with incompetent governments, one might add. There is an additional point to speaking of the free market rather than

capitalism: it is a broader concept. A system that required all firms to be cooperatives, shares being owned only by those who worked for the firm in question, could be a *free* market, but would not, presumably, constitute capitalism.

The responsiveness of the free market to the needs and desires of people has its limitations, of course: advertising can create artificial markets; those with minority interests, and the poor may find it difficult to get the market to respond to their needs. The market only responds to need if backed up by sufficient wealth – and if wealth is unjustly distributed, the marked will be unjust as well. The desires of many have more impact on the market than what is genuinely of value, desired by few (unless the few are especially wealthy, of course): desires, however mundane, take precedence over what is genuinely of value. The market, responding to demand, may develop in ways which are, in some respects, undesirable for many – as when supermarkets cause small shops to close. If the market is to operate in ways which are, as far as possible, genuinely beneficial to all of us, it needs to be restricted in scope, encouraged and controlled by appropriate regulations, and developed in desirable directions by the purchases of enlightened consumers. All this in turn, in my view, requires a kind of academia that puts wisdom-inquiry into practice.

Is it the free market system, as it exists and operates at present, that needs primarily to be changed in order to develop a wiser world? An overwhelming need at present is to reduce dramatically and rapidly emissions of CO_2 and other greenhouse gases. Modern methods of energy production, industry, travel and agriculture, conjoined to explosive population growth are what have led us to the present impending crisis. It does not make any difference whether these modern methods of energy production, industry, etc., develop in a world dominated by capitalism or communism: if CO_2 is emitted at the same rate, global warming will proceed at the same rate. Nature knows nothing of political or economic systems. The old Soviet Union was even worse at polluting the environment than the capitalist "West". If the world was dominated by communism we would be today in an even worse predicament, in part because it would be even more difficult, in such a world, to get a hearing for the unwelcome news of global warming.

Can CO_2 emissions be reduced rapidly without the destruction of the capitalist system, as it exists and operates at present? Consumers,

governments and scientists could conspire to make offers to the capitalist system which it could not refuse. A combination of legislation, incentives, penalties, and consumer choices could redirect energy production and use, industry, travel and agriculture so that CO_2 emissions are rapidly reduced. In my view, this would require the redirection but not the destruction of capitalism.[62] And even if it did, we should still direct our attention towards what needs to be done: reduction of CO_2 emission. Turning our attention away to the attempted destruction of capitalism would be a strategic disaster.

Leaving global warming on one side, is capitalism what primarily needs to be changed to create a wiser world? It is a part of the problem. In *From Knowledge to Wisdom* I argued for a cooperative free market system. But it is only a *part* of our current clutch of global problems. There is our current tendency to resort to war, and the lethal character of modern war. There is the stockpiling of armaments. There are the countries lacking democracy, civil rights, free media. There is the rapid increase in world population, and pollution of land, sea and air. Capitalism is not required to generate these problems. Pollution in the Soviet block was as bad as, if not worse than, anything produced in the capitalist "West". And the Soviet Union was not exactly renowned for democracy, civil rights and free media. Pursuing an aim energetically and enthusiastically which one, at the same time, doubts and subjects to sustained criticism – which is what is required for aim-oriented rationalistic action – is inherently difficult, at both personal and institutional levels. We do not need to invoke capitalist greed to explain why it is so hard to put aim-oriented rationality into practice in our personal and social lives.

Joseph Agassi

Joseph Agassi sets out to explore the fundamental problem: How ought we to set agendas, for scientific research, for philosophy, for academic inquiry more generally, and for the public arena, for politics? During the course of his exploration, Agassi takes us on a whirlwind tour of such matters as: the impact of the Enlightenment and Romanticism, Arne Naess, Russell and Popper, the idea that knowledge is morally neutral, Popper's battle against inductivism, the question of

[62] I am inclined to think that the view that the free market system should be abolished, and the view it should be entirely unregulated, are two sides of the same coin. Everything of interest lies between these two extremes

whether we should attempt to engage irrationalists in rational debate, Kant and Kuhn, the failure of philosophers to discuss the problem of agenda setting apart from Francis Bacon and Polanyi who, however, have nothing useful to say. Boyle, Whewell, Schrödinger, Born, Peirce, Carnap and Wittgenstein are, amongst others, briefly visited during our whirlwind tour. Agassi tellingly remarks that the problem of agendas is judged by philosophers not to be on the agenda. In politics, Agassi declares, he who sets the agenda holds power. Tyrannies can create a semblance of democracy by determining the agenda of parliaments. Democracies set out to improve conditions by critical discussion and legislation, but there is a general preference for muddling through rather than adopting radical solutions.

