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«The Boundary between Scientific and Non-Scientific Knowledge»

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The Boundary between Scientific and Non-scientific Knowledge

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The main claim of this paper is that the boundary between scientific and non-scientific knowledge does exist — which means several things. First, it's not the case that anything goes: some irrationalists have been mistaken into acceptance of that wrong conclusion because they have remarked that, however the boundary might be drawn, some important scientific developments would fall afoul of the standards entitling a research practice to count as scientific. Second, the boundary is not an imaginary one, that is to say besides what is scientific and what is unscientific there also is what lies at the boundary, certain research practices which are neither wholly scientific nor fully unscientific. Third, studying what is science is itself a kind of research belonging to the boundary, since the methods available in that research are not as strictly rigorous as those used in science proper; in fact, all of philosophy is included in the boundary in question. Fourth, the boundary (and in fact science itself) displays a characteristic structure pertaining to what are by now usually called "non-wellfounded sets" — sets, that is, which are somehow or other involved in themselves, whether as members, or as members of members or so on; the significance of the last thesis is that the best way of approaching philosophy of science is not standard set-theory, but theories allowing non-wellfounded sets are preferable. Fifth, and last, admission of the boundary's existence compels us to go beyond standard classical logic and to look for a more suitable logic, as for instance some kind of fuzzy paraconsistent logic.

§1.— When we ponder the kind of arguments put forward by Feyerabend — whom I take to be an outstanding irrationalist and one who has argued for that position better than most, to say the least —, we can realize that his main arguments (or even perhaps all of them) boil down to some version or other of a slippery slope argument. They are very similar to sorites arguments. Suppose there is a demarcation line between science and non-science;

suppose it is drawn according to the compliance with such and such procedures; then either the chosen criterion is too loose or else it leaves out some practices which as a matter of fact have contributed to the progress of science. Thus, past experience shows that enforcing some standards is going to rule out developments which **may** once become material and positive for science, even if now most of us don't think so.

That kind of argument is by no means absurd or entirely unconvincing. On the contrary, there is much to say on its behalf. Were the boundary between science and non-science rigidly established, there would always be a big risk of banning research activities which may turn out to constitute interesting developments after all, however open-mindedly the frontiers might be settled, unless of course they were designed so generously that lots of utterly undesirable results would thus be ruled in — a great many pseudo-sciences, for instance. Therefore, there is no arresting point short of one allowing total collapse of science as a definite, exclusive, rigorous set of research activities characterized by its own methods and standards.

What is wrong with the argument is not its set of premises, but its logic, which is typically two-valued. It admits nothing else but crisp, clear-cut fields, precisely defined. Its inference rules are disjunctive syllogism and contraposition: (1) either a [purported] research practice complies with the standards in operation or else it doesn't and so fails to be scientific; now, a number of fruitful and — as has emerged eventually — advancing theoretic activities don't or didn't comply with those standards; hence, if the standards must hold sway, those activities fail (or failed) to be scientific; consequently, (2), since we are not prepared to give them up as genuinely scientific, the standards must go. Any standards whatsoever.

That kind of reasoning has also been applied elsewhere, and with equally unpalatable results. Certain "nihilists" are keen on rejecting any difference between what is and counts as moral and what neither is nor counts as such; or between progress and lack of progresses, etc. But the structure of many, or most, of those arguments is the same as that which makes Peter Unger conclude «I don't exist» — in fact that none of the currently accepted objets of our usual ontology exists at all. In his recent book ***The ontology of Physical Objets*** (Cambridge, 1990) Mark Heller argues that that ontology is inconsistent because it gives rise to sorites like those Unger has gone into. Whatever the merits of M. Heller's favoured solution (namely, to resort to an ontology of unchangeable spatio-temporal hunks), it's hard to see how it could be applied to our present concern. Yes, science can be conceived of as a field whose defining criterion is variable according to time. That may be the case (and as a matter of fact I find the idea congenial), but then, if that is the end of the story, epistemology serves no interesting purpose, because only afterwards could we say whether some practice under consideration is (rather, **was**) scientific or not.

