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APPROXIMATE APPLICATION OF EMPIRICAL THEORIES: A GENERAL EXPLICATION*

1. INTRODUCTION

The rôle and structure of approximation in empirical sciences is a neglected field in the philosophy of science. Not even a systematic account of the different kinds of problems involved is known to me. Some effort has still to be made to clearly state the problems supposed to be solved.

The responsibility for this neglect should not be adscribed to the philosophers alone. Theoretical scientists share some part of the blame. Philosophers of science normally look at what theoretical scientists write in their textbooks and treatises – or at least, they should do so. But theoretical scientists do not write much about approximation questions. These they leave as a kind of boring subsidiary work for experimental scientists and engineers. Only sometimes, when two different theories 'should' be related in some simple and direct way, but are obviously not so, some rather unclear words are said about 'limiting cases', 'constants tending to O or infinity', etc. There is not much philosophical insight to be gained from such phrases. In any case, laws, theories, and empirical results are very often presented without saying a word about their approximative character and the problems involved in it. A similar picture is offered by many historians of science, who write much about, say, Newton's discovery in the seventeenth century that the planetary system 'obeys' the law of gravitation, but usually forget to tell us about the painstaking efforts made during the 18th century and afterwards to establish that Newton's law actually fits the astronomical observations 'with quite good an approximation'.

Of course, everybody knows – or should know – that scientific laws and theories can only be applied to reality up to a certain degree of approximation, and that competition between different laws, theories, and 'research programmes' is, in many cases, nothing but competition to attain a better degree of approximation – not the 'truth', whatever this

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may be. However, this fact is taken by systematic accounts in the philosophy of science as an uninteresting accidental feature of the scientific enterprise – something not really belonging to the 'essence' of science. I believe this view to be fundamentally misleading. It is misleading to take for granted that the canonical situation the philosopher should account for, is the situation in which a scientific theory with perfectly exact concepts perfectly fits the facts it is supposed to systematize. There is no present significant theory in which this really happens, and I see no reason to suppose this will ever happen. To speculate about this possibility seems a waste of time to me. And putting empirical science without approximation as an ideal to be reached by future scientists, is nothing but a part of the speculative teleology that W. Stegmüller has described as the "teleological myth of increasing verisimilitude" – the idea that there is one absolute goal to be attained by science some time in the future (cf. e.g. [9], p. 36).[1]

It does not seem to be an audacious generalization to hold that scientific knowledge in general, quantitative as well as qualitative, is essentially approximative. In fact, this is so obvious, that nobody really cares much about it. For some people, especially working scientists, it may appear as a truism. But one of the tasks of philosophy is to carefully analyze and explicate alleged truisms. And I do not think this task is, in the present case, completely trivial.

As far as I know, the first author to have seriously taken approximation as an *essential* feature of empirical theories, was Prof. G. Ludwig in [3]. The second chapter of his book includes a sketch of a semi-formal explication of the structures of a physical theory, containing an approximative element as a constitutive part.[2] Leaving aside Ludwig's general (and rather sketchy) explication of the theory concept, which is not our concern here, the main point I wish to stress is his (implicit) idea that any explication of the concept of empirical theory must take the approximative element into account, otherwise the explication will be incomplete. To put it crudely: a realistic concept of empirical theory necessarily includes a concept of approximation. I think this is an important philosophical insight. It was one source of inspiration for the present study.[3]

The program is, then, to define appropriate approximation concepts and structures to be included in adequately defined theoretical structures.

A possible reaction against this program could be the following objection. The approximation talk does not refer to the theory itself but to its applications; approximation notions should be kept out of theoretical structures.

This objection rests on the assumption that the applications of a theory do not conceptually belong to the notion of the theory itself. That is, an empirical theory should be treated like a mathematical theory, as a mere formal structure (a 'mathematical formalism') determined by some axioms. However, for many reasons which I can not discuss here, this purely 'formalist' concept of an empirical theory does not seem to be the most adequate one.[4] Anyway, this is not the theory concept on which this article rests. We take an (advanced) empirical theory to be an entity composed of *both* the formal conceptual structure and its domain of applications. Approximation notions, then, have to be explicated as something that refers to both the formal structure and the domain of applications of the theory.

2. DIFFERENT KINDS OF APPROXIMATION

In the previous introductory remarks, we have talked about approximation in rather vague terms. The time has come to be more systematic. First of all, the logical status of the approximation concept seems to be that of a dyadic relation: Something is an approximation of some other thing. (It may be that in a formal reconstruction we shall be constrained to introduce more terms in the relationship; but then we shall see.) It also seems obvious that scientific research uses many different kinds of approximation, in many different contexts. But perhaps they are not so many. Let us try to get at a crude typological classification of different approximation cases. At first sight, at least, I see four main types.

(a) There is the case where we try to systematize some empirical data within a given conceptual framework, and in the process we have to make some 'idealizations' and 'simplifications' (read: approximations) in order to get a manageable model. For example, we approximate the movement of a macroscopic body by considering it as a particle on a continuous path; or from a finite number of geodetical measurements we conclude that the Earth is an ellipsoid – forgetting all its mountains and valleys; or we consider a visible ray of light in our room as a geometrical straight line;

and so on. This level of approximation (or 'idealization', if you prefer) may be called 'model construction'. It builds the roots of science, but we shall not be concerned with it here. The reason is just that I have nothing to say about its formal structure.[5]

(b) At the next level we approximately apply some law or theory to a 'constructed model', that is, we try to subsume a conceptually systematized collection of data under a propounded law or theory. For example, we try to make plausible that the body moving around us is a case of Galilei's law, or that the Earth's movement around the Sun obeys the law of gravitation, or that we can apply the law of reflection to a(n) (idealized) ray of sun light.

(c) When we consider a law as an approximation of another, more complicated law of the same theory (that is, the two laws belonging to the same conceptual framework), we have an approximative relation on a purely theoretical level. In such cases, we do not worry much about the empirical facts outside the theory – we are only doing mathematics within the theory. This comparison of laws is very common in physics. For example, Galilei's law is taken as an approximation of the law of gravitation; or the ideal gas law is taken as 'approximately valid' with respect to the law of Van der Waals, and this is itself an approximation of a still more complicated expression – the virial equation of state; or the law for the simple pendulum

$$\tau = 2\pi\sqrt{\frac{l}{g}}$$

is an approximation of

$$\tau = 2\pi\sqrt{\frac{l}{g}}\left(1+\frac{1}{4}\sin^2\frac{\alpha}{2}\right)$$

which is in turn an approximation to a power series. Many examples of this sort can be found in any physics textbook. It is frequent in such cases that the simpler or 'less exact' law derives from the more complicated by substituting a fixed numerical value (e.g. 0 or 1) for a parameter (a constant or a real function) which appears in the approximated law. For example, Van der Waal's equation

$$\left(p+\frac{a}{V^2}\right)\cdot(V-b)=R\cdot T$$

reduces to the law of ideal gases by taking $a = b = 0$. Not all cases are so simple, of course. But commonly the 'approximation trick' consists in changing (and fixing) the value of some parameter of the law.

We are speaking here of a given law approximating another given law. But in many cases the expressions approximating or approximated do not really deserve the label of 'laws'. They do not appear in the systematic exposition of the theory, they are just calculating devices introduced for some special purpose – usually approximation needs. Formally, there is no clear-cut distinction between laws and special calculating devices; but sometimes we really would not like to call some special equation a law. Take the case of the systematization of the movement of a motor vehicle where we take the mass as time-dependent (fuel mass being lost). Should we call the resulting dynamical equation a physical law?