Agassi concludes by pointing out that our situation is fraught with peril. Pollution, population growth, CO_2 emissions, nuclear war all threaten humanity. We urgently need a new radical global plan to tackle our global problems, but politics concerns itself only with local matters of party and nation, and philosophers discuss these matters not at all. Science cannot be relied upon for, as Churchill once remarked: "the Stone Age may return on the gleaming wings of science". Global survival ought to be on all our agendas.

Agassi fails to solve the problem he has raised concerning agendas, as he himself admits. What is odd is that he seems not to have noticed that my own work amounts to a proposal as to how to solve at least the *methodological* part of the problem.[63] Standard empiricism (SE) – including Popper's version – leaves the whole issue of choice of research aims or agendas beyond the domain of the scientific.[64] But SE is untenable. The widespread attempt to put SE into scientific practice damages science, in part precisely because it does place aims or agendas beyond science. SE is untenable because, in physics (the fundamental natural science) theories are selected on the basis of compatibility with (1) evidence, and (2) the best available metaphysical conjecture as to

[63] I am puzzled too by Agassi's suggestion that I hold that our most urgent global problem is the pressure of migration from poor countries to rich ones. I do not hold this, and have never said that I do.

[64] It so happens that Agassi declares questions concerning agendas to be "inherently unscientific". This suggests to me that he upholds some version of SE. As I explain in the text, once it is recognized that SE is untenable, it becomes clear that the rationality of science requires that questions concerning scientific aims or agendas be included within the rational, scientific domain. Just this would be done by AOE science – or science pursued within the context of wisdom-inquiry.

how the universe is physically comprehensible. The rationality of science requires that its metaphysical assumptions be rationally chosen, and aim-oriented empiricism (AOE) provides a meta-methodology for doing just that. AOE specifies how the agenda of theoretical physics can be progressively improved as science proceeds. And I go on to argue that, having acknowledged that science has the problematic aim of seeking *explanatory* truth, we need to take into account that science, more generally, seeks *valuable* truth, and seeks to make this available for the use and enrichment of humanity – these further scientific aims involving values and politics, if anything even more inherently problematic than metaphysics, and thus requiring sustained imaginative and critical discussion, as an integral part of science itself. But in order to accommodate this vital dimension of agenda discussion and improvement, the institutions of science need to be changed – especially as value and political aspects of scientific aims need the involvement of the public. We urgently need, in short, a new kind of science which acknowledges the need for sustained discussion at three levels: (1) empirical (2) theoretical and (3) aims or agendas.

All this, I go on to argue, has a profound significance for the rest of academia, and for the rest of life – especially our political life and our efforts to come to grips with our menacing global problems. Treading a path parallel to Popper, I have generalized AOE to form *aim-oriented rationality* (AOR), designed especially to help us improve problematic aims as we proceed, as we live. AOR is especially relevant when it comes to tackling our immense global problems – when it comes to attempting to make progress towards as good a world as possible. But a world that puts AOR into practice is hardly conceivable without wisdom-inquiry being put into practice in our universities and schools instead of what we by and large have at present, knowledge-inquiry. As a first step, we urgently need to bring about a revolution in academia.

Not a whisper of this argument is to be found in Agassi's contribution. He does say that I am "one of the few who have raised the alarm", but seems not to have noticed that I have had something to say about how we should deal with what is so alarming – how we should set about tackling our immense, intractable, threatening global problems.

Margaret Boden

In her outstanding contribution, packed with fascinating information about the birth and early development of cognitive science, Margaret

Boden argues that cognitive science could not have developed without computers, and computers would not have been developed without the military. In short, "Cognitive science as we know it today simply couldn't have existed without the military." Good science may have to depend on bad sources for funding. There are, Boden concludes, no easy answers.