Fortunately there is an available alternative, namely the one offered by some non-classical logics. It is not a question for a thing of either wholly and fully being a table or else utterly and thoroughly failing to be a table. Nor is it a question for a research activity of either being entirely scientific or else lacking any scientificalness at all. There are of course degrees. Perhaps nothing counts as absolutely as a table as many people ore fond of thinking, since for any given table there could be another fulfilling the tablehood requirements better. Anyway, a great many tables needn't (and don't) count as being in fact tables to the utmost, so to say. They are imperfect tables — not just tables which happen to be imperfect tools, but, more to the point, entities which fail to completely meet the requirements for a thing correctly to be called 'a table': they may be a little bit too small, or too low, or too unstable, or may have

a top too slanting, or too rough-surfaced. Nevertheless, they serve us and we are wont to call them tables; they are tables — up to a point. On the other hand, many entities whose tablehood seems beyond doubt do in fact, all things considered, count less as tables than some others: their tops may be less smooth, or less horizontal, etc. Likewise, some practices which we rightly call scientific may be less rigorous, less moulded according to standards of simplicity, elegance, ontologic economy, methodological soundness, linkage with observability and so on.

No need to seek for them very far away. Each scientific discipline blossoms in a plurality of schools and doctrines such that followers of one of them regard suspiciously what devotees of the opposite school do, deeming it either out and out unscientific or at least **less** scientific. And for any discipline there is another which many scientists are less prone to admit as having scientific status. Yes, you can differentiate between hard and soft sciences, but even so some hard sciences are probably harder, and some softer sciences softer than others, and some sciences lie in-between, don't they?

§2.— Ignoring degree differences strands us in a sorrow, un-welcoming beach. What ought to be conceded to such as argue for irrationalism or for the complete blotting out of the frontiers between science and non-science is that those borders — as many others — are blurred and hazy. That's why boundaries exist. Otherwise they wouldn't be existing entities, but imaginary items. More accurately put, speaking [as it were] about boundaries or limits would be a mere *façon de parler*. There are lots of man-made tools belonging to the boundary between tables and non-tables because we are only up to a point right when saying that they are tables. Likewise there is a real boundary between science and unscientific theoretical activities because the boundary has some thickness, whether large or small; otherwise there could be scientific activities, on the one hand, unscientific ones, on the other, but nothing in-between, and so no boundary proper.

Now, adversaries of solutions to sorites paradoxes by means of alternative logics are fond of finding fault with those solutions on account of their inability to cope with so-called second order vagueness. Should it be admitted that the is no particular point or [perfectly] thin line between two domains, or fields, or sets, or whatever, the problem remains of whether there is a point or line between what totally and fully counts as belonging to one of the domains and what [completely] fails to do so; and likewise whether there is a similar line between what doesn't belong to the field **at all** and what to some extent or other does belong to the field. However, the first half of the objection can be satisfactorily answered by at least some fuzzy logics, as follows. To be true is not the same as to be wholly true. Being true admits of degrees. For a proposition (or sentence or whatever) to be true is just for it to be true, not to be true up to this or that point. Likewise, for an entity to be a table there is no need for it to have perfect or complete tablehood. At the same time, even the slightest change visited upon a table in virtue of which it becomes even so little less horizontal, smooth-

surfaced, solid, and so on, diminishes — even so little — the table's degree of tablehood. At the opposite extreme, though, the problem is more serious, but the challenge can be met in some less simple manner: for instance by accepting the **principle of Anaxagoras** to the effect that in the physical world all differences are matters of degree, or at least by contending that nothing resulting from a table by a series of feasible minute chip removals is ever a perfect non-table (in other words the table would never cease being one over such a chain of processes, although its degree of tablehood would become so small as to make pragmatically unfounded, mistaking, any utterance calling the result a table; but there would also be degrees of pragmatic appropriateness, of course, whether or not thresholds ought to be reckoned with here); alternatively by accepting a definite and in principle knowable point of dwindling beyond which nothing is a table at all. (But why then such a solution is not available to the classicist? For one thing, he regards whatever falls short of the cutting-off point as being a table [wholly, entirely, fully — all those words are for him redundant and insignificant, purely pragmatic devices], and so as being a table to the same degree as any chosen paradigmatic table may do; so the cutting-off point becomes a jump from all to nothing. For another, even if our now considered solution were found to be less than satisfactory, the gradualist approach would at least have solved the first half of the difficulty, namely the real and common problem of knowing whether, since a series of tamperings with a table's integrity would deprive it of its tablehood, any tampering whatever would do so, too; whereas that solution wouldn't be open to the classicist.)