As a terminological convention let us speak in general of 'theoretical systematizations' of empirical states of affairs described in some previously given conceptual framework. This label is intended to cover 'serious' laws as well as calculating devices introduced *ad hoc*, for particular applications of the theory. We shall say that one theoretical systematization is an approximation of another one in cases of the sort we have just reported. In the following, we abbreviate 'theoretical systematization' to 'th. sys').

As already noted, approximation between th. sys.'s is, in a certain sense, a purely theoretical endeavour, a paper-and-pencil operation, as P. W. Bridgman would say. Nevertheless, this fact does not preclude us from admitting an intimate relation between this kind of approximation and the approximation of the second kind mentioned, that is, the approximative application of a th. sys. to an empirical constructed model. Suppose we have applied some law L_1 to an empirical domain D with some success, but we are still not very satisfied with the degree of approximation obtained; we may try to construct a second law L_2 such that L_1 is taken as an approximation of L_2, and L_2 applied to D gives a better degree of approximation. Or, inversely, L_1 applies 'quite well' to D, but the calculations with L_1 are very tedious: We would like to have a simpler, even if cruder, way of systematizing D. So, we look for a L_0 which, while being a simpler approximation of L_1, can be applied to D with a degree of approximation that is sufficient for our purposes. A more formal description of this relation between the two levels of approximation will be given below.

(d) Finally, there is a kind of approximative relation at the most theoretical level, which does not seem to be reducible to the approximative relationship between laws. I mean approximation between two general theories, i.e. between whole structures with different conceptual framework and different fundamental laws. Let us call it '*intertheoretical approximation*' – while kinds (b) and (c) could be described as '*intra*theoretical approximation', and (a) would be a *pre*theoretical approximation'. Intertheoretical approximation probably holds between classical and special relativistic mechanics, between phenomenological thermodynamics and statistical mechanics, between geometrical optics and electrodynamics. These examples are frequently subsumed under the label 'reduction'. But it seems fairly clear that these cases can not be subsumed under *exact* reduction as it has been reconstructed by E. W. Adams in [1] and by Sneed in [6]. Perhaps, we could speak here of 'approximative reduction', but the problem of the logical relationship between approximative and exact reduction is still an open question. Moreover, there is no reason to suppose that all intertheoretical approximations should be considered as a 'blurred' reduction.

The problem of giving a formal reconstruction of intertheoretical approximation in general remains untouched. The concrete case of Kepler's laws being reduced to Newton's gravitation theory – I take this as an example of intertheoretical, not intratheoretical approximation – has been carefully analyzed by E. Scheibe in [4]. Scheibe's reconstruction of this case is very stimulating indeed. But it is doubtful whether he has succeeded in filtering out a general concept from this particular example, as he has tried to do elsewhere (cf. his article [5]). There is another proposal in this direction, viz. that of Ludwig in [3], pp. 120ff. Ludwig first defines a concept of exact '*Einbettung*' ('embedding') of one theory into another, and he afterwards gives some conditions for 'blurring' this notion into a '*unscharfe Einbettung*' ('approximative embedding'). In [9], Stegmüller has pointed out the promising program which lies behind this notion; but Ludwig's explication is a very brief and sketchy one. It only can be viewed as a start to be further developed and completed. I hope to be able to contribute to this program in the future.

This article does not deal with the problem of giving a formal explication of intertheoretical approximation. The discussion is restricted to categories (b) and (c), i.e. to the notions of approximative application of a theory and of approximative relationship between th. sys.'s.

3. MODEL-THEORETIC FUZZY SETS

We have now to be more precise about the logical category of the entities to be compared with each other in an approximative relationship of kinds (b) or (c).

A first thought could be to take *statements* as the entities to be compared. In a case of kind (b) we would say that the statement of an empirical law (say, the gravitation law) is approximative relative to the statement describing a physical system in mathematical terms (e.g. planetary orbits described in terms of particle mechanics). This would be the exact form of the explicandum we have to explicate.

But this explicandum does not seem a very adequate candidate. Can we really give a precise meaning to the claim that some statement is the approximation of some other statement? It is likely that we would get into clumsy semantical trouble here.

Of course, a possible way to give a semantical analysis of 'statements approximation' would be in terms of relations between the *models* of the statements. This is, indeed, similar to the direction I propose in this article. But then, if we explicate approximation between statements as a model-theoretic relation, we can forget about 'approximation of statements' altogether and work with models alone. That this is possible, I shall try to show in the following.

The basic idea is to take not statements but set-theoretically defined models as the minimal entities to be compared in an approximative relationship. This approach to the matter is in accord with the so-called structuralistic view in the philosophy of science first developed by Sneed in [6]. At least in its first step, the present explication could also be backed by the more traditional approach of Suppes and his collaborators. To make this point clear, I shall briefly resumé Suppes' method.

As Suppes himself puts it (e.g. in [12], p. 2–25) the slogan of his approach could be so stated: To axiomatize a theory is to define a set-theoretical predicate – with the axioms written down as definitory conditions of the predicate; a model of the theory is then a tuple (a 'structure') whose members satisfy the conditions of the set-theoretical predicate.[6] In [12], Suppes follows standard model-theoretical terminology and distinguishes between a *model* and a *possible realization* of a set-theoretical predicate (= of a theory). This distinction is especially relevant to the analysis of empirical theories and will prove very important for our present aims. The axioms or conditions defining the set-theoretical predicate of a theory can be divided in two classes: the general conditions describing the logico-mathematical categories to which each primitive term belongs and the proper axioms of the theory stating specific properties and expressing the 'real content' of the theory. Let us call the first class 'structural axioms', the second one 'material axioms'. The first class determines a possible realization of the set-theoretical predicate; it is only when we add the material axioms that we determine a model. Every model is a possible realization, but not conversely.

Suppes' 'possible realizations' have been renamed 'possible models' by Sneed-Stegmüller. We follow their terminology. If M is the class of models of a set-theoretical predicate (= of a theory), M_p is the class of its possible models. In general, we have $M \subset M_p$.

In the case of an empirical theory, a possible model is a possible mathematical description in the framework of *this* theory of a state of affairs. It is a *possible* description because, by applying the conceptual framework of the theory to that given state of affairs, we still have not settled the question of whether this description actually renders a model of the theory or not. If metric functions appear as primitives in the theory, there are uncountably many different possible descriptions of a given state of affairs. This is well-known from the discussion of the structuralistic view. The point I wish to stress here, is that, among the host of different possible descriptions which may correspond to a state of affairs, some pairs of them will be considered more similar to each other than other pairs; and the 'grade of similarity' between possible mathematical descriptions of some facts may be an important question to consider when we try to apply the theory to those facts.

For example, if we try to apply a dynamic theory to the movement of some physical system S consisting of a set of particles P during some time interval T by help of a position function s, a significant question to ask is what grade of similarity between two possible descriptions of this system may be required. A possible answer may be given in the following terms. If x and y are two possible descriptions of S (= two possible models) we shall be content with their similarity if they satisfy the following condition for some previously given real numbers ε and δ:

[S] $$P_x = P_y \wedge T_x = T_y \wedge \bigwedge p \in P_x, t \in T_x: \\ [s_x(p, t) - s_y(p, t)] < \varepsilon \wedge [D^2 s_x(p, t) - D^2 s_y(p, t)] < \delta)$$

We shall then say that x is an approximation of y, and conversely. Note that, in order to state this approximation condition, no material axiom (empirical law) of the theory is needed. That means in our terminology that we are working at the level of M_p alone.

The present approach towards a general explication of approximative notions mainly rests on the proposal of taking approximation as a relation between two possible models of a theory, i.e. as a dyadic relation on M_p. The concrete features of the theory are totally irrelevant for the realization of this program. We do not need to suppose that we are dealing with a 'quantitative' theory (i.e. a theory with metric functions) nor that approximation rests upon some special measurement methods – as it is sometimes presupposed when talking about approximation. The generality level of the present explication is such as to be applied to very crude and qualitative theories as well. The only assumption needed is that we can axiomatize the theory and distinguish the structural from the proper axioms.