Boden appreciates that her argument does not tell against wisdom-inquiry (or, more specifically, *humane aim-oriented empiricism* as I have called the doctrine that holds that a proper basic intellectual aim of science is to seek knowledge of valuable truth and make it available to humanity to help enrich human life[65]). Central to my whole argument is the point that the real aims of science are *profoundly problematic*. Boden's claim concerning military funding of science reinforces this point. Nevertheless, Boden does raise awkward questions for wisdom-inquiry – and for anyone who supports the whole idea of science being for humanity.

But before I discuss her main argument in more detail, I must first try to clear up what seems to me to be a rather serious misunderstanding of my work. Towards the end of her contribution, Boden says that in "insisting that social enquiry must precede science" I hold "not merely that social concerns should guide science's research agenda" but "also that the philosophy of the conscious human subject/society must underlie the epistemology of science itself" which, Boden remarks, comes "perilously close" to the irrationalism of Heidegger, Roszak, the strong programme, and views of Harré and Bruner.

I am horrified that I should have been so misunderstood. What I argue for is diametrically opposed to irrationalism, social constructivism and anti-realism.[66] As I have repeatedly said, my concern is to *strengthen* the objectivity, rationality and realism of science, not undermine these things.[67]

In arguing for wisdom-inquiry I do, it is true, argue that problems of living, of action, are intellectually more fundamental than problems of

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[65] See my *From Knowledge to Wisdom*, 1st ed., pp. 95 and 100-110; 2nd ed., pp. 108 and 113-122.

[66] Furthermore, I have repeatedly argued against irrationalism, anti-realism, social constructivism and the strong programme: see for example, my work on quantum theory (note 3 of chapter one), my "Induction and Scientific Realism: Einstein versus van Fraassen", *British Journal for the Philosophy of Science 44*, 1993, pp. 61-79, 81-101 and 275-305, and my *From Knowledge to Wisdom*, 2nd ed., pp. 7-9.

[67] See, for example, my *From Knowledge to Wisdom*, *op. cit.*, chs. 3-5.

knowledge, social inquiry is more fundamental than natural science, and action is, in a certain sense, more fundamental than knowledge (in that written statements of knowledge become mere marks on paper if not related to and understood by conscious beings acting in the world). I also argue that what I have called "person-to-person understanding"[68] is, in a certain sense, more fundamental than scientific understanding, in that the latter presupposes the former (although I also remark that "the two kinds of understanding dovetail together, being interdependent"[69]).[70] But far from any of this coming "perilously close" to irrationality, quite to the contrary, science pursued in accordance with aim-oriented empiricism (AOE), and wisdom-inquiry is, I argue, both more *rigorous* and likely to be of greater human value than science shaped by standard empiricism (SE), as at present. Whereas SE misrepresents the aims of science, and thus stifles scrutiny of problematic assumptions concerning metaphysics, values and politics inherent in these aims, AOE provides a meta-methodological framework for the scrutiny and improvement of these assumptions and aims. It is this which makes AOE science both more rigorous and likely to be of greater human value than science shaped by SE.[71]

Social inquiry is, for me, more fundamental than natural science *given the aim of promoting human welfare* (or seeking wisdom), but it is very definitely *not* more fundamental *given the aim of improving scientific knowledge and understanding of the world*. AOE science does not merely seek objective factual truth, and accept scientific realism; it goes very much further in that, according to AOE, physicalism is a relatively secure part of scientific knowledge.

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[68] Person-to-person understanding is my version of what is sometimes called "empathic understanding", "folk psychology" or "theory of mind": see my *From Knowledge to Wisdom*, *op. cit.*, 1st ed., pp. 183-189, 2nd ed., pp. 206-213. See also my *The Human World in the Physical Universe*, *op. cit.*, pp. 13-14 and 103-112., where I employ the term "personalistic" understanding.

[69] And I continue "Only the philosophy of wisdom can do justice to both kinds of understanding, and their interdependence, in a unifying way, both being essential to wisdom": see my *From Knowledge to Wisdom*, 1st ed., p. 189, 2nd ed., pp. 212-213.

[70] For arguments for these points see my *From Knowledge to Wisdom*, chs. 4, 5, 8 and 10; *The Human World in the Physical Universe*, *op. cit.*, chs. 2, 5, 6 and 9.

