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Veröffentlichungsversion / Published Version

Sammelwerksbeitrag / collection article

Empfohlene Zitierung / Suggested Citation:

Balzer, W. (1981). Sneed's theory concept and vagueness. In A. Hartkämper, & H.-J. Schmidt (Eds.), *Structure and approximation in physical theories* (pp. 147-163). New York: Plenum Press. <https://nbn-resolving.org/urn:nbn:de:0168-ssoar-39213>

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SNEED'S THEORY CONCEPT AND VAGUENESS

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INTRODUCTION

The discussion of vagueness and approximation in empirical and especially physical theories - as far as it is intended as a part of meta-science - faces the following problem. There are practically no utterances of practising scientists about the relation of theory and approximation which could serve as 'data' against which the meta-discussion might be 'tested'. This is not to say that approximation is not relevant for physics: the very first laboratory courses in physics prove the contrary. The calculation of errors in measurement and the 'theory of disturbances' are essential for physical methodology. But these achievements are not suited to clarify the relation between theories and reality.

Consequently the meta-discussion cannot proceed 'empirically', i.e. by formulating hypotheses for which 'real life' examples are presented. Rather the discussion is 'analytic' in the sense that it analyses the connection between theory and reality and the role of approximation on the basis of some general background. As long as we have no case studies of concrete examples of approximation the discussion will remain analytic, and what I shall have to say in the following will be analytic to a large extent, also.

There are at least three proposals for treating vagueness or approximation technically. A first, 'classical' text here is Przelecki's [9]. He offers an apparatus in terms of the semantics of first-or higher-order theories. A second proposal is due to Ludwig who introduces topological uniformities to 'smear over' the sharp theoretical images of the theory (see his contribution in this volume, or his [7]). Thirdly, Moulines in [8] has applied Ludwig's ideas to Sneed's theory concept.

My aim in this paper is to consider these propositions with respect to their underlying intuitions and to look for adequate formal realisations of these intuitions in Sneed's concept of an empirical theory.

It should be noted that the latter concept is 'open' enough to allow for an 'emendation' by building in aspects of approximation without essentially changing the original content. I think such 'openness' is favourable for meta-theoretical concepts and fits the idea propounded by Stegmüller in [14] of starting an analogue to the Bourbaki-programme in the realm of empirical sciences on the basis of some suitable theory concept. In order to build in approximation into Sneed's theory concept it seems necessary to start with a brief discussion of some central features of this concept.

I. SNEED'S THEORY CONCEPT

I will concentrate on the aspect of formulating empirical claims which is especially relevant for problems of approximation. The discussion of empirical claims will force me to look at the so called 'problem of theoretical terms' in some detail. For a broader discussion the reader is referred to [12], [13], [14] and for an appropriate technical formulation especially to [3].

The starting point of Sneed's theory concept is that there are axiomatizations of physical theories. Any such axiomatization yields a class of structures or (in the following) models: M . In special contexts it is profitable to regard M as the class of structures of a certain species in the sense of Bourbaki [4] (as proposed by Ludwig [7]) or as the class of structures of a certain 'similarity type' in the sense of Feferman [5].

But the characterization of a class of models has nothing to do with experience or reality. We have to ask how M can be used in order to formulate statements about reality or empirical claims. It is clear that this cannot be done without reference to 'real systems'. So let us introduce a set I such that the members of I are 'real systems'. In classical mechanics, for instance, such real systems are the solar system and its sub-systems, concrete pendulums, the tides, and concrete harmonic oscillators. Such real systems will be called intended applications, so I is the set of intended applications. As soon as we have these two components, M and I , we can try to formulate an empirical claim of the following form: "All intended applications of the theory are models of the theory". This claim admittedly is a bit unclear, in particular it is not clear what is meant by 'are'. The meaning of 'are' depends on the structure of the intended applications. There are various possibilities of giving a special structure to the intended applications of which I want to point out two extremes. A first possibility is to regard an intended application

as a (finite) set of atomic propositions¹⁾. (Proponents of this view are, e.g., Ludwig and Popper). A second possibility is to regard an intended application as a structure of the same 'type' as the members of M , i.e. intended applications are structures which possibly might be models. A discussion and comparison of these possibilities which, I think, arise from different strategies in reconstructing a theory or a 'hierarchy' of theories is beyond the bounds of this paper. As I want to concentrate on Sneed's theory concept I shall follow Sneed and choose the second possibility.

A theory T then consists of at least three components, M , I and M_p , where M_p is the class of potential models, i.e. of structures of the type of models (which need not satisfy the central axioms of the theory).

D1 a) T is a theory* iff $T = \langle M_p, M, I \rangle$ and

- 1) M_p and M are classes of structures of the same type
- 2) $M \subset M_p$
- 3) $I \subset M_p$

b) the empirical claim* of $T = \langle M_p, M, I \rangle$ is that $I \subset M$

A claim of this form immediately leads to the problem of theoretical terms if we ask how the claim can be tested. Since the claim has the form of a universal sentence

$$\forall x (x \in I \Rightarrow x \in M)$$

it is sufficient to consider one arbitrary $x_0 \in I$. How do we test

$$(1) \quad x_0 \in I \Rightarrow x_0 \in M ?$$

If we know that $x_0 \in I$ we know that x_0 has the form of a potential model. Let us assume the potential models of T having the form $x = \langle y_1, \dots, y_n \rangle$ where y_1, \dots, y_n are functions or relations. Then x_0 also has this form: $x_0 = \langle y_1^0, \dots, y_n^0 \rangle$. But we do not know what the various y_i^0 look like. The set I is not defined explicitly but 'paradigmatically', i.e. some concrete systems are singled out - forming a finite set I_0 of 'paradigm intended applications' - and other systems are said to belong to I if they are 'sufficient-

1) I speak of propositions rather than of sentences in order to preserve the distinction between models and axioms, or between interpretation and language. Propositions (in the technical sense) are entities of the first kind. They correspond to sentences at the level of language, and by an atomic proposition the reader may understand a proposition corresponding to an atomic sentence.

ly similar' to members of I_O . In order to test (1) we must describe y_1^O, \dots, y_n^O and this in turn leads to measurements in order to find out the 'values' of y_1^O, \dots, y_n^O .

Now we have the following problem. There are theories T with theoretical components. That is, there is some component, say y_n , in the potential models of T such that every measurement of y_n presupposes some version of T 's axioms to be satisfied in the course of the measurement (for details compare (2)).

As an example take mass in classical particle mechanics (CPM). The potential models of CPM have the form $\langle P, T, s, m, f \rangle$, where P is a non-empty, finite set (of 'particles'), $T \subset \mathbb{R}$ is an open interval (time), and s (position), f (force), m (mass) are such that $s : P \times T \rightarrow \mathbb{R}^3$ and $f : P \times T \times \mathbb{N} \rightarrow \mathbb{R}^3$ are smooth and $m : P \rightarrow \mathbb{R}_+$. Models of CPM are those potential models which satisfy Newton's second law:

$\forall p \in P \forall t \in T (m(p) \ddot{s}(p, t) = \sum_i f(p, t, i))$. Mass in this theory is a theoretical component. To prove this statement we have to consider all methods of measurement for mass. Since this totality is very difficult to describe we can only try to confirm it by means of some examples. Let us consider the measurement of mass by means of central, inelastic collisions. This method yields the mass ratio in terms of differences of velocities, provided the two particles move on a straight line. But these assumptions do not guarantee that we really measure mass. If, e.g., the whole measuring device is accelerated along the line of motion we will not accept the ratio of