INCOMMENSURABILITY -
AN ALGORITHMIC APPROACH

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Although most authors agree that Hanson, Kuhn and Feyerabend are responsible for the introduction of the principle of incommensurability in the philosophy of science, they seldom agree about the exact content of its meaning. In this paper I want to propose a definition in the form of an algorithm that captures the various meanings of (in)commensurability presented in the literature. Using this definition, I then want to address two questions : is an elementary form of comparability always possible and — knowing that full comparability can never be achieved — just how far can we push a comparison. A pragmatic approach will be defended in these matters.

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It seems reasonable to suppose that we arrive at the conclusion that two "things"[1] are incommensurable when we have tried to compare them and are forced to conclude that the result of the comparison is negative. The first part to examine is the attempt at comparison. What do we do when we compare two things ? First of all, we have to agree what the two things we intend to compare, are supposed to be. Though this may seem extremely trivial, the examples that follow show this not to be the case. Having identified the things, we have to decide on what grounds the comparison will take place. In other words, a set of criteria has to be selected on which the comparison will be based. If all these conditions can

be met, the comparison can be executed. And, in the best of cases, we want to arrive at a single measure, allowing us to rank the things on a single scale. Then, and only then, we may say that one thing ranks higher than an other[2]. Formulating this procedure in terms of an algorithm, we arrive at the following :

(1)   identify the things to be compared
(2)   decide on a set of criteria on which the comparison will be based
(3)   execute the comparison
(4)   reduce to a single scale.

Two things will be called incommensurable if the above algorithm fails.

It then follows straight away that two things can be incommensurable in various ways : we fail to identify the things, or we don't fid suitable criteria, or we cannot execute the comparison, and so on, including of course combinations of these types of failure.

The first question to be answered obviously is : how well does this definition capture the various, existing interpretations of incommensurability. I will defend my case by presenting three examples taken from the current literature[3].

Example 1 : the approach of Thomas Kuhn.

(1) Kuhn is rather clear about what the things are supposed to be : they are either paradigms, or in a more restrictive sense, scientific theories. Although these things are fairly complex objects, Kuhn apparently assumes that we have some intuitive notion of them.
(2) The criterium is expressibility of both paradigms or theories in a common language.
(3) The execution of the comparison consists of a point-by-point comparison.
(4) The reduction to a single scale is rather unclear. Since the point-by-point comparison will produce a list of "yes" and "no", there must be some sort of adding rule in order to arrive at a single number.
Evidence[4] :

"The point-by-point comparison of two successive theories demands a language into which at least the empirical consequences of both can be translated without loss or change." (Kuhn, 1970b, p. 266).
"In applying the term 'incommensurability' to theories, I had

> intended only to insist that there was no common language within which both could be fully expressed and which could therefore be used in a point-by-point comparison between them." (Kuhn, 1977, pp. 300—1).

Kuhn holds theories to be incommensurable. For him the algorithm fails in its third step : the faithful and exact translatability of both theories in a common language is impossible, as for arguments, Kuhn relies on Quine's intranslatibility thesis[5]. The question whether this position makes sense, will be discussed later.

As a side-effect of the algorithm proposed here, I would like to show that Kuhn himself understands 'incommensurability' incorrect to a certain point. Kuhn says :

> "Most readers of my text have supposed that when I spoke of theories as incommensurable, I meant that they could not be compared. But 'incommensurability' is a term borrowed from mathematics, and it there has no such implication. The hypothenuse of an isosceles right triangle is incommensurable with its side, but the two can be compared to any required degree of precision. What is lacking is not comparability but a unit of length in terms of which both can be measured directly and exactly." (Kuhn, 1977, p. 300—1).