Similar considerations apply to our case. Why to worry about perfectly scientific practices? Perhaps there is none — and then nothing is wholly scientific. For any standard of rigor, accuracy, theoretical elegance, empirical content, and so on, there may be another more pressing standard. So, for instance, for any logical inference rule, there is another which can be rightly termed less controversial, less risky, less lax, and so on. (Examples? Take *modus ponens*: a minority of logicians reject it, or accept it only with qualifications; for any such qualification, there is or may be another one which lets out inferences allowed by the former; and so on. For any cogent argument, there in another more cogent one.)

But whether or not there are perfectly scientific practices, most interesting research activities are not perfectly scientific. Exception can be taken at them, and is in fact taken from some quarters — not gratuitously, but in virtue of telling if not necessarily unquestionable reasons.

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§3.— One of the studies which are blamed for their unscientificalness is philosophy. A philosophical theory would fall much short of meeting the requirements of empirical content, solvability of disagreements and so on which purportedly would pertain to science. Nowadays such a view is not as fashionable as it used to be, since most contemporaries theories of what science is are much less demanding and commendatory. So philosophy might have better chances of passing the test of being scientific or at least closer to science than positivists

were bent on saying. Quine goes so far as to make science a whole field within which metaphysics would be at the centre, less exposed to unforeseen somersaults of encountering recalcitrant experiences. Yet Quine's own conceptions can be regarded as in a way shaping a framework within which philosophy's degree of unscientificalness can be better understood. For Quine's gradualistic approach can be construed as bestowing highest degrees of rigor to what is neither as close to experience as observational statements are nor as distant as metaphysics or logic. Observational claims can of course be challenged, assessed, argued for and against, but never, **to the extent they are observational**, completely, since their main force resides in the fact, or purported fact, that things have been seen [or touched, or whatever] thus and so. At the other extreme, so-called analytical sentences can also be challenged, argued for and against, etc., but, **to the extent they are analytical**, there always remains a measure of unchallengeableness ("What you are alleging doesn't counter my point, since you are giving the words a different meaning, or else…") Rigorous science fails to contain either analytic or observational sentences. More accurately, a discipline is rigorously scientific in so much as it does so (among other requirements, too, of course).

The metaphysical and logical core or our body of beliefs and research practices is less subject or liable to critical assessment than other provinces because of its remoteness from observation, and accordingly its poor empirical content. But, needless to say, that is just a difference of degree, and perhaps not a great one. Physical, biological, sociological, linguistical or economic theories can be protected by countless devices from recalcitrant experience. All that we by now are well aware of. So any difference on that score between them and philosophical theories is, if at all, a question of extent and nothing more. After all, empirical content can also be alleged to militate for or against certain philosophical theories, as is obviously the case very often. But the degree to which a philosophical theory can be damaged by recalcitrant experience is smaller, much smaller than for instance a physical theory.

And yet, philosophy comprises the study of science itself. There is no [other] scientific discipline concerned with what science is. Of course, there is history of science (but is history a **rigorous** science?), and the like, but none of them goes into the question of what science is, and of what counts as a science.

For many a scientist, any such question is uncongenial, "too metaphysical". A biologist may contend that he is studying some organisms, but that studying what his study is goes beyond the scope of what he does and of what interests him, or even beyond the scope of what is rationally ascertainable. Unfortunately, though, such a stance suffers from a deep weakness. For then there would be no scientific, rational ground for preferring biologic theories to theories as the ones astrologists and the legion of their like try to foist upon us. If there is no rational means of choosing science, then science is in a precarious state indeed.

But no, there is a rational way of approaching that study, and it is through philosophical investigation. Only, that investigation is not as rigorous, as endowed of empirical content, as testable, as those studies it is concerned with. If we are intent on regarding the theoretical study of science as providing a ground for science itself, we needn't expect the basis to be as strong, or solid, or reliable as what it is called to ground. Yes, all-or-nothing approaches would then pretend that unless a ground is firmer that what it is called to support, there is no use for it. Such a maximalism seems to me wrong-headed. If we gain in understanding thanks to philosophical elucidation, why to spurn it just because it doesn't afford us enhanced firmness?