[71] See my *From Knowledge to Wisdom*, chs. 5 and 9; 2nd ed., ch. 15; *The Comprehensibility of the Universe, op. cit.*; *Is Science Neurotic?*; "Do We Need a Scientific Revolution?", *op. cit.*

Person-to-person understanding has a fundamental role within wisdom-inquiry, and cannot, in my view, be reduced to, or fully explicated in terms of, scientific understanding. In fact scientific understanding is itself, I argue, an aspect of person-to-person understanding with everything personal removed.[72] In seeking to acquire person-to-person understanding of another, I may be interested in *the person*, or I may be interested in *the world*, and I seek to know what the other believes about the world because I think this may contribute to my knowledge of it. Public scientific knowledge emerges from this aspect of person-to-person understanding. (And construing science in this way can aid the rigour and objectivity of science because, as a result of re-introducing the personal dimension, personal aims and motivations, into science, it becomes possible to critically assess, and possibly improve, these aims and motivations.)

The crucial point to appreciate is that, in construing scientific knowledge as an aspect of, a development of, person-to-person understanding, we do not in any way undermine the fact-seeking character of science, its objectivity or rigour (just the opposite, in fact). Quite generally, in order to acquire good, authentic person-to-person understanding of another I need to know, not just how that person sees his world, but what relevant aspects of the world are really like. Person-to-person understanding (as understood by me) thus presupposes and requires factual knowledge of the world, a concern for factual truth, objectivity, realism (ultimately, scientific realism). The two kinds of understanding, person-to-person and scientific, dovetail together, as I have said, in that they presuppose each other.

Finally, in holding that person-to-person understanding cannot be reduced to scientific understanding, I am not maintaining that it can replace cognitive science, AI or neuroscience (which is perhaps what Boden fears). My actual view is that these sciences are enriching, and will continue to enrich, person-to-person understanding, but will not *replace* this latter kind of understanding. Person-to-person understanding is not folk psychology, as conceived by Paul Churchland, to be replaced eventually by scientific psychology, just as folk physics is replaced by scientific physics.[73]

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[72] See my *From Knowledge to Wisdom*, 1st ed., pp. 188-189, 2nd ed., pp. 211-213; *The Human World in the Physical Universe*, *op. cit.*, pp. 108-111.

[73] See Ibid, p. 135, notes 31 and 32.

With that misunderstanding I hope cleared up, I return to considering Boden's main argument.

Is Boden right in holding that computers only exist today because they were developed by the military, and without "computers, no cognitive science"? I have my doubts about both claims. It is notoriously difficult to establish the truth of such counterfactual theses as these. Boden herself points out that many of those who played a crucial role in developing computers, and computer models of the brain – Turing, von Neumann, Bush and others – did so initially independently of the military. It was because of the second world war that they were dragged into doing war work. This must have played an important role in inducing the military to fund work developing computers. Quite generally, the war – and above all the outcome of the Manhattan project – played a crucial role in inducing the military to spend vast sums on scientific research (especially, of course, research in physics), as Daniel Greenberg has convincingly argued.[74] Suppose the second world war never happened. Hitler was assassinated before he attained power, Germany did not invade Czechoslovakia or Poland, and Japan, without Germany as an Ally, did not go to war with the USA. The founding fathers of the computer in the 1930s and 1940s successfully argued for the fundamental importance of the computer, for research and for civil society. Computers were developed without military funding.

Boden may object that this counterfactual story misses the point of her argument. Given that the war did happen, could those responsible for developing the computer have eschewed all involvement with the military after the war, and still found funds (from non-military sources) sufficient to create the modern computer? (Turing did, in fact, work on developing the computer, not very successfully,[75] at the National Laboratory in Teddington, UK.) The development of computers might have been delayed, but I think it would be very difficult to establish that we could not have something like the modern computer today by such a research route.

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[74] D. S. Greenberg, *The Politics of American Science* (Harmondsworth: Pelican Books, 1969).

[75] Turing was not good at presenting the case for the importance of his work to his superiors, and his approach to creating a workable computer was flawed: see A. Hodge, *Alan Turing* (London: Burnett Books, 1983).