It is worth recalling here what incommensurability in the mathematical sense means. Two line segments — such as the side and the hypothenuse of an isosceles right triangle — are incommensurable if there is no rational number equal to the ratio of the lengths of these segments. If we set the length of the side of the triangle equal to 1, then the hypothenuse has length equal to $\sqrt{2}$, an irrational number. Another way of saying that two line segments are incommensurable, is that the following algorithm fails :

(1) take two line segments
(2) compare the segments as follows : set the length of one of them equal to 1 and determine the rational number that expresses the length of the other segment measured with the first one as unit
(3) execute this comparison
(4) rank the two lengths on the scale of rational numbers.

Of course, I have formulated the algorithm in such a way that it is easily seen to be an instance of the general algorithm I have proposed. Kuhn is surely right in seeing a connection between his meaning of incommensurability and the mathematical one. But, there is a crucial difference. In Kuhn's sense, the algorithm will always fail because full translatibility can never be achieved :

"Translation, in short, *always* involves compromises which alter communication. The translator must decide what alterations are acceptable." (Kuhn, 1970b, p. 268).

But this is not the case in the mathematical sense. For certain pairs of line segments the algorithm will indeed fail, but for others it will work perfectly. Take e.g. a segment and its double; this gives a ratio of 1 to 2. Although this way seem a minor difference, I believe it shows that the general algorithm presented here does a good job in accentuating these differences.

Example 2 : the approach of Larry Laudan.

The originality of Laudan's work lies in his problem solving approach to science. It allows him to define in a very sharp manner what the progressiveness of a scientific theory or a scientific research tradition is, an important achievement indeed. As to the incommensurability problem, his answer is equally important. In terms of the algorithm, it looks like this :

(1)   The things to be compared are scientific theories or research traditions.

(2) The criterium will be the relative problem-solving effectiveness of both theories or traditions over a set of shared empirical problems. It is important to note here that the comparison involves only a part of both theories or traditions. It is not Laudan's intention to present a full comparability, i.e. covering the complete empirical domain of both.

(3) The execution will consist of two steps : first, determine the set of shared problems and second, calculate the problem-solving effectiveness over this set. The second task presents no problems, but the first task needs some comment. To decide whether a problem is shared by two theories, is equivalent to decide whether it is the same for both. In this sense, we now have to compare problems. This again can be presented as an instance of the general algorithm, thus (to avoid confusion, literals are used to indicate the steps of the algorithm) :

(a)   The things to be compared are problems.

(b)   The criterium consists of the formulation of the problem in a language neutral with respect to the two given languages or theories in which the problems are expressed.

(c)   The execution will very often be straightforward. E.g., all theories of light, presented in the late seventeenth century, addressed

the problem of reflection, which can be easily formulated in a neutral way as "why is light reflected off a mirror or other polished surface according to a regular pattern".

(d) The reduction to a single scale is trivial, since the answer will be either "yes" or "no", i.e. the problems are the same or they are not.

When this subalgorithm has run and the problem-solving effectiveness has been calculated, the last step becomes quite trivial :

(4) rank the two numbers expressing the effectiveness on a single scale.

Evidence for the main algorithm :

> "... the weaker claim being made is this : with respect to any two research traditions (or theories) in any field of science, there are some joint problems which can be formulated so as to presuppose nothing which is syntactically dependent upon the research traditions being compared." (Laudan, 1977, p. 144).
> "These shared problems provide a basis for a rational appraised of the relative problem-solving effectiveness of competing research traditions." (ibidem, p. 144).

Evidence for the subalgorithm :

> "If a problem can be characterized only within the language and the framework of a theory which purports to solve it, then clearly no competing theory could be said to solve the same problem. However, so long as the theoretical assumptions necessary to characterize the problem are different from the theories which attempt to solve it, then it is possible to show that the competing explanatory theories are addressing themselves to the same problem. " (idem, p. 143).