Logicism in mathematics is nowadays often rejected on several counts. All of them seem to me mistaken, but that is by the way. One of the objections against logicism is that the degree of certainty of logical axioms and inference rules is lesser than that of the mathematical theorems proved within a certain logical calculus, like Russell's PP.MM. or Quine's ML or the like. I surmise that a hidden assumption underlying such allegations is that no ground is useful if it is less solid than what it is destined to ground. But then all science is unnecessary, since every scientific theory is less certain than some empirical generalizations it tries to explain. As science advances, it becomes less uncontroversial. Perhaps, too, it becomes less scientific. But of course scientificalness is just one among several *desiderata*, and nothing proves that a greater degree of scientificalness is always to be preferred, whatever else is the case. In pre-Copernican times unanimity reigned in astronomy, by and large. Our current physical theories are more speculative, more controversial, more philosophy-like.

Doing philosophy of science is of course going in for a most controversial subject. If anybody thought that, unlike traditional philosophy, the philosophical study of science was going to enjoy the certainty which purportedly characterizes science itself (or rather which was a number of decades ago supposed to pertain to science), he has been shown to be wrong. Contemporary theories of science are among the craziest things philosophers have ever dared to say. Many of them are very interesting, besides having other virtues, too. But the [absolute] certainty which was once hoped for, no, nothing of the sort has come or will come our way. Those theories are surely much more controversial than what they intend to study. They are less scientific or rational. Less rational since accepting one of such theories rather than an alternative one rests more on "ultimate choices". (But ultimateness can also be taken to be a matter of degree; no ultimate choice need be an absolutely unshakable dogma to be cleaved to come what may.)

Should there be no boundary between science and non-science, should there be only science, on the one side, and non-science on the other, with both of them being entirely one thing or the other, then the study of science would be as scientific (as unscientific) as necromancy. Fortunately that is not the case.

The use of a logical rule of inference is subject to much less controversy than the arguments which can be put forward in support of the rule. (So, logical theories are much less uncontroversial than most other scientific theories.) But, what a sorrow situation indeed would be one wherein such arguments wouldn't belong **at all** to science!

§4.— In his excellent book ***The situation in Logic***, J. Barwise explores the philosophical significance of non-well-founded sets, which (within a set-theoretical framework akin, in other respects, to ZF) have been studied by Peter Aczel.

Barwise puts forward an illuminating way of looking at sets: to view them as situations with their inner structure erased, so to speak. Take any situation, leave out the particular ways

in which its different components are there related among them. The result is a set. Orthodox standard set-theoretical lore has it that every set is well-founded, i.e. such that either it is empty or else it comprises a member none of whose members is comprised by the former set. It follows that no set comprises itself; that no set comprises another set comprising the former set; and so on. Those standard dogmas have been shattered by now. In my recent book, ***Rudimentos de lógica matemática***, I've contributed additional arguments to Barwise's main theme on that subject. And moreover there are further, independent reasons for waiving such standard dogmas.

Very briefly put, Barwise's idea is that there are lots of situations which are involved in themselves. Replace the situation by the set of its components, replace the relation of *having as a component* by that of comprising (or, if you want, of *comprising the entities involved in*, but identifying then the situation and the set corresponding to it), and you have it: any situation involving itself yields a set comprising itself.

Well of course the argument may be gibed at. Even if to each situation a corresponding set can be assigned, they remain different — or so it may be claimed. Thus, even if the situation is involved in itself, the set doesn't belong to itself; what belongs to it is the situation. The objection doesn't convince me, because Barwise's point is that the set is just the situation, not something else: it is the situation in abstraction of the ways its components are related in it. Yes, that way of putting it is not as easy as it seems on first sight, since it is far from clear, in general, what is meant by viewing an entity "in abstraction of" such and such features thereof. Well, if it really has those features, you can refrain from looking at them, but you don't remove them by closing your eyes on them.

But Barwise's idea is not that you eliminate or remove the structure. It is just that to say that set A comprises entity E is to say that situation A has E as one of its components, whatever the particular way it lies there. Personally I don't wholly share that view, but that is immaterial here. (My own approach entails the need for a complication in the argument. The first set corresponding to a self-involving situation is bound to comprise the situation; but the second set corresponding to the situation will be one comprising no situations but, in their stead, the sets corresponding to them in the first place, and so on; as a limit of the sequence, situations have been eliminated in favour of sets. And then the [finally emerging] set corresponding to the situation does comprise itself.)