6 D. S. Greenberg, *op. cit.*.

Boden is of course correct in arguing that computers were massively influential in the development of cognitive science, both in being a source of theoretical ideas for the science, and in providing the means to test artificial intelligence models. What she does not consider, however, is a point I made long ago, in 1985: computers may also have played a role in *retarding* progress in the fields Boden is concerned with: cognitive science, AI, psychology, neuroscience.[76]

These fields are all concerned to understand the human brain/mind. This is a profoundly intractable problem. It is vital to get as good a basic formulation of the problem as possible, and then choose the best possible *preliminary* or *subordinate* problems to tackle in an attempt to work progressively towards the basic problem one seeks to solve – the best possible research route, in others words, to the ultimate goal of understanding how the brain works. The fields Boden is concerned with failed in both of these elementary tasks, in the 1950s to 1970s, in part because of the all-pervasive influence of the idea that one could take the computer, with its distinction between hardware and software, as a good model for the brain and mind.[77]

In my 1985 paper I argued that, in order to get clear about how to formulate the basic problem properly, it is important to put things into the context of biology and evolution, and ask "what is the *function* of the brain?" as one might ask "what is the function of the heart or lungs?". Put in that way, the answer is obvious: the function of the brain is so to guide or control the actions of the animal that, in its given environment, it does what it needs to do to survive and reproduce. Evolution designs brains to be good at this. The fundamental problem, then, is to understand how brains control animals to act so as to be conducive to survival and reproductive success. At the most basic level, the problem of understanding how the brain works is the problem of understanding how the brain *controls* action. It is not primarily a problem of *computation* or of *intelligence*, but of *control*. At the very outset, this point was appreciated quite well, for example by those who, like Wiener, spoke up on behalf of the cybernetic model of the brain, but it got

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[76] See my "Methodological problems of neuroscience" in D. Rose and V.G. Dobson, eds., *Models of the Visual Cortex* (Chichester: John Wiley,1985) pp. 11-21.

[77] Margaret Boden, whose knowledge of the history of cognitive science and AI is vastly more extensive than mine, disagrees with my historical remarks about the field. For her account see M. Boden, *Mind as Machine: A History of Cognitive Science* (Oxford: Clarendon Press , 2006).

somewhat lost sight of as the computer model became increasingly influential in cognitive science and AI.

In my 1985 paper I argued that, if one takes *control* as fundamental, it is at once clear that what is required is an understanding of what may be called *hierarchical control*. At the top of the hierarchy there are (brain) processes determining the overall action: hunting, escaping from predator, caring for offspring. This high level control system controls, we may presume, a large number of lower level control systems which control specific actions (running, leaping, freezing), each of which in turn controls many lower level control systems controlling individual limbs, and ultimately individual muscles – the contraction of individual muscles being coordinated so at to produce the actions required or specified by the high level control system. It did not seem to me that AI and related fields in the 1960s and 1970s made any attempt at all to develop hierarchical control architecture of the type I have just indicated. This obvious idea did not seem to exist.[78] And the reason for this failure even to see what is needed seemed to me to come from the all-pervasive influence of the computer model of the brain. My knowledge of desktop computers is extremely limited, but my conjecture is that their architecture did not then, and probably still does not, resemble the hierarchical model I have indicated in the least.

I remember well that at some point in the 1970s, the arrival of the idea of parallel processing provoked great excitement. This rather confirms my hypothesis that the hierarchical model was not common currency at the time. If it had been, then parallel processing would not have been treated as a novel idea, for of course such processing is a part of hierarchical control, but only a part.

Not only did AI and related fields fail to formulate the basic problem properly (because, I suspect, of the influence of the computer model of the brain); they failed also to tackle the proper *preliminary* problems, i.e. choose the proper research route. AI, during the 1960s and 1970s sought to develop programmes which imitated fragments of "intelligent" human behaviour, such as playing chess, recognizing objects or speaking a language. The implicit idea, in other words, was that solutions to these

[78] But I may be overstating things here. Margaret Boden tells me that in the 1970s a few AI researchers were writing about, but not developing, the kind of hierarchical control systems I have indicated. Nevertheless, it still seems to me that much work in AI at the time did make the mistake I have attributed to it. I do not need to claim that *all* AI work funded by the military rested on an intellectual mistake.

preliminary problems could eventually be put together to solve the fundamental problem – an approach that eventually led to a kind of crisis in the field. But such an approach, based again on the computer model is, I argued in my 1985 paper, misconceived from the outset. Putting the problem into the context of biology and evolution would have made it abundantly clear that the proper research route is to follow the path of evolution, and begin by developing artificial brains which mimic the actions, the way of life, of living things with the simplest nervous systems – for example, the neural net of the sea anemone, and then work up gradually to nervous systems of increasing complexity and sophistication. Eventually this did become the approach adopted by AL – "artificial life".