Now, we are in a position to clearly state the explicandum we are after. It is a metatheoretical statement of the sort: 'potential model x is an approximation of potential model y in theory T'. What is the way to its explicans? The set-theoretical point of view will help us on this question.

Potential models of a theory T are all elements of the same set M_p of T. This means that we want to define an approximation relation between any two elements (or 'points') of a given set. There is a well-known method for defining such a relation in topological analysis: to introduce the concept of *uniform structure* as a specialization of a filter. Admittedly, the intended applications of this notion in topology are sets of real numbers. But, is there any cogent reason for not applying it also to sets of different entities, if required? I do not see any such reason, assuming that we are willing to accept that (a) the topological concept of uniform structure is the adequate explication for intuitive approximation in sets of points, and (b) the class of potential models of a theory axiomatized by means of a set-theoretical predicate is a well-defined entity. These two assumptions seem quite obvious to me. In any case, I do not intend to discuss them here. We reconstruct model-theoretic approximation by defining a uniform structure on potential models. In the following, we abbreviate 'uniform structure' to 'uniformity'.[7]

Before going to the formal definition of a model-theoretic uniformity let us grasp an intuitive picture of it. For a given set (in the present case, M_p), a uniformity determines a whole array of subsets, each of them representing a 'degree of approximation' or 'measure of fuzziness'. These subsets may be called 'fuzzy sets'. A fuzzy set consists of pairs of elements of the original set (M_p). If a pair $\langle a, b\rangle$ is in a fuzzy set U, this means that a and b approximate each other in *at least* the degree given by U. We could alternatively say: a and b coincide at least up to U. Or still: a and b are U-nearly equal. In the case of a set of real numbers, where we have a standard metric (the absolute value of the difference) we can define the standard uniformity where each fuzzy set is determined by a particular ε:

$$U_\varepsilon = \{\langle a, b\rangle / [a-b] < \varepsilon\}.$$

But this is a special case only. The concept of a uniformity does not depend on the concept of a metric; it is more general. The determination of empirically relevant fuzzy sets can be much more complicated than the previous example, or even not expressible in metrical terms.

Such a general concept is precisely what we need for explicating the comparison of potential models. For the general treatment of approximation we do not need to bother about the specific terms of the models (mainly functions) involved in the approximation. Besides brevity, this abstract treatment has some other advantages. The same degree of approximation, i.e. the same fuzzy set U may be settled by taking the approximation on different functions of the theory. For example, in a gravitational study of the system Earth-Sun we may state the approximation condition by considering $1/m_{\text{sun}} \simeq 0$, or alternatively by considering the force which the Earth exerts on the Sun 'very small'; the same degree of approximation is obtained by considering masses and by considering forces. Still more important, in a general treatment we do not need to assume that the fuzzy sets used in some applications of the theory are determined by any completely specifiable conditions: it may be the case that a specific fuzziness depends on the 'personal knowledge' of the scientist, on his 'intuitions', changing pragmatic needs, and so on. All we need to know is that the fuzzy sets used are elements of a uniformity associated with the M_p of the theory. We need some special notation for the formal definition of the uniformity.

If M is a set, then $\Delta(M)$ is the 'diagonal' of M, i.e. the set of all pairs of identical elements of M.

If R is a dyadic relation, R^{-1} is the inverse, and

$$R \circ R = R^2 = \{\langle x, y\rangle / \bigvee z(\langle x, z\rangle \in R \wedge \langle z, y\rangle \in R)\}$$

R^n can be defined analogously.

The following axioms correspond essentially to those given by N. Bourbaki in [2], p. 131.

(D1) $\mathfrak{U}$ is a uniformity on M_p iff

(1) $\emptyset \neq \mathfrak{U} \subseteq \mathfrak{P}(M_p \times M_p)$

(2) $\bigwedge U_1, U_2(U_1 \in \mathfrak{U} \wedge U_1 \subseteq U_2 \rightarrow U_2 \in \mathfrak{U})$

(3) $\bigwedge U_1, U_2(U_1 \in \mathfrak{U} \wedge U_2 \in \mathfrak{U} \rightarrow U_1 \cap U_2 \in \mathfrak{U})$

(4) $\bigwedge U(U \in \mathfrak{U} \rightarrow \Delta(M_p) \subseteq U)$

(5) $\bigwedge U(U \in \mathfrak{U} \rightarrow U^{-1} \in \mathfrak{U})$

(6) $\bigwedge U_1 \bigvee U_2(U_1 \in \mathfrak{U} \rightarrow U_2^2 \subseteq U_1 \wedge U_2 \in \mathfrak{U})$.

We call the elements of $\mathfrak{U}$ '(uniform) fuzzy sets'. Note that a fuzzy set is a dyadic relation on M_p.

The first three axioms essentially define a filter – with a little variation in axiom (1) (see [2], p. 32). The following axioms (4)–(6) are specific of the uniformity concept.

Let us try to interpret this formal structure in terms of model-theoretic approximation to see whether it appears intuitively adequate. First, note that the pragmatic ground for associating a uniformity $\mathfrak{U}$ with the set M_p of a theory T, is to allow for a liberalization in the application or manipulation of T. Should we assume that the theory applies exactly to its domain, or that the relationships between its special laws are always of an exact kind, we would bump into falsification quite rapidly. Formally, this would mean that we only allow the use of the diagonal – as a 'degenerated' fuzzy set – in the manipulations of the theory. The use of the diagonal represents absolute exactness. But real scientists are more liberal than that. They use much bigger fuzzy sets in their manipulations of the theory. The size of the fuzzy sets used depends on the different kinds of applications and manipulations of the theory. The bigger the fuzzy set, the easier to stay out of trouble. Of course, we should suppose that if a certain limit is exceeded, the theory is considered useless. But this is a point we neglect for the moment; we shall discuss it in the next section.

As a kind of slogan for quickly interpreting the axioms of (D 1) let us say that: To associate a uniformity with a theory is a way to immunize it against troubles in its applications. (The limits to this immunization will be discussed later.) Suppose we have constructed uniformity $\mathfrak{U}$ for theory T, and we are especially using some $U \in \mathfrak{U}$ for a given application of T.

This means that we have 'immunized' this application up to degree U. For example, the application may consist in the following. (This is only *one* possible case.) Given a system S, we make some theoretical calculations using T and predict that S will have the mathematical form described by potential model a. But then, after some 'empirical observations', we come to the conclusion that S should be better described by potential model b, being $a \neq b$. If $\langle a, b\rangle \in U$, then we are satisfied – at least for the moment. If $\langle a, b\rangle \notin U$, we may have two possible reactions: Either to be still more liberal and admit that, after all, we would also be satisfied with a $U' \in \mathfrak{U}$ such that $U \subset U', \langle a, b\rangle \in U'$; or to reconsider T and/or the mathematical description of S. Both kinds of reactions are copiously exemplified in everyday science.

The more pairs $\langle a, b\rangle$ you let in U, the safer you are; and, of course, the less you can really do with the theory.