In contrast to Kuhn, Laudan is convinced that the algorithm will work for certain inputs (for certain sets of shared problems) — in that sense Laudan's idea of incommensurability is much closer to the mathematical one — but, in case it doesn't a second algorithm is ready to take over. Its basic procedure consists in attaching a number — expressing the progressiveness — to each theory or research tradition separately and then simply in comparing these numbers. In Laudan's words :

> "Now, an approximate determination of the effectiveness of a research tradition can be made within the research tradition itself, without reference to any other research tradition. We simply ask whether a research tradition has solved the problems which it set for itself; we ask whether, in the process, it

generated any empirical anomalies or conceptual problems. We ask whether, in the course of time, it has managed to expand its domain of explained problems and to minimize the number and importance of its remaining conceptual problems and anomalies. In this way, we can come up with a characterization of the progressiveness (or regressiveness) of the research tradition. If we did this for all the major research traditions in science, then we should be able to construct something like a progressive ranking of all research traditions at a given time." (Laudan, 1977, pp. 145—146).

I assume the reader agrees with me that the above quotation can be rewritten as an algorithm for comparing two research traditions : just calculate the progressiveness of both and compare the numbers. One might be puzzled over the fact that a single number has to be arrived at, but Laudan suggests the following :

"... progress can occur if and only if the succession of scientific theories in any domain shows an increasing degree of problem solving effectiveness." (ibidem, p. 68).

and

"... the overall problem-solving effectiveness of a theory is determined by assessing the number and importance of the empirical problems which the theory solves and deducting therefrom the number and importance of the anomalies and conceptual problems which the theory generates." (ibidem, p. 68).

So, in principle, this second algorithm would allow us to compare research traditions or theories even if they have completely different domains !

Example 3 : the approach of Mary Hesse.

(1)   For Hesse, the things to be compared are (theoretical) terms in theories, e.g. mass in Newtonian and Einsteinian physics.
(2)   The criterium is the meaning of the terms, both in an extensional and in an intensional way. The extensional part needs no comment, but the intensional part is seen here as the locus of the term in a semantical network (or conceptual scheme or fabric)[6] relating the term to the other terms of the theory.
(3)   The execution of the comparison consists in checking whether the terms have the same meaning. This is realized by correlating the term in both theories to a third term that shares properties with the

other two. The only constraint on this third term is that its reference or extension lies within the intersection of the extensions of the given terms.

(4)   The reduction to a single scale might be realized by introducing a meaning difference scale, which starts at zero if the terms are identical, and goes to infinity as the terms turn out to be more and more different in meaning.

Evidence :

> "There are many objects which are within the reference of 'mass' in both Newton's and Einstein's theories, where 'mass' is used of these objects denote it by '$mass_i$'. Then there are many statements of the two theories that are logically comparable, ..." (Hesse, 1974, p. 65).

Hesse then introduces '$mass_n$' and '$mass_e$' for mass resp. in the Newtonian and Einsteinian sense and states :

> "Then Newton's theory will contain a statement ' ',$mass_i$" and "$mass_n$" are the same property', whereas Einstein's theory will have no use for '$mass_n$', and conversely ... It then follows, as required, that some statements containing 'mass' are logically comparable in the two theories and some are logically incommensurable." (Hesse, 1974, p. 65).

It must be added here right away that Mary Hesse does not believe tht the outcome "the terms are identical" is possible. In the best of cases, we will find out whether two terms have some meaning in common. The following quote makes this clear :

> "There is no 'essential meaning' of a term in any one theory that must survive into subsequent theories". (Hesse, 1974, p. 65).

In other words, together with Laudan, she endorses the view that a partial successful comparability is possible, whereas Kuhn believes that only an approximation to success is reachable.

Summarizing, I believe I can rather safely claim that the proposed general algorithm captures well the different specific proposals that exist. The three examples also illustrate the necessity for keeping the general algorithm as general as it is. E.g. it is necessary to speak of "things" because they can be very different things indeed.