There are several reasons in support of the claim that science is a self-involving situation. We have seen that the study of science is itself scientific, at least in some degree, since it belongs to an investigation lying at the boundary between science and non-science, and what lies at the boundary does — to some extent or other, whatever small — belong to science. So there is a [to some extent] scientific study of science. Science is one of its own components. Or, if you prefer to regard science as a set of research activities, one of them is the one which studies the set, which would then become a component of a situation belonging to the set. After all the process of replacement of situations by sets has been carried through, we'd come up with the result that science comprises something comprising … something comprising science.

But there are other ways in which science is self-involving, too. They have much to do with one of the arguments for irrationalism which have been put forward of late and to which I was referring some pages earlier. One hears that there is no [clear-cut] boundary between science and non-science (and therefore, according to maximalists or classicists, no

boundary at all between them) because in the last resort for a method to be scientific is for it to be accepted as such by the community of scientists. In a less stringent version what is claimed is that no proper characterization of science is possible unless it refers to the brute fact of science, or at least its tradition, which cannot be independently defined. Science couldn't be exhaustively characterized in terms none of which mentioned science; thus it could be well individuated only through deictic means.

Some of the point can be conceded. A comparison with politics may be in order. A State is nothing else but an organization claiming the monopoly of the use of force within some land and recognized as rightly entitled to it by the other States. Suppose there were two jointly [worldwide] exhaustive and mutually exclusive sets fulfilling those requirements. Which of them would be **the** set of States? Just one of them, the one which would in fact comprise the States, rather that its competitor. But the definition doesn't allow us to know which one.

As regards science things are fortunately brighter and gayer. The comparison still holds. A scientific discipline is one recognized by [practitioners of] other sciences as having the privilege of studying some field or subject. We know that all that is a matter of degree (and furthermore also a matter of different respects or aspects) rather than an all-or-nothing issue. Yet, as regards science, should a situation arise like the one we were just considering for politics, there would be a good criterion, namely: science would be the more rational set of research and theoretical activities.

Something remains though. No complete characterization of what defines science is possible without mentioning science itself. The boundary between science and non-science is drawn in a way that, even if we have, as we do, sundry partial criteria, none of them offers us necessary and sufficient conditions, but only approximations to the ones or to the others. A residue remains, which evades definition. Nothing is of course scientific unless it fulfils some rationality requirements. And everything meeting all those requirements is doubtless scientific. But a procedure's degree of scientificalness may be higher, or may be lower, than the one to which it meets those rationality requirements. What makes the difference is the fact of science. Not a brute mysteriously irreducible metaphysical fact, but a fact elucidable by the degree of deviation of its membership from the degree of compliance with rationality requirements. Elucidable *in principle*, although only through an infinite analysis (an unending task). Meanwhile, we have something to give us patience, something thanks to which science is not elusive or ungraspable. That something is the fact that science is self-involving as so many other situations are. One of the standards of scientificalness is scientificalness itself. Which doesn't mean that to be accepted by scientists is the same as to be scientific. Only *cæteris paribus* (or more exactly, in a way tending towards *cæteris paribus*) is it true that the more a procedure is accepted as correct by scientists the more scientific it is, and conversely. But a full analysis of the degree of scientificalness of some method at a given period cannot proceed if we forget that the practice of science determines — in a way which is neither purely arbitrary nor completely based on valid objective reasons — a preference for some methods rather than others.

That can be conceded to people like Feyerabend without thereby admitting their conclusion that anything goes or, alternatively, that science is just science, one among many possible ways of approaching or investigating the world. It is not just one among many. It is the best one. But the way and degree in which it bestows acceptability to a certain method

or procedure depends in part of science's own factuality — its existence and traditions as things stand (not in a different possible world). But those deviations occur within strict and perhaps narrow limits.

§5.— Whatever else logic is, it is the study of — among other things (viz. logical truths) — correct inference rules. To be more specific, whether or not logic is concerned with other kinds of inferences, it is, more or less, beyond dispute that it investigates deductive inferences, that is to say inferences wherein transition from the premises to the conclusion is subject to no further condition whether internal to the reasoning person's mind or external to it.