Leaving aside the purely structural axiom (D 1)-(1), let us 'translate' the rest of them into the 'immunization language'. (D 1)-(2) states that if the theory is immunized by a fuzzy set U_1, it will also be immunized by a bigger U_2 including U_1. (D 1)-(3) states that if you immunize T by using U_1 and by using U_2, you may put both of them together and immunize T by using their intersection. This axiom is, perhaps, less obvious than (D 1)-(2), but it seems quite plausible – at least as an idealization. (Note that the intersection can not be vacuous, by (D 1)-(4): It always contains at least the diagonal.) If you are satisfied with approximation degree U in some application, whatever this degree may be, you will also be satisfied with exact application, if this casually and fortunately happens; that is, exactness is included in every fuzzy set. This is the content of (D 1)-(4).[8] Axiom (D 1)-(5) is obvious in the present interpretation: The order of the models of a pair in a fuzzy set has no significance at all for the 'immunization-degree' of the theory. (D 1)-(6) is a critical axiom. It roughly states that for any given fuzzy set U_1 immunizing T, a sharper U_2 may be found such that U_2 still immunizes the theory; U_2 is at least twice as sharp as U_1.

For an easy appraisal of this last axiom, consider a very simple example. Suppose the model-theoretical fuzzy sets of a geodesical description are determined by an approximation condition on the position function of 'particles' or 'points':

$$U_\varepsilon = \{\langle x, x'\rangle / \bigwedge p \in P_x \cap P_{x'}([s_x(p) - s_{x'}(p)] < \varepsilon)\}.$$

Then, if U_ε is to be a fuzzy set of a uniform structure, axiom (D 1)-(6) asserts the existence of a U_δ of the same sort as U_ε but twice as sharp as it, and this means in this special case:

$$U_\delta = \left\{\langle x, x'\rangle / \bigwedge p \in P_x \cap P_{x'}\left([s_x(p) - s_{x'}(p)] < \delta \leq \frac{\varepsilon}{2}\right)\right\}.$$

In this particular example, axiom (D 1)-(6) guarantees the formal possibility of determining the position of a given p with ever increasing exactness. Such an axiom certainly represents an idealization of actual scientific practice, because it implies the possibility of making ever sharper applications of the theory. (But note that it does *not* imply that we shall ever attain Δ, i.e. exactness.)

The mathematically relevant properties of uniformities in general may be found in any advanced topology textbook. For the present study we only need some simple corollaries.

Let $\mathfrak{U}$ be a uniformity and U any element of it.

(Th. 1) $\emptyset \notin \mathfrak{U}$.

This directly follows from (D 1)-(1), (4).

(Th. 2) $U^n \subseteq U^{n+1}$.

This follows by induction from (D 1)-(4), definition of U^2 and of U^n in general.

(Th. 3) $U \in \mathfrak{U} \rightarrow \bigwedge n(U^n \in \mathfrak{U})$.

This follows from (Th. 2), (D 1)-(2).

4. FUZZY SETS ON TWO LEVELS

Up to this point, the present analysis of approximation has rested on no special approach in the philosophy of science, but only the very general assumptions (a) and (b) p. 209. Now we proceed to further develop the analysis of approximation within a particular metatheoretical framework: Sneed's structuralistic approach. We shall see that this approach is also capable of enlightening interesting aspects of approximation.

In the following, the reader is supposed to have some knowledge of the main tenets of the structuralistic approach as expounded mainly in [6] and

[8]. In more recent writings (e.g. in [7]) some formal modifications have been introduced into the former conceptual framework; but they are not essential for the present discussion.

Neither need we all complexities of the structuralistic approach – for example, the notions of *constraint* and *law specializations*. The basic ideas assumed here are: the distinction between a theoretical and a non-theoretical level within a given theory, i.e. the distinction between the set M_p of *possible models* and the set M_{pp} of *partial possible models* in the Sneed–Stegmüller terminology;[9] the notion of an *empirical theory* (or *theory-element* in the new terminology of [7]) as an ordered pair $\langle K, I\rangle$, where K is the so-called *core* (the mathematical apparatus of the theory) and I is the range of intended applications of the theory. The whole *empirical claim* of a theory is resumed in the 'holistic' statement that I can be subsumed under some specialization of K. All this we assume as already known.

The structuralistic approach in its original form does not give any account of approximation. If we think – as I do – that the Sneedian approach gives us a valuable framework for reconstructing empirical science, then we face the problem of supplementing it with approximation structures. To put it more conspicuously, we have two aims to get at here: First, to introduce an approximative component into the structuralistic notion of an empirical theory in such a way that it accords with the theoretical- non-theoretical distinction; secondly, to formally give an approximative version of the empirical claim of a theory. The first program is undertaken in this section; the second one is left to the next section.

By introducing the distinction between the set of possible models, M_p, (i.e. the theoretical level) and the set of partial possible models, M_{pp} (i.e. the non-theoretical level) a new possibility for expressing approximation is opened to us. For partial possible models are nothing but possible mathematical descriptions of empirical systems in quite the same sense as possible models are. The only difference is that the mathematical description is offered in this case without any theoretical terms. Hence, it should be clear that the same reasons that lead us to talk about 'nearness' or approximation of possible models, and of fuzzy instead of exact description, can also be argued for the case of partial possible models, viz. non-theoretical descriptions. Indeed, we have already seen an example of approximation stated in purely non-theoretical (kinematical) terms in the case of particle mechanics (condition (S) on paths in p. 208).

Consequently, it is reasonable to formally express non-theoretical approximation by using a uniformity defined on M_{pp} in quite the same way we have done it for M_p. We introduce a relation class $\mathfrak{V} \subseteq \mathfrak{P}(M_{pp} \times M_{pp})$ satisfying axioms analogous to those of (D 1). Its elements V_i may be called 'non-theoretical fuzzy sets'.

This means that, in principle, we can associate two kinds of uniformities with an empirical theory: a $\mathfrak{U}$ for M_p and a $\mathfrak{V}$ for M_{pp}. Our intuitive expectations lead us to suppose that there should be some relationship between a uniformity on M_p and a 'corresponding' uniformity on M_{pp}, or still more intuitively: Approximation on the M_p-level is done for the sake of approximation on the M_{pp}-level.

To make these intuitions somewhat clearer, let us consider the 'paradigmatic' example of Newtonian gravitation theory and its applications once again.

Let G be the set-theoretic predicate for 'gravitational Newtonian particle mechanics' as it has been axiomatized by Sneed in [6], p. 140–141. The possible models x for this predicate are tuples of the sort $x = \langle P, T, s, m, f \rangle$, where P is a set of particles, T is a time interval, $s(p, t)$ denotes the position of particle p at time t, $m(p)$ denotes the mass of p, and $f(p, t, i)$ represents a particular force acting on p at t. D^2s is the acceleration function.

Suppose we want to apply G to the physical system consisting of E, the Earth, and S, the Sun. We construct an $x_1 = \langle \{E, S\}, T, s, m, f \rangle$ and claim $G(x_1)$. This claim entails, among other things, that the mutual forces which Earth and Sun exert on each other are non-zero:

$$\sum_i f(E, t, i) = \sum_i f(S, t, i) \neq 0.$$

Since we admit that the respective masses are finite, this implies in turn (by Newton's Second Law, which is a definitory condition of the set-theoretic predicate G), that the respective accelerations of Sun and Earth are non-zero: $D^2s(E, t) \neq 0$, $D^2s(S, t) \neq 0$.

If we translate this theoretical calculation into a description of the system in purely kinematical terms, we obtain a partial possible model $r(x_1) \in M_{pp}$ mathematically described by two ellipses: a big ellipse for the Earth and a 'very small' ellipse for the Sun.

Since the Sun ellipse is so 'small', physics textbooks tell us that, for some applications, it can be 'neglected'. We suppose the Sun to be stationary, and this means that we construct an x_0 with the condition $\sum f(S, t, i) = 0$ instead of

$$\sum_i f(S, t, i) = -G \cdot \frac{m(E) \cdot m(S)}{[s(E, t) - s(S, t)]^2}.$$

On the other hand, we continue to assume that

$$\sum_i f(E, t, i) = -G \cdot \frac{m(E) \cdot m(S)}{[s(E, t) - s(S, t)]^2}.$$

This 'idealization' makes the theoretical calculation and the kinematical description much simpler; and this is sometimes a very good reason for assuming it – at least for didactic purposes. The kinematical translation of x_0, $r(x_0) \in M_{pp}$, is then a single ellipse for the Earth with the Sun on a focus of the ellipse.