3

Having a general definition has at least one advantage : if one

manages to prove something about it, it is valid for all possible
special cases. And this is precisely the aim of this section. More
specifically, I want to show that the idea of complete incommensur-
ability is not tenable. This idea is usually associated with Paul Feyer-
abend and it leads of course straight to his famous "anything goes".
Let me first of all try to formulate this position more clearly. Feyer-
abend does not deny the possibility of comparison, for he says :

> "Quite the contrary, I tried to find means of comparing ...
> theories. Comparison by content, or verisimilitude was of
> course out. But there certainly remained other methods."
> (Feyerabend, 1978, p. 68).

but he claims that the comparison will always contain subjective ele-
ments. In terms of the algorithm this means that the second step
— where the criteria are introduced and justified — will to a certain
extent be subjective :

> "Transition to criteria not involving content thus turns theory
> choice from a "rational' and 'objective' routine into a complex
> decision involving conflicting preferences and propaganda will
> play a major role in it, as it does in all cases involving arbitrary
> elements." (idem, p. 69).

This, of course, reduces the effect of the comparison to nil. If you
tell me that you have compared two things and you have found out
that they are the same, then I can always criticize your criteria and
disagree. Although I am rather sympathetic with this view, I do
believe that it cannot be pushed to the extreme. In other words,
there is a lower bound and the central argument is very well summa-
rized by Smith :

> "If there is no way of telling whether by 'mass' a Newtonian
> means the same thing as an Einsteinian, then when the former
> assents to the sentence "Mass is a constant property of an
> object" and the latter dissents from it there is no way of telling
> whether they are even assenting to and dissenting from the same
> statement; they are talking past each other. Showing that
> statements from different theories are incompatible appears to
> involve at least the possibility of translating from one to the
> other. The possibility of translation, however, is precisely what
> incommensurability denies." (Smith, 1981, p. 8).

What Smith, sharing his view with Shapere, tells us is this : if mass, in
the Newtonian and Einsteinian sense are really that different, then
why worry about the fact that no comparison is possible. It is
obviously the case. Stated generally, we must at least start on the

assumption that the things to be compared have something — whatever that might be — in common in order to start worrying what it exactly is they have in common. In terms of the algorithm, this means that the two things we start with are preselected. We don't start with any two things you can think of. Of course it doesn't make sense to compare a chair with Einsteinian physics ![7]

The nice thing about the algorithm that I have proposed is, that this preselection can be expressed in it :

(1)   select two things
(2)   produce reasons for believing that the things have something to do with one another (these reasons may be of a very informal nature)
(3)   examine the reasons
(4)   use a scale that consists of two labels only : "different" and same" and rank the two things under one of these headings.

Let me call this algorithm the zero-order algorithm. What I mean by this will be explained in the next chapter. Smith's argument can now be translated as follows :

(T)   If we want to discuss the (in)commensurability of two things, we must at least assume that the zero-order algorithm runs with success.

(T) expresses the existence of the lower bound I referred to. Paraphrasing Feyerabend : "Anything goes, but (T) certainly goes." An additional argument — but not so strong as Smith's — can be invoked : suppose that (T) does not hold. It then follows that, given any two things, they are basically incomparable. A consequence thereof is that it is no longer possible to speak of, say, Newtonian physics as such, because Newtonian physics-at-time-t would be incomparable to Newtonian physics-at-time-t+1, simply because in between t and t+1 a problem of the theory that was not yet solved has now been solved. Although one might argue that in principle, this is possible, it would make any statement in or about scientific theories so complex that we would no longer be able to handle it. As I said, the argument is not as strong as Smith's, for one might say "yes, indeed, that is what the situation is like". Even if I have to admit this, it will still be the case that physicists in practice will not share this view for the simple reason that the complexity of the matter will force them to immobility.

If the existence of the lower bound is accepted, it seems natural to ask if we can improve on it. That is the problem of the next

chapter.