Finally, the computer model misled workers in the field into thinking one could make a straightforward distinction between hardware and software when it comes to biological brains. This, I argued in my 1985 paper, is a grave mistake (and still seems to me to be a mistake). The distinction makes sense in connection with computers. Computers are designed quite specifically to run different programmes, and thus perform different functions. But brains are not designed like that at all. The proper way to think about the brain, I repeat, is that it is a hierarchical control system, and not anything like a computer at all.

If these points are correct, there are grounds for holding that the availability of funds from the military for the development of computers, and cognitive science linked to computers, served to corrupt research into understanding the human brain and mind. One could regard this as a specific example of a much broader kind of corruption that overtook natural science (especially physics) as a result of funds being poured into the field by the military after the second world war. Such is the message of Daniel Greenberg's brilliant and important *The Politics of Pure Science*.[79] Scientists squandered funds on expensive research projects which had poor prospects of yielding significant results. Deceiving grant giving bodies about the aims and likely outcome of research projects became standard. Scientists lost a certain independence from government. The availability of funds for expensive, big science research projects may have distracted scientists from formulating and tackling fundamental problems of understanding.[80] There may be, too, a

[79] D. S. Greenberg, *op. cit.*

[80] Vast sums were spent on building ever bigger particle accelerators, ostensibly to explore the fundamental nature of the physical universe, and yet basic flaws in the

gradual corrupting influence over priorities of research in that they came to correspond increasingly to the interests of the military. Furthermore, the military may fund science so that students can be trained to work, as graduates, in military research establishments.

Accepting funding from the military can, in short, damage science. Furthermore, in so far as it leads to deception when it comes to grant applications for research, it takes science further away from engaging in sustained *honest* discussion of the problematic aims of science – and thus from implementing humane aim-oriented empiricism and wisdom-inquiry.

Boden presents us with a sharp dilemma. Accepting military funding may be good for science, even essential, but it is also bad, because it supports the military, and may lead to results being used by the military. But if my remarks above are more or less correct, the dilemma may not be quite as sharp as Boden suggests, in that military funding is not as good for science as she indicates.

Boden knows vastly more about the history of cognitive science and AI than I do, and she may be able to tear my remarks about the negative influence of computers on these fields to pieces. My point, in any case, is not that military funds *only* have bad effects on science, but rather that, along with what is good, there may be bad effects as well.

Whatever conclusions one comes to concerning this matter, Boden's central point remains. The question of funding, especially from the military, is absolutely crucial when it comes to the task of developing a kind of science genuinely devoted to the best interests of humanity.

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relevant fundamental theory – orthodox quantum theory (OQT) – were ignored. OQT is defective because it fails to solve the wave/particle problem and, as a result, is a theory only about *the results of performing measurements on quantum systems*. It does not tell us what quantum systems, such as electrons and atoms, are (when not being measured), and does not unambiguously declare whether the quantum domain is fundamentally deterministic or probabilistic. The two most basic questions about the nature of the quantum domain remain answered. In order to answer them, what is needed is relatively inexpensive *thought*, not bigger particle accelerators (which do not help at all). Not till the 1990s did most physicists come to appreciate that OQT is fundamentally defective. In short, during the post-war decades, physicists failed to tackle the intellectually difficult but inexpensive research into the defects of OQT, and instead built ever bigger and more expensive particle accelerators. The sheer availability of funds may have encouraged this betrayal of the Einsteinian quest for understanding. See chapter one, note 3, for references to work on the defects of OQT.

When ought scientists to accept such funding? When one's country is engaged in a just war, but not otherwise?

So far I have said something about how military funding may corrupt science. But there is another, far more important issue to discuss: the vast size of the military in the world today, the spread of lethal armaments, conventional and nuclear, the threat of chemical and biological weapons, and the obscene levels of funding of the military, especially in the US, but in other countries too, such as the UK. In the UK, 30% of the budget for research and development is spent on the military; in the US it is 50%.[81] As I write, the US Department of Defence has just announced that it will invest an additional $400 million over the next five years to support basic research at academic institutions.