Of course, the price we have to pay for this 'idealization' (= approximation) is that potential model x_0 is no more an actual model of G, but a model for a th. sys. which only approximates, but is not identical with G. In our general framework, this means that the uniformity we take when handling gravitation theory contains a fuzzy set U determined by some conditions such that $\langle x_0, x_1 \rangle \in U$, and that we actually use U at least sometimes for some purposes.

U is a theoretical fuzzy set determined by a condition about forces (or, alternatively, about masses). But it can be 'translated' into a non-theoretical fuzzy set V determined by a condition comparing a two-ellipses-system with a single-ellipse-system; and then we have $\langle r(x_0), r(x_1) \rangle \in V$.

A further question is this: Does either of these two kinematical descriptions of the Sun-Earth-system correspond to any intended application of G, to any 'really observed' system? The answer is clearly: No – at least not anno 1976.[10] The empirically 'constructed models' for the Sun-Earth-system do not fit either $r(x_0)$ or $r(x_1)$. The actual paths of Sun and Earth derived from astronomical observations are more complicated than the big-ellipse-plus-small-ellipse description, to say nothing of the single-ellipse description.

Let us call i the partial potential model corresponding to the intended application Sun-Earth-system in 1976. It is the case that $i \neq r(x_0)$, $i \neq$

$r(x_1)$. Nevertheless, x_0 and x_1 are sometimes used because they approximate i at least up to a certain degree. Again, this means formally that gravitation theory sometimes uses non-theoretical fuzzy sets V_0 and V_1 such that $\langle i, r(x_0)\rangle \in V_0$ and $\langle i, r(x_1)\rangle \in V_1$, and $V_1 \subset V_0$. We could also say that gravitation theory sometimes uses theoretical fuzzy sets U_0, U_1 such that for a theoretical extension of $i, e(i) \in M_p, \langle e(i), x_0\rangle \in U_0$ and $\langle e(i), x_1\rangle \in U_1$, and $U_1 \subset U_0$.

If we want to be more exact in the theoretical description of i, we may construct a potential model x_2 that takes the forces of the other planets on the Earth and the Sun into account; that is, we take a model x_2 of a th. sys. which approximates G while considering the external planetary forces on the Earth-Sun-system. The kinematical description derived from $x_2, r(x_2) \in M_{pp}$, will be more complicated than the two-ellipses-description; but, of course, x_2 will still not fit i exactly; it only fits i with a better approximation than the previous models. This means that we pick out a fuzzy set U_2 with $\langle e(i), x_2\rangle \in U_2$ and $U_2 \subset U_1 \subset U_0$; or, alternatively, a V_2 with $\langle i, r(x_2)\rangle \in V_2$ and $V_2 \subset V_1 \subset V_0$. And so on.

The essential moral to draw from the analysis of this simple example is roughly this. Approximation between th. sys.'s on the fully mechanical M_p-level induces approximation between kinematic descriptions on the M_{pp}-level. The last is in turn needed to compare the calculated kinematic descriptions with the really intended application we have got as a 'constructed model' from pre-theoretical data. In more general terms: Approximations on the theoretical level induce approximations on the non-theoretical level needed for zeroing-in a given intended application. These are, I think, the essential features of the approximation mechanism (within *one* theory).

However, from a general standpoint there is no guarantee that this mechanism will always work. There is, in principle, no guarantee that a uniformity on M_p will induce a 'corresponding' uniformity on M_{pp} – notwithstanding the intuitive plausibility of the idea. In fact, there is no general proof to the effect that this induction is always possible. The critical axiom (D 1)-(6) is the trouble-maker here: Assuming the present concept of uniformity, it can not be generally proved that ever increasing sharpness of the theoretical fuzzy sets induces an ever increasing sharpness of their corresponding non-theoretical restrictions. The basic reason for this impossibility lies in the fact that *many* theoretical models correspond to *only one* non-theoretical partial model. It is easy, but somewhat lengthy, to see that this fact has the over-mentioned effect. The reader may check this by himself.

Nevertheless, we should not give up our hopes too hastily. It can be shown that by restricting the original concept of uniformity with a further plausible condition one gets the wanted induction theorem.

The modified uniformity we call an '*empirical uniformity*' – 'empirical' in the sense that it is significant for empirical theories only (i.e. for theories with the distinction between theoretical and non-theoretical concepts). It can be proved, then, that an empirical uniformity on M_p induces a corresponding empirical uniformity on M_{pp}.

To state this, some auxiliary definitions are needed.

Let $\mathfrak{U}$ be a uniformity on M_p and U_i its fuzzy sets.

(D 2) $Rs(U_i) =: \{\langle y, y' \rangle / \bigvee x, x' (y = r(x) \wedge y' = r(x') \wedge \langle x, x \rangle \in U_i\}$.

Let us call $Rs(U_i)$ the 'restriction' of fuzzy set U_i. (Our aim may be roughly stated as the proof that the restriction of a fuzzy set is itself a fuzzy set.) Three trivial corollaries follow from this definition:

(Th. 4) $Rs(U_1 \cup U_2) = Rs(U_1) \cup Rs(U_2)$.

(Th. 5) $Rs(U_1 \cap U_2) \subseteq Rs(U_1) \cap Rs(U_2)$.

(Th. 6) $U_1 \subseteq U_2 \to Rs(U_1) \subseteq Rs(U_2)$.

(D 3) $\mathfrak{B}[\mathfrak{U}] =: \{V / \bigvee U (U \in \mathfrak{U} \wedge V = Rs(U))\}$.

Now, let us consider pairs of possible models that have exactly the same non-theoretical components, i.e. the same non-theoretical parts. They are, so to say, 'empirically identical'. Two possible models of such a pair can be considered as describing exactly the same possible state of affairs; formally, they are different, but, as to their empirical import, they are equivalent. If possible model x_1 approximates possible model x_2 by a certain degree U, and if we take an x'_1 such that $r(x_1) = r(x'_1)$, then the result of the approximation will be the same, i.e. x'_1 will approximate x_2 by the same degree U. A change in the choice of the theoretical functions which has no bearing on the non-theoretical determination of the physical system, is irrelevant for approximation matters. This is the intuitive content of the axiom we add to the original uniformity concept.

(D 4) $\mathfrak{U}$ is an *empirical uniformity* on M_p iff

(1) $\mathfrak{U}$ is a uniformity on M_p

(2) $\bigwedge U, x_1, x_2, x'_1 \, (\langle x_1, x_2 \rangle \in U \wedge r(x_1) = r(x'_1) \to \langle x'_1, x_2 \rangle \in U)$.

In entirely the same way, an empirical uniformity on M_{pp} can be defined. But it is obvious that the concepts of an empirical uniformity on M_{pp} and of a uniformity on M_{pp} are coextensive, since, in this case, $r(y) = y$.

Now we come to the 'induction theorem'.

(Th. 7) If $\mathfrak{U}$ is an empirical uniformity on M_p, then $\mathfrak{V}[\mathfrak{U}]$ is a(n) (empirical) uniformity on M_{pp}.

Proof: $\mathfrak{U}$ is assumed to satisfy the seven axioms of an empirical uniformity. It has to be shown that $\mathfrak{V}[\mathfrak{U}]$ also satisfies the uniformity axioms with respect to M_{pp}.

(1) $?\emptyset \neq \mathfrak{V}[\mathfrak{U}] \subseteq \mathfrak{V}(M_{pp} \times M_{pp})$.