4

  To state the problem more clearly, I must invoke an analogy. in mathematics, there exists an algorithm to compute an arbitrary close approximation of $\sqrt{2}$. The algorithm looks like this :

  (1) start with a rough estimate x_0

  (2) compute $x_1 = (x_0^2 + 2)/2x_0$

  (3) use x_1 as a new estimate.[8]

If we start with $x_0 = 1$ and let the algorithm run once, we obtain $x_1 = 1.5$. Using x_1 as a new, better estimate, we find $x_2 = 1.416$ and so on. After each run of the algorithm, we obtain a better approximation to the value of $\sqrt{2}$. With a slight modification, we can use the analogy for the general comparison algorithm. The zero-order algorithm can be compared to a rough estimate, since the result of a run of that algorithm is the statement whether the two things have something in common or not. This knowledge can now be used to run the algorithm once more in order to obtain a better approximation, i.e. to obtain more detailed information in what respect(s) the two things are comparable or not. But, as said, a slight modification must be made : if we run the zero-order algorithm once more, it is very unlikely that we will get better information because it uses only "rough" reasons and a very rough scale (just two labels) For a successful second run, the algorithm must be modified. Evidently, there are two ways to achieve this :

(I1) by sharpening the reasons on the basis of which the comparison is to be executed

(I2) by sharpening the scale on which the results of the comparison are to be ranked.

We can now speak of a first-order algorithm that is the result of a sharpening of the reasons and/or scale of the zero-order algorithm. Likewise we can talk about a second-order algorithm and so on. Fig. 1. shows a graphical representation of the overall procedure :
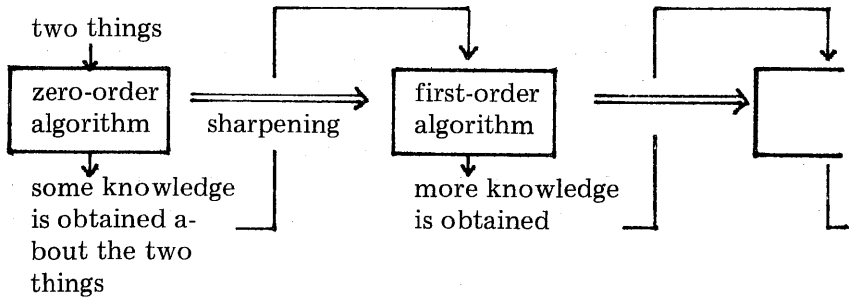
fig. 1.

This will be the setting for the discussion to follow.

It seems reasonable to claim that an improvement in (I2)-sense does not make sense if it is not accompanied by an improvement in (I1)-sense. What good would it do to have a very precise scale if I have bad reasons for ranking things on it. So any improvement must involve an improvement in the (I1)-sense. However an improvement in (I1) need not entail one in (I2). It may very well be the case that I only want to find out whether two terms have the same meaning or not and not how different in meaning they are, but that I still want to have very good reasons for deciding just whether they have the same meaning or not. I insist on this point because it shows that commensurability when it is 'fine-tuned' can take on two different forms : either a fine-criterion, rough-scale comparison, or a fine-criterion, fine-scale comparison. It may very well be that the first form (which is clearly weaker than the second form) is sufficient for practical purposes so that it is not necessary to go into a lengthy defence of the second form. I will not develop further this point but concentrate on the (I1)-type of improvement. What will be said for (I1) will at the same time be valid for (I2), since improving a scale in the end turns down to producing criteria by which the scale is to be refined[9]. So, the problem comes down to this : what are we to understand by "sharpening the reasons".