Far from enhancing the security of the globe, this vast expenditure on the military is a global menace. The US's military-industrial complex spreads like a cancer through the economy. It is now, for political and economic reasons, very difficult to dismantle or substantially diminish. Having a military of this size makes it almost imperative that it is used, every decade or so, to justify its existence. A war must be found to fight. Its mere existence provokes other countries to develop their military. Thus it was the US that intensified the arms race during the cold war, in massively increasing its arsenal of nuclear intercontinental missiles. The Soviet Union, initially, had far, far fewer missiles, but Khrushchev concealed this fact in order not to lose face, and those involved in the missile industry exploited his deception. The star wars episode during Ronald Reagan's presidency, is another example of scientists exploiting the situation to obtain funds to pursue their research. This was done quite cynically; no one – apart from Reagan himself perhaps – believed that star wars could actually work. Now, as I write, George Bush seeks to install star wars systems into countries on Russia's borders, thus provoking retaliation from Russia, which might lead to a new cold war and arms race. Scientists have played a crucial role in these lunatic developments.

If science is for humanity, the primary concern of scientists should be for humanity, if the interests of science and humanity come into conflict. The implications are, in my mind, quite clear: the scientific community

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[81] C. Langley, C. *Soldiers in the Laboratory* (Folkstone, Scientists for Global Responsibility, 2005).

must now refuse offers of funding from the military – especially in the US and UK.

But that is not enough. Indeed, it is not even the primary issue. The implication of my argument for humane aim-oriented empiricism is quite clear: scientific debate and discussion needs to proceed not, as at present, at two levels, (1) evidence, and (2) theory, but at *three* levels, (1), (2), and (3) problematic aims. We urgently need to bring about a revolution in the intellectual/institutional structure of science, so that scientists and non-scientists engage in sustained discussion about (a) what it is scientifically possible to discover, and (b) what it is desirable to discover, in the hope that the highly problematic overlap of (a) and (b) can be discovered and pursued.

Discussion of research aims and priorities must involve the public, since scientists, however qualified to discern what is scientifically discoverable, are not especially qualified to discern what it is of value to discover. New institutional means need to be created to promote and sustain such discussion. There need to be scientific/public conferences, journals, websites, radio and TV programmes. Journalists, MPs and other public figures need to be involved.

And this debate must include, what has been so scandalously neglected up to the present, discussion of funding of military research, and military funding of academic research. It is the deafening *silence* of the scientific community (apart from a few exceptions[82]) that is so shocking, and so harmful.[83]

A long term goal must surely be to demilitarize the globe, so that, eventually, we have police but not armies and weapons of mass destruction. A first step would be to reduce dramatically the size of the military in the US, and the budget for military research and development. To this one might add world-wide nuclear disarmament. These steps require, in my view, massive backing from academia. The US public

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[82] Boden tells me that the AI community objected vociferously to the "star wars" project.

[83] Boden indicates how damaging it can be for a research worker to decide, on an individual basis, not to accept funds from the military. But instead of military funding being resisted on an individual basis, what is required is for the discipline as a whole to find ways to discourage such funding – at least by means of public discussion and reporting of funding. Whether researchers should be blacklisted for accepting funding from the military – as they would be if they faked results – is another matter. However the issue is tackled, it would clearly be highly controversial. Boden's conclusion is absolutely right: there are no easy answers.

needs to be educated in how dangerous the massively oversize military is, and what needs to be done progressively to reduce its size. But I do not see this happening without wisdom-inquiry. We are not likely to get the military under sane control unless we have institutions of learning rationally devoted to helping us tackle our global problems, our problems of living, in increasingly cooperative ways.[84] Academics urgently need to put their house in order, and do all that they can to implement and pursue wisdom-inquiry.

Donald Gillies

Donald Gillies summarizes his earlier critique of the Research Assessment Exercise (RAE), operative in the UK, and then goes on to indicate how matters could be amended to improve both research and teaching. Both his criticisms of the *status quo*, and his positive proposals, seem to me to be eminently sensible. I was particularly struck by his account of the way the RAE operates so as to encourage Mr. B, good at teaching but bad at research, to do more research, while at the same time encouraging Ms. A, good at research but bad at teaching, to do more teaching. Gillies's main criticism is, however, that the RAE is designed to avoid the error of funding research which does not produce anything of value, but operates in such a way that it is very likely to commit the much more damaging error of excluding from research those few individuals who have the capacity to do research work of immense value.