By (D 3), it is obvious that $\mathfrak{V}[\mathfrak{U}] \subseteq \mathfrak{P}\,(M_{pp} \times M_{pp})$.

And by (D 1)–(1) and (Th. 1), there is a $U \in \mathfrak{U}$ such that $\emptyset \neq U$; it follows from (D 3) that $\emptyset \neq Rs(U) \in \mathfrak{V}[\mathfrak{U}]$.

(2) $?V_1 \in \mathfrak{V}[\mathfrak{U}] \wedge V_1 \subseteq V_2 \rightarrow V_2 \in \mathfrak{V}[\mathfrak{U}]$.

There is a $U_1 \in \mathfrak{U}$ with $V_1 = Rs(U_1)$ by (D 3).

Let $V_2 = V_1 \cup \{y, y'\rangle, \ldots\}$, where $y = r(x)$, $y' = r(x')$ for some x, x'.

Consider $U_2 = U_1 \cup \{\langle x, x'\rangle, \ldots\}$

$V_2 = Rs(U_2)$ by (D 2).

$U_2 \in \mathfrak{U}$ by (D 1)–(2).

$V_2 \in \mathfrak{V}[\mathfrak{U}]$ by (D 3).

(3) $?V_1 \in \mathfrak{V}[\mathfrak{U}] \wedge V_2 \in \mathfrak{V}[\mathfrak{U}] \rightarrow V_1 \cap V_2 \in \mathfrak{V}[\mathfrak{U}]$.

By (D 3) there are U_1, $U_2 \in \mathfrak{U}$ such that $V_1 = Rs(U_1)$, $V_2 = Rs(U_2)$ $\emptyset \neq U_1 \cap U_2 \in \mathfrak{U}$ by (D 1)–(3), (Th. 1).

This means $\emptyset \neq Rs(U_1 \cap U_2)$. Further

$Rs(U_1 \cap U_2) \subseteq Rs(U_1) \cap Rs(U_2) = V_1 \cap V_2$ by (Th. 5).

Take $V_0 =: Rs(U_1 \cap U_2)$.

We have $\emptyset \neq V_0 \subseteq V_1 \cap V_2$

$V_0 \in \mathfrak{V}[\mathfrak{U}]$ by (D 3).

$V_1 \cap V_2 \in \mathfrak{V}[\mathfrak{U}]$ by (D 1)-(2) for $\mathfrak{V}[\mathfrak{U}]$, which we have already proved.

(4) $?V \in \mathfrak{V}[\mathfrak{U}] \rightarrow \Delta(M_{pp}) \subseteq V$.

By (D 3), there is some $U \in \mathfrak{U}$ with $V = Rs(U)$

$\bigwedge x \in M_p$: $\langle x, x\rangle \in U$ by (D 1)–(4).

$\bigwedge x \in M_p$: $\langle r(x), r(x)\rangle \in Rs(U) = V$ by (D 2).

This means $\Delta(M_{pp}) \subseteq V$.

(5) $?V \in \mathfrak{V}[\mathfrak{U}] \rightarrow V^{-1} \in \mathfrak{V}[\mathfrak{U}]$.

This trivially follows from (D 1)–(5), (D 2), (D 3).

(6) $\bigwedge V_1 \bigvee V_2 (V_1 \in \mathfrak{V}[\mathfrak{U}] \rightarrow V_2^2 \subseteq V_1 \wedge V_2 \in \mathfrak{V}[\mathfrak{U}])$.

Take any $V_1 \in \mathfrak{V}[\mathfrak{U}]$.

By (D 3), there is a $U_1 \in \mathfrak{U}$ such that $V_1 = Rs(U_1)$.

By (D 1)-(6), we know there is a $U_2 \in \mathfrak{U}$ such that $U_2^2 \subseteq U_1$; $Rs(U_2^2) \subseteq Rs(U_1) = V_1$ by (Th. 6).

Consider $V_2 = Rs(U_2)$; $V_2 \in \mathfrak{V}[\mathfrak{U}]$, by (D 3), $U_2 \in \mathfrak{U}$; $V_2^2 = Rs(U_2)^2 = \{\langle y_2, y_2'\rangle / \bigvee v_2(\langle y_2, v_2\rangle \in Rs(U_2) \wedge \langle v_2, y_2'\rangle \in Rs(U_2))\} = \{\langle y_2, y_2'\rangle / \bigvee x_2, x_2', w_2, w_2'(y_2 = r(x_2) \wedge r(w_2) = r(w_2') \wedge \langle x_2, w_2\rangle \in \{U_2 \wedge \langle w_2', x_2'\rangle \in U_2)\}$ by (D 2).

From $r(w_2) = r(w_2')$ it follows, by (D 4)-(2), that $\langle w_2, x_2'\rangle \in U_2$. That is,

$Rs(U_2)^2 = \{\langle y_2, y_2'\rangle / \bigvee x_2, x_2', w_2(y_2 = r(x_2) \wedge y_2' = r(x_2') \wedge \langle x_2, w_2\rangle \in U_2 \wedge \langle w_2, x_2'\rangle \in U_2)\} = Rs(U_2^2)$.

Since $Rs(U_2^2) \subseteq Rs(U_1)$, we get

$V_2^2 = Rs(U_2^2) \subseteq Rs(U_1) = V_1$. Q.E.D.

An empirical uniformity on the theoretical level always induces a corresponding uniformity on the non-theoretical level in a natural way. Hence, I propose that, for approximation matters, the structuralistic theory concept should be supplemented with the notion of an empirical uniformity on M_p. To put it formally, an empirical theory is no longer a pair $\langle K, I\rangle$, but a triple

$$\langle K, \mathfrak{U}, I\rangle,$$

where $\mathfrak{U}$ is an empirical uniformity defined on M_p of K.

5. THE APPROXIMATIVE VERSION OF THE EMPIRICAL CLAIM OF A THEORY

A well-known aspect of the structuralistic approach is the reconstruction of the empirical content of a theory at a given time as a single 'holistic' proposition stating that the theory as a whole is applicable to its range of intended applications I (as a whole). If E_t denotes the so-called 'expanded core' of the theory at time t, then the theory-claim is, in Stegmüller's symbolism: $I \in A(E_t)$, where A is a set-theoretic operator applying the theoretical structure E_t to the non-theoretical level (see e.g.

[8], p. 136).[11] It is important to note here that the empirical claim of a theory is a historical time-relative entity.

The original reconstruction takes the empirical claim of a theory as an *exact* claim. This is, of course, a strong simplification. The purpose of this section is to 'blur' that reconstruction in order to get a more realistic account of the structure of the empirical claim. Let us try to do this by using the explicated approximation concept.

The basic idea is that the theoretical structure E_t is not applied exactly, but only approximately to the domain I; and this is done by using many different fuzzy sets of the empirical uniformity $\mathfrak{U}$ associated with the theory. But, of course, not every fuzzy set will do. For one thing, $\mathfrak{U}$ contains fuzzy sets too big (remember (D 1)-(2)) to be used in a reasonable way in the application of the theory. If you blur the application too much, nobody will take you seriously. Some fuzzy sets will be considered *admissible*, others not.

What degree of fuzziness will still be considered admissible, is an issue that heavily depends on the particular $i \in I$ which is supposed to be explained. When applying the law of gravitation to planetary motion, we do not expect the same degree of approximation as in the case we apply it to the attraction between two large lead spheres on the Earth's surface, making measurements by means of a delicate torsion balance.

However, the admissible degree of fuzziness not only depends on the particular application at stake. It also depends on the stage of scientific evolution. The expectations of the scientific community with respect to approximation change in time, according to varied and informal factors, like technological progress in detection and measurement, mathematical progress in the theoretical calculations, previous success attained by rival theories, etc. These are strongly pragmatic factors. It is hardly to expect that they will ever be completely and formally explicable. On the other hand, they are very important for the admission of a given fuzzy set for a particular application. In the reconstruction of the empirical claim of the theory, we should take them somehow into account.