An obvious interpretation is that we want more and better reasons, better in this sense that they must allow us to decide in a more precise manner how two things are to be compared. In terms of the Kuhnian example we want at each refinement a better translation. An equally obvious drawback — and this is basically what the

incommensurability problem is all about — is that the more reasons
there are, the more criticisms can be made. In the case of Laudan e.g.
it is fairly easy to claim that the problem of the reflection of light
is the same problem whether it is treated in a Newton's or in
Huygens' theory. But if we try to formulate more sharply the
reflection problem in Newton's sense may we still say that this is
identical to the familiar intuitive problem. This is precisely the point
made by Thomas Nickles, when he claims that when a problem is
made more precise, background knowledge is introduced thus allow-
ing the criticism again that it is impossible to decide whether two
problems are the same because the whole of the background know-
ledge has to be assumed :

> "This means that we must break down the distinction between
> problems per se and conditions arising out of their historical-
> theoretical background. We must recognize that at least some
> theoretical conditions also help to set the problem. But once
> this distinction is breached, I can see no general grounds for
> excluding any constraint on a problem solution from the
> definition of the problem." (Nickles 1980, p. 12).

In the extreme case, one might argue that only (T) holds, but as
soon as we move to a higher order algorithm, the convincingness
of the result falls away. Otherwise said, (T) represents a lower bound
that cannot be improved. The point I want to make is that this is
not the case. It seems possible to me on the one hand to agree with
Feyerabend in saying that at the higher levels 'propaganda' sneaks
in, and on the other hand, to claim that the comparison remains
sufficiently convincing. The core of the argumentation is this : if
'propaganda' or value judgments enter the picture, there will still be
a certain group for whom the result of the comparison remains ac-
ceptable and that is precisely the group that agrees with these value
judgments. This point is trivial of course — if someone shares my
opinions, there is no need to convince — unless we can say something
about the size of the group. If it could be shown that the presence
of value judgments does not prohibit a large group of agreeing with
the result of the comparison, then it seems reasonable to speak of
sufficiently convincing. Obviously some people will drop out, but
the idea is to try to minimize the 'loss'. The question is how.

The answer I propose, though not the only one possible, relies
on a pragmatical view of the matter. In what follows, I will restrict
the things to scientific theories and add some comment for the
general case. In constructing the answer for the restricted case, my

preferred witness will be Nicolas Rescher.
First of all, as trivial as it may seem, scientific theories are intended
to be applied. And Rescher says :

> "... at bottom the progress [of science] at issue does not pro-
> ceed along theoretical but along practical lines. Once one sees
> the legitimation of science to lie ultimately in the sphere of
> its applications, the progress of science will be taken to center
> on its pragmatic aspect — the increasing success of applications
> in problem solving and control." (Rescher, 1977, p. 185).

And he goes on stating :

> "This pragmatic dimension endows science with the continuity
> it may well lack at the contextual level of the technical machi-
> nery of its ideas and concepts — a continuity that finds its
> expression in the persistence of problem-solving tasks in the
> sphere of praxis." (idem, p. 187).

A beautiful illustration is presented :

> "The sending of messages is just that whether horse-carried
> letters or laser beams are used in transmitting the information."
> (idem, pp. 187—8).

Secondly, applications usually take on very concrete forms : an
application can result in the construction of some kind of machinery,
or in a drastic change in an environment, and so on. Take the
example of a bridge and suppose (as it does) that Newtonian
mechanics explains to me why the bridge is not collapsing. Suppose
further that there is a second theory I want to compare with
Newtonian mechanics and suppose that it is not capable of
explaining the 'bridge-problem'. This then is a clear and sufficient
basis for comparing the two theories. But, the sceptic will say, why
is this reasoning supposed to work for 'bridge-problems' and not for
'reflection-of-light-problems' ? For just one, but very important
reason : because the bridge is used (perhaps daily) by the members
of the culture in which the scientific subculture is imbedded.
Furthermore, it is relatively easier to identify a 'bridge-problem'
separately from any theory that tries to explain it. The concrete
object, namely, the bridge, is lying there and a transition from
Newtonian to Einsteinian physics in the scientific subculture does
not affect that concrete thing out there. That, I take it, is the
continuity Rescher finds so convincing. One cannot argue that by the
transition in theories, the problem disappears. This may very well be
the case within science itself, but it seems a pretty hard job to have
the bridge disappear !