We saw earlier that an argument, outstanding and frequently brought forward, buttressing irrationalist conclusions is a string of an application of disjunctive syllogism and one of contraposition. From the view-point of classical logic, that is all right. But not so from all other view-points.

The kind of logical approach I favour (for the time being, while we wait for a better solution) in order to cope with such difficulties as we have encountered concerning boundaries is ***transitive logic***, a family of closely related systems wherein two different negations are acknowledged: simple or natural negation, 'N', and strong negation, '¬'. The former is characterized by such properties as pertain to a unary operator in a Kleene algebra; namely (supposing both conjunction and disjunction are commutative, associative, idempotent, mutually distributive, enjoying absorption [i.e. ⌜p∨q∧q↔q⌝, where '↔' is the strongest equivalence functor in the system], addition [⌜p→.p∨q⌝], simplification [⌜p∧q→p⌝] and adjunction [⌜p⌝, ⌜q⌝ ⊢ ⌜p∧q⌝]): DeMorgan, involutivity, excluded middle (i.e. theoremhood of ⌜p∨Np⌝), abduction (⌜Np→p→p⌝) and the Kleene principle, ⌜p∧Np→.q∨Nq⌝ — with '→' being the strongest implication or entailment functor in the system). Although *modus tollens* is valid for simple negation, contraposition is not; in other words, this rule is correct in transitive logic: ⌜p→q⌝, ⌜Nq⌝ ⊢ ⌜Np⌝; but this **sequent** is not correct: If ⌜p⌝ ⊢ ⌜q⌝, then ⌜Nq⌝ ⊢ ⌜Np⌝. So the second leg of the pro-irrationalist argument fails (if, that is, my favoured logic is right). And so does the first leg, too, since disjunctive syllogism is not correct for simple negation. On the other hand, strong negation, '¬', is marked off by DeMorgan, a weakened version of involutivity (⌜p≡¬¬p⌝, where '≡' is a biconditional functor such that it is a necessary and sufficient condition for ⌜p≡q⌝ to be true that the world fails both to contain p without containing q and the other way round), excluded middle (⌜p∨¬p⌝) and the Cornubia principle, viz. ⌜p∧¬p→q⌝. Therefore, strong negation differs from simple negation mainly by being endowed with the Cornubia principle, owing to which disjunctive syllogism and contraposition are correct for that kind of negation.

Now, the reasoning eventuating in the irrationalist conclusion I am wrestling with in this paper alleges that a number of practices, which have later on emerged as scientifically fruitful, didn't comply with the so-called scientific standards. Here, though, negation is to

be taken as weak or simple. For one thing, not all standards are equally in force, equally valid or even equally held as valid; some are more staunchly and unswervingly adhered to than others. For another, there are sundry different degrees of transgressing a norm. Suppose the norm forbids doing so and so. Then you infringe the norm in so much, and in so much only, as you do so and so. But, whenever doing so and so comes in degrees, breaking the norm is a matter of degree, too. Well, culprits such as Galileo, for instance, doubtless did up to a point break some sound rules. But the situation was new, some of the rules themselves had by then deservedly become dubious, or were beginning to come under suspicion, evincing a need for qualification. What Feyerabend has termed Galileo's propaganda and rhetoric encompassed good and cogent arguments, although by no means conclusive. But nor were conclusive his adversaries' allegations either. As so often, each side had good reasons for their respective position. The conservative party clung to some entrenched standards but paying the price of to some extent disregarding some good evidence. The innovators were encroaching upon some well-established and well-tested rules, but only up to a point. They complied with most received standards while deviating more or less slightly from certain less well-confirmed methodological norms.

Much the same happens today, too. It seems to me safe to say that physics is in disarray, that it is now rife with paradoxes to such a point that dazzling and bold new hypotheses are badly needed. I hope new logical approaches like transitive logic will be found helpful here as elsewhere. Withal, complex or imaginary quantities can also be resorted to, thus overcoming some limitations or difficulties with relativity theory. Those newfangled approaches are fated to in some degree come up against some well-established methodological norms. However, by and large, they can to a considerable extent satisfy the loose and fuzzy set of scientific standards. No iconoclasm is needed in order for science to advance. Just some little, cautious and partial infringement of some of the current rules.

As logic is concerned, revolution is well under way. Or rather a quickened evolution. Revolutions are far less revolutionary than revolutionaries would dream. Which doesn't mean they are useless or redundant or idle. No, no, they play an indispensable role. Only, a revolution is never an absolute breaking-off. Under the apparent interruption, things continue following their course as before, although with a number of important differences of degree. The last decades have witnessed previously unforeseeable developments in logic. A lot of alternative fuzzy logics have been successfully applied to many scientific fields. On the other hand, paraconsistent logics have been born (those with a negation, '~', for which the Cornubia rule is not valid, namely the rule $\ulcorner p \urcorner, \ulcorner \sim p \urcorner \vdash \ulcorner q \urcorner$). To myself, the best prospect awaits such logics as are both paraconsistent and fuzzy, i.e. such as can deal with degrees of truth while retaining excluded middle and noncontradiction for each and every negation. (Notice that for a system to be paraconsistent doesn't entail for it to jettison the principle of noncontradiction; nor does the converse entailment hold either, as is well-known since Łukasiewicz's three-valued logic in the early twenties. Quite the opposite is nearer the truth: Łukasiewicz's systems are not paraconsistent, whereas changing them in order to return validity to noncontradiction is bound to issue in a paraconsistent system, or even, under certain conditions, in an outright contradictorial system wherein there is some theorem, $\ulcorner p \urcorner$, such that $\ulcorner Np \urcorner$ is a theorem, too.)

What binds fuzziness up with paraconsistency is the **taking-up rule**, namely that whatever is, up to a point at least, thus and so is indeed thus and so. In other words, what is not entirely false is true. Oddly, perhaps, that rule, or principle, is often put forward as

a reason for rejecting fuzzy or gradualistic approaches. Yet it is reconcilable with such an approach provided it is a paraconsistent one.

Most logicians are still hanging on to what seems to me an outdated paradigm, that of **overconsistency**, the rejection of any negational inconsistency. What is more, most of them also reject fuzzy logics and the like. Outside the professional field, many philosophers are nonplussed by such logical developments, which to their ears sound as incongruous tales or wonderland stories. Yet, such innovations as are embodied into the new logics are in fact relatively small. Even though there is perhaps no single logical law unanimously accepted by all systems (even identity is questioned in a Schrödingerian logic), every system accepts most of what is classically admitted, with clashes between classical logic and the new systems being the exception rather than the rule. A good measure of conservatism is thus secured.

I think that all other scientific revolutions pale beside the current renewal in logical thinking, since it cannot help bearing on all research fields and activities. Philosophy for instance can profit by gaining a much enlarged spectrum of alternatives, previously ruled out. Intractable problems can this way begin to get new solutions, formerly inconceivable or else pertaining to philosophers outside the orthodox field of logic-adherents (philosophers like Plato, or Nicholas de Cusa, or Hegel). So I dare say that we paraconsistent logicians have in fact gone beyond all the bold innovative dreams of the irrationalistic iconoclasts. Nonetheless, we are not iconoclasts. We are very very conservative-minded in many respects. And so was Galileo, and so was Leibniz, and so was Lobachewski, and so was Einstein, and so were all scientific innovators. Only irresponsible quacks come up with proposals purporting to offer something completely and utterly new.

Being a revolutionary in logic, I feel heartfelt sympathy for innovators in other scientific fields. I have already mentioned the urgent need for a revolutionary proposal in physics. Transitive logic has also been suggested to offer new interesting ways for linguistics and for some social sciences (history, economy, political theory, law-theory). I also think that people like Feyerabend have done a very good job of path-blazing, by denouncing pernicious academic ultraconservatism. However there is no need to say farewell to reason. By going so far, they lessen their own enterprise's chances of success, since most sensible people will balk at the very idea of abandoning reason or even of breaking off with the set of rationally acceptable methods as received in the scientific community. I hope this paper has shown that a revolutionary conservatism, unyieldingly rationalistic, is feasible and much more attractive.

Let me finish with a warning. I've insisted that most differences are of degree. But that doesn't mean that I replace the old all-or-nothing views with one espousing an undifferentiated magma. Such an obliteration would again amount to advocating the principle that anything goes. Were all differences of degree unimportant and negligible, then, yes, my approach would in the end boil down to a variety of such nihilism. But no, the lesson to be learned is that degrees do matter. And yet they are just what they are, mere degrees.