Here, we are facing a problem similar to that of formally determining the concept of an intended application, which is also strongly pragmatic. This problem has been thoroughly discussed in [6] and [8]. All that can be demanded from the philosopher of science in such cases, is that he tries to find some (weak) necessary conditions for the determination of the concept in question. This is what I shall try to do here for the notion: '*class of admissible fuzzy sets* (for the application of a theory at a given time)'. Let us denote this class by $\mathfrak{A}$.

Obviously, this class must be a subset of the empirical uniformity on M_p – or, alternatively, a subset of the induced uniformity on M_{pp}. Further, as already discussed, a too high degree of fuzziness in any application would appear unacceptable to the scientific community. So, it seems plausible to postulate a *maximum* for the admissible fuzziness in particular applications. (Different kinds of applications will generally have different maxima.) Perhaps, it would also be plausible to introduce a minimum: It seems realistic to assume that we can never guarantee that full-blooded theories used by full-blooded scientists will exactly apply to any domain – at least, if they are not trivial. However, I do not want to formally preclude the possibility of exact application at least in some particular kinds of domains of some particular theories; so, I shall not propound the minimum requirement here. (The reader who thinks it is really a necessary condition for admissible approximation, may easily add it to the axioms below.) An obvious requirement is further that the admissibility of a fuzzy set should be invariant with respect to purely theoretical changes in its models; that is, if U fully coincides with U' in its non-theoretical parts, and U is admissible, then U' should also be. A last requirement which really seems necessary to me, is that an admissible fuzzy set should have some non-trivial significance to at least one intended application of the theory; that is, every admissible fuzzy set should be used to give a non-trivial approximation of at least one intended application. This is the intuitive meaning of the last axiom of the list below.

Since we do not want to preclude the possibility that other necessary conditions for the concept of admissible fuzzy sets could be postulated, we shall speak of a '*potential* class of admissible fuzzy sets'.

Let $\mathfrak{U}$ be an empirical uniformity on M_p of theory T and I the range of intended applications of T.

(D 5) $\mathfrak{A}$ is a potential class of admissible fuzzy sets of $\mathfrak{U}$ iff

(1) $\mathfrak{A} \subseteq \mathfrak{U}$.

(2) $\bigvee U_m(U_m \in \mathfrak{A} \wedge \bigwedge U(U_m \subset U \rightarrow U \notin \mathfrak{A}))$.

(3) $\bigwedge U, U'(U \in \mathfrak{A} \wedge U' \in \mathfrak{A} \wedge Rs(U) = Rs(U') \rightarrow U' \in \mathfrak{A})$.

(4) $\bigwedge U \bigvee x, x'(U \in \mathfrak{A} \rightarrow r(x) \in I \wedge r(x) \neq r(x') \wedge \langle x, x' \rangle \in U)$.

Observations: (D 5)-(2) allows for many different 'maxima' U_m, so long as they are not connected. In (D 5)-(4), the condition $r(x) \neq r(x')$ is necessary since otherwise the requirement would be trivially satisfied by identity pairs $\langle x, x \rangle$, which are all in every U.

In an entirely analogous way, a concept of admissible fuzzy sets could be defined for a uniformity of M_{pp}. Requirement (D 5)-(3) would then reduce to a tautology.

A question similar to that of the foregoing section can be raised now: Are classes of admissible fuzzy sets on the non-theoretical level related in some natural way to classes of admissible fuzzy sets on the theoretical level? The answer is: yes – so long as we restrict ourselves to the present list of axioms.[12] It can be shown that a potential class of admissible fuzzy sets on M_p induces its corresponding class of admissible fuzzy sets on M_{pp}.

Let $\mathfrak{A}$ be a subclass of uniformity $\mathfrak{U}$ on M_p. We define:

(D 6) $\mathfrak{B}[\mathfrak{A}] =: \{V / \bigvee U(U \in \mathfrak{A} \wedge V = Rs(U))\}$.

(Th. 8) If $\mathfrak{A}$ is a potential class of admissible fuzzy sets on M_p, then $\mathfrak{B}[\mathfrak{A}]$ is a potential class of admissible fuzzy sets on M_{pp}.

Proof:

(1) $\mathfrak{B}[\mathfrak{A}] \subseteq \mathfrak{B}[\mathfrak{U}]$, by (D 6), (D 5), (D 3).

(2) $?\bigvee V_m(V_m \in \mathfrak{B}[\mathfrak{A}] \wedge \bigwedge V(V_m \subset V \rightarrow V \notin \mathfrak{B}[\mathfrak{A}]))$.

From (D 5)-(2), we know there is at least one $U_m \in \mathfrak{A}$ such that

(A) $\bigwedge U(U_m \subset U \rightarrow U \notin \mathfrak{A})$.

Take $V_m =: Rs(U_m)$.

Suppose there were a V_0 such that $V_m \subset V_0$, $V_0 \in \mathfrak{B}[\mathfrak{A}]$.

This implies that there is a $U_0 \in \mathfrak{A}$ with $V_0 = Rs(U_0)$.

Since $V_0 \neq V_m$, also $U_0 \neq U_m$. Take $U_0 \cup U_m$.

By (Th. 4), $Rs(U_0 \cup U_m) = Rs(U_0) \cup Rs(U_m) = V_0 \cup V_m = V_0 = Rs(U_0)$.

Since $Rs(U_0 \cup U_m) = Rs(U_0)$ and $U_0 \in \mathfrak{A}$, also $U_0 \cup U_m \in \mathfrak{A}$ by (D 5)-(3); further, $U_m \subset U_0 \cup U_m$.

This contradicts (A).

(3) is trivial since $Rs(V) = V$.

(4) easily follows from requirement (D 5)-(4) for $\mathfrak{A}$. Q.E.D.

Now, suppose we are given a class of admissible fuzzy sets $\mathfrak{A}$ to approximately apply an expanded core E_t of theory T to the range I at a given time of the theory evolution. ($\mathfrak{A}$ is itself a time-relative entity.)

What does this mean? Roughly, this means that the exact claim $I \in A(E_t)$ is blurred by an approximative claim – in symbols: $I \tilde{\in} A(E_t)$ – constructed by means of $\mathfrak{A}$. How can '$I \tilde{\in} A(E_t)$' be made formally precise?

To this purpose, it is useful to introduce a special notation which leads to a sort of 'approximation logic' – in order to blur model-theoretic relations.

'$x \sim x'$' means: $\bigvee U(U \in \mathfrak{A} \wedge \langle x, x' \rangle \in U)$.[13]

For any model-theoretic predicates P, R, . . . :

'$P(\tilde{x})$' means: $\bigvee x'(x \sim x' \wedge P(x'))$.

'$R(\tilde{x}, y)$' means: $\bigvee x'(x \sim x' \wedge R(x', y))$.

It should be clear how $R(\tilde{x}, \tilde{y})$, $R(\tilde{x}, y, \tilde{z})$, etc. could be defined along the same lines.

And for model-theoretic classes X, Y, . . . we have:

'$X \sim Y$' means: $\bigwedge x \bigvee y(x \in X \rightarrow y \in Y \wedge x \sim y) \wedge$
$\bigwedge y \bigvee x(y \in Y \rightarrow x \in X \wedge x \sim y)$.

'$\bar{P}(\tilde{X})$' means: $\bigvee X'(X \sim X' \wedge \bar{P}(X'))$.

'$\bar{R}(\tilde{X}, Y)$' means: $\bigvee X'(X \sim X' \wedge \bar{R}(X', Y))$ etc.