As Rescher puts it :

> "The question of technological superiority ... is something
> far less sophisticated, but also far more manageable, than the
> issue of theoretical superiority. The assessment of technical
> superiority is relatively easy vis-à-vis that of theoretical super-
> iority, because the issues involved function at a grosser and
> more rough-and-ready level than those of theoretical meaning-
> content." (Rescher, 1977, p. 186).

Thirdly — and this is the important step in the reasoning — I claim
the following holds : the values shared by a certain community are
in some way or other linked to the technical products of that
community. I will not enter into the discussion what is the exact
nature of this relationship, since it is sufficient here that some kind
of link exists. In other words, part of the value judgments a certain
group shares, depends on the products they commonly use — such as
bridges, automobiles and so on — and these products in turn are the
result of the application of certain scientific theories.

We are now close to the answer. Suppose we want to compare
two theories and we agree on (T). We start to refine the reasons (and
eventually the scale) and assume that some value judgment sneaks
in. By the fact that we belong to a certain community and therefore
share the technical products of that community, themselves the
result of the application of the scientific theories under discussion,
we will share some values, according to the argument developed
above. If the invoked value judgment lies within these values, we can
continue to discuss. If not, of course, we are in trouble. The point
is, that the introduction of value judgments in the comparison, does
not automatically entail that convincingness is lost. In a number of
cases it will be possible to continue. And, if we can continue, we are
more or less guaranteed that our audience will consist of all those
persons that share with me the specific applications connected with
the introduced value judgment. In that sense, the 'losses' are mini-
mized.

The following consequences are straightforward : (i) the convin-
cingness of the comparison algorithm depends on the familiarity
one has with the applications involved in the comparison. If we talk
about cars and somebody from a culture that doesn't know anything
about cars, is presented with a comparison algorithm involving cars,
then it need not come as a surprise that the convincingness
disappears. (ii) The number of levels you can move up in a
comparison will be dependent on the two theories that are being

compared. If e.g. the two theories have few applications or if the relation between the theories and the applications is very complex then of course the comparison algorithm will loose its convincingness pretty fast[10]. (iii) As we go to higher levels, more value judgments will appear. Some of these will be of a strict personal nature. This in turn implies that my audience gets more restricted, since there is a low probability that I will find somebody who will exactly share those values. In the end, I will only be convincing myself !

As to the general case, matters are far more complex. Depending on what the two things are, it may be more or less easy to execute a convincing comparison. If I have to compare two T.V. sets as to their performance, that is not a problem. But if I have to compare the term 'wave function' as it is presented in the various formulations of quantum mechanics, matters become very complex indeed. However, what can be said, is that some generalization of the above procedure for scientific theories is possible. If, in the course of a comparison, value judgments are introduced, I can always according to the method outlined above, try to determine the maximal group that accepts those judgments. If we would then be able to establish a connection between the things compared and some set of shared things — such as the technical products in the case of scientific theories — then we can try to further maximize the group. The difficulty lies in establishing the connection. As far as I can see, little can be said here is general.

<div align="center">5</div>

By way of conclusion, I would like to stress the fact that although the aim of this paper was to show that the thesis of full incommensurability is not tenable, it does not follow that full comparability is the ultimate goal. The complexity argument that has been adduced for (T) applies equally well here : in order to reach a complete comparison, things might become so complex that again we are forced to immobility. I have therefore focused the discussion on the problem how to go from zero-level to a certain number of levels higher and not how to go from zero-level to the final level, granted that there would be one.

A second remark of similar type concerns the fact that this paper has not dealt with the complementary problem, i.e. is it possible to decide convincingly that two things are incomparable.

I believe this to be an equally important problem. It might save a scientist a lot of time if he or she knows that an alternative theory that at first sight seemed comparable to his or hers own theory, turns out to be incomparable at a higher level (algorithmically speaking, that is). It is interesting to note that the discovery of the incommensurability of 1 and $\sqrt{2}$, opened up new and fascinating areas in the mathematical field.