Gillies's proposal is that the RAE should be disbanded, and the status of teaching should be transformed so that it becomes possible to advance one's career via excellence in teaching (and not just by means of research or admin, as at present). Individuals should be free to decide for themselves whether they wish to concentrate on teaching, research, or admin. This might not work at present, because most academics want to keep their teaching load as light as possible. But, Gillies argues, this would change dramatically if teaching was perceived to be a reliable road to promotion.

I am tempted to say that Gillies's proposals are far too sensible and practical to be adopted.

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[84] I have developed this argument in connection with the infamous "war on terror"; see my "The Disastrous War against Terrorism: Violence versus Enlightenment" in A. W. Merkidze, ed., *Terrorism Issues: Threat, Assessment, Consequences and Prevention* (New York: Nova Science Publishers, 2007), ch. 3, pp. 111-133.

If adopted, would they help acceptance and adoption of wisdom-Inquiry? They might help a bit, perhaps, but not much. The chief obstacles to the acceptance and adoption of wisdom-inquiry by the academic community are, it seems to me, those that I indicated in my *From Knowledge to Wisdom*. As I have already mentioned, there is what I have called "the lobster pot effect": standard empiricism, once adopted, shields itself from effective criticism by restricting the intellectual domain of science to testable claims to knowledge (and that which bears on such claims). Criticisms of standard empiricism, arguments for what purports to be an improved conception of science, do not qualify for entry, and are excluded from science. The philosophy of science is not, at present, an integral part of science (as AOE requires it to be). Again, knowledge-inquiry, being built into the institutional structure of academe, determines what is to count as a contribution to academic thought. It determines criteria for publication, for academic excellence, and thus influences such things as promotions, careers, status, prizes, funds for research. Senior academics will tend to be resistant to the idea that what decides these important features of academic life – the aim and methods of academic inquiry – needs to be transformed. Again, what opposition there is to knowledge-inquiry tends to express itself as anti-rationalism, social constructivism, doubts about the reality or value of scientific knowledge and progress. This has led to a backlash from those defending scientific rationality, the authenticity and value of science. The resulting "science wars" debate has led to orthodox positions becoming all the more firmly entrenched. Both sides in the debate miss the crucial point that "scientific rationality", so called, is actually a characteristic kind of irrationality masquerading as rationality, there being an urgent need to develop a more rigorous kind of science, and academic inquiry more generally, of greater value when judged from both intellectual and humanitarian standpoints. Standard empiricism and knowledge-inquiry are not, of course, taught explicitly; they are rather implicit in everything that is taught – implicit in much that goes on in universities. These doctrines become deeply ingrained habits of thought as a result, difficult to dislodge, call into question, and revise. Again, rampant specialization tends to make academics responsible for their own speciality, but indifferent to questions about the value, the integrity, of academic inquiry *considered as a whole*. No one takes responsibility for the intellectual and human value of the entire academic enterprise, and there is no arena for the expression of such responsibility, such

concerns, should they be felt. It is as if everyone has decided, long ago, that the proper basic task of academia is to amass more and more specialized knowledge, this being so utterly obvious, so wholly beyond all possible doubt, that it can be placed permanently in limbo, ignored and forgotten.[85] But just occasionally, as Gillies in effect reminds us, such elementary, banal truisms turn out to be nothing of the kind. Sometimes, what everyone ignores ought to receive the most active attention.

Gillies's proposals would return us to a slightly improved version of what prevailed when I began my academic career in the mid 1960s – improved because greater emphasis would be placed on the value of, and the career rewards to be had from, teaching. But in the 1960s, knowledge-inquiry was even more firmly in place than it is nowadays, in 2008.

[85] For these points see my *From Knowledge to Wisdom*, 1st ed., pp. 45-46, 123-124; 2nd ed., pp. 7-9, 58-59, 134-135 and 305-306; "Science, Reason, Knowledge and Wisdom: A Criticism of Specialism", *op. cit.* See too works referred to in note 47.