To further develop this device in order to obtain a kind of 'approximation calculus' could prove to be an interesting task for its own sake. But this is not our present aim. What we want is to use this device for precisely reconstructing an approximative version of '$I \in A(E_t)$'.

Since the empirical claim of the theory is a dyadic $\in$-relation between a set and a class of sets, we can, in principle, blur it in three different ways: Either to blur the first relation term only, or only to blur the second one, or to blur both of them simultaneously. We have, then, following possibilities for expressing the approximative claim of the theory:

(a) $\tilde{I} \in A(E_t)$ i.e. $\bigvee Y(Y \sim I \wedge Y \in A(E_t))$.

(b) $I \in \widetilde{A(E_t)}$ i.e. $\bigvee \mathfrak{J}(\mathfrak{J} \sim A(E_t) \wedge I \in \mathfrak{J})$.

(c) $\tilde{I} \in \widetilde{A(E_t)}$ i.e. $\bigvee Y, \mathfrak{J}(Y \sim I \wedge \mathfrak{J} \sim A(E_t) \wedge Y \in \mathfrak{J})$.

It should be noted that (b) implies (a), but not conversely; and (c) has no logical relationship to (a) or (b). Each one of these statements has a formally precise meaning according to the previous definitions. The symbol $\sim$ allows for a strong abbreviation of the respective approximative propositions; otherwise, the expressions (a), (b), (c) would be lengthy and cumbersome. As an example, take the most simple of them, the first one, which runs as follows when explicited without $\sim$:

(a′) $\bigvee Y(Y \in A(E_t) \wedge \bigwedge y \bigvee y', V(y \in I \rightarrow y' \in Y \wedge V \in \mathfrak{B}[\mathfrak{A}] \wedge \langle y, y' \rangle \in V)$.

The explicit form of (b) and (c) is still more complicated. It is, however, totally precise.

The question now is, which of the three possible versions (a), (b), and (c) is the most adequate for representing the approximative claim of an empirical theory. (a) means that we take approximations only on the intended applications; this does not seem plausible, since approximation of laws is undertaken independently of the intended applications – as the example of gravitation law applied to the Earth-Sun-system has already shown. For quite the opposite reasons, (b) does not seem to be a good candidate either, since even if we take a theoretical approximation of a law, we shall not be warranted to infer that this approximation exactly applies to some intended application (remember the same example). So, it really seems that '$\tilde{I} \in \widetilde{A(E_t)}$' is the best candidate for expressing the approximative empirical claim of the theory: Its theoretical systematizations as well as its intended applications are generally blurred. In this way, there may be some hope for the scientist that "the theory fits the facts".

NOTES

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1 Unfortunately, the term 'approximation' contributes itself to the teleological myth of science coming closer and closer to an Absolute Goal waiting for us somewhere. But the explication of the approximative relations which I have in mind and which is (partly) exposed in this paper, is completely independent of any teleological ideas about science one might hold.

2 Prof. Ludwig only deals with physical theories. However, his explication is stated in such general terms that it should be applicable to any theory whose axioms are expressed in 'mathematical language'. Since set theory can be included in mathematical language, it follows that Ludwig's explication is relevant to almost every significant empirical theory.

3 Although the formulations in the present article do not have much in common with Ludwig's. They rather belong to the Suppes-Sneedian framework. My main debt to Ludwig is the idea of using the topological notion of a uniformity for reconstructing approximative relations in empirical theories. Of course, I am responsible for the concrete details of formulation and interpretation.

4 The reader interested in the arguments for introducing a 'more empirical' notion of empirical theory should read the relevant passages in the books of J. D. Sneed [6] and W. Stegmüller [8], where the concept of a physical theory is discussed.

5 A first step towards a logical analysis of this level has been made by P. Suppes in [11].

6 The reader not well acquainted with Suppes' ideas may consult his explanations and examples in [10], Chapter 12, and [12], Chapter 2.

[7] The idea of explicating approximation in empirical theories by means of uniform structures goes back to Prof. Ludwig. But Ludwig's approximated entities do not correspond to our potential models; they are variously defined tupels of terms. I consider the present use of uniform structures to be simpler and more homogeneous than Ludwig's.

[8] It is not assumed that the diagonal is itself a fuzzy set; this can not be proved in this axiom system. And I think this is all right. It is science fiction to assume that a theory will ever be supposed to apply with absolute exactness.

[9] The reader is reminded that the sets M_p and M_{pp} are related by a many-one *restriction function r* such that it applies a possible model to its corresponding partial model by 'cutting' the T-theoretical terms: $r[M_p] = M_{pp}$.

[10] Against Sneed, I consider the concept of *range of intended applications* as a historical time-relative notion. Most of Newton's intended applications are *not* the intended applications of present mechanics – of course, most of them are *nearly* the same. I have no use for a notion of *God's* intended applications of mechanics.

[11] In more recent writings, Sneed and his collaborators reconstruct the empirical claim of a theory by using the notion of a 'theory-net' instead of an 'expanded core'. But this modification has no particular bearing on the present issue.

[12] The addition of the 'minimum' postulate to (D 5) would not modify this assertion, as can be easily proved.

[13] Since the admissible class $\mathfrak{A}$ on the M_p-level induces an admissible class $\mathfrak{B}[\mathfrak{A}]$ on the M_{pp}-level by (Th. 8), the relation $x \sim x'$ on the M_p-level can always be translated into a corresponding relation $r(x) \sim r(x')$ on the M_{pp}-level.

BIBLIOGRAPHY

[1] Adams, E. W.: 'The Foundations of Rigid Body Mechanics and the Derivation of its Laws from Those of Particle Mechanics', in *The Axiomatic Method* (ed. by Henkin, Suppes, and Tarski), North-Holland, Amsterdam, 1959, pp. 250–265.

[2] Bourbaki, N.: *Topologie générale*, Hermann & Cie., Paris, 1951.

[3] Ludwig, G.: *Deutung des Begriffs 'physikalische Theorie' und Grundlegung der Hilbertraumstruktur der Quantenmechanik durch Hauptsätze des Messens*, Springer-Verlag, Berlin-Heidelberg-New York, 1970.

[4] Scheibe, E.: 'Die Erklärung der Keplerschen Gesetze durch Newtons Gravitationsgesetz', in *Einheit und Vielheit. Festschrift für Carl Friedrich v. Weizsäcker* (ed. by E. Scheibe, and G. Süßmann), Göttingen, 1973).

[5] Scheibe, E.: 'Vergleichbarkeit, Widerspruch und Erklärung', in *Philosophie und Physik* (ed. by R. Haller), Braunschweig/Germany, 1975.

[6] Sneed, J. D.: *The Logical Structure of Mathematical Physics*, D. Reidel, Dordrecht/Holland, 1971.

[7] Sneed, J. D.: 'Philosophical Problems in the Empirical Science of Science: A Formal Approach', this issue, pp. 115–146 (1976).

[8] Stegmüller, W.: *Theorie und Erfahrung. 2. Halbband (Theorienstrukturen und Theoriendynamik)*, Springer-Verlag, Berlin-Heidelberg-New York, 1973.

[9] Stegmüller, W.: 'A Combined Approach to the Dynamics of Theories. How to Improve Historical Interpretations of Theory Change by Applying Set Theoretical Structures', to appear.

[10] Suppes, P.: *Introduction to Logic*, Van Nostrand Reinhold, New York, 1957.

[11] Suppes, P.: 'Models of Data', in *Logic, Methodology and Philosophy of Science: Proceedings of the 1960 International Congress*, Stanford University Press, Stanford, Calif., 1962, pp. 252–261.
[12] Suppes, P.: *Set-theoretical Structures in Science* (mimeographed copy), Stanford University, Stanford, Calif., 1970.

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