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NOTES

[1] I use here the word 'things' to avoid any identification with a particular notion such as theories, terms, problems, fields, networks and the like. Insofar as I am talking about things, the reasoning applies to objects other than the ones mentioned here as well.

[2] 'Higher' does not refer here to a quality statement. It is not implied that the thing that ranks higher is therefore necessarily the better. It only refers to the fact that any scale must possess an internal order structure.

[3] The examples are chosen so as to represent the major views in the incommensurability debate. A point that is not discussed in this paper is the problem whether other things besides the current ones, can be presented as input for the algorithms. In other words, we need not necessarily focus on theories or problems and the like.

[4] I call the quotations that illustrate the algorithm 'evidence' because the algorithm-like formulation is mine and is extracted from the quotations. I also want to leave open the question whether or not the authors mentioned would agree with my formulation in terms of an algorithm.

[5] "Quine has recently concluded 'that rival systems of analytic hypotheses ... can conform to all speech dispositions within each of the languages concerned and yet dictate, in countless cases, utterly disparate translation... Two such translations might even be patently contrary in truthvalue.' One need not go that far to recognize that reference to translation only isolates but does not resolve the problems which have led Feyerabend and me to talk of incommensura-

bility." (Kuhn, 1970b, p. 268).

Whether this support that Kuhn draws from Quine is correct, is open to discussion. Ian Hacking, e.g., believes that Quine's thesis is the opposite of the incommensurability thesis :

"Both writers [Kuhn and Feyerabend] once suggested that incommensurability should be understood in terms of schemes and translation. Incommensurability meant that there would simply be no way of translating from one scheme to another. Thus this idea pulls in a direction exactly opposite to Quine's. Indeterminacy says there are too many translations between schemes, while incommensurability says there are none at all." (Ian Hacking, 1982, p. 59).

[6] A conceptual fabric is the term used by Barry Barnes. His approach is very close to Mary Hesse's.

"... a conceptual fabric, a structure made up of generalisations which connect concepts into a single integrated whole." (Barnes, 1982, p. 71).

Compare this to what Mary Hesse says about networks :

"Briefly, the model interprets scientific theory in terms of a network of concepts related by laws, in which only pragmatic and relative distinctions can be made between the 'observable' and the 'theoretical'." (Hesse, 1974, p. 4)

[7] At first sight, that is. If e.g. we take both objects as illustrations for a general theory of man-made artefacts, then eventually they might turn out to be comparable.

[8] The formula in step (2) is easily discovered. If x_n is the n-th estimate, then this means that there is an O, such that $\sqrt{2} = x_n + O$. Squaring this formula, leads to : $2 = x^2_n + 2 x_n O + O^2$. Ignoring the O^2 — if O is small, O^2 will be much smaller — we find that $O = (2 - x^2_n)/2 x_n$. The new estimate x_{n+1} is precisely $x_n + O$ or $x_n + (2 - x^2_n)/2 x_n$, i.e. $(x^2_n + 2)/2 x_n$.

[9] The example par excellence is the following. Suppose that a ranking is arrived at consisting of a pair of numbers, say (a,b). The reduction to a single scale involves the problem of relating $<$a,b$>$ to $<$a',b'$>$ when a $<$ a' and b $>$ b'. The only way out is to have some kind of ranking in the first and second component of the ordered couple. If e.g. the first component is more important than the second, the fact that a $<$ a' is sufficient to justify the conclusion that $<$a,b$>$ $<$ $<$a',b'$>$. But this ranking is additional and needs to be motivated. Otherwise said, criteria are wanted.

[10]An interesting consequence is that new theories are very difficult to compare. Since they are new, the number of applications will presumably be small and thus the comparison, according to the procedure outlined here, will be difficult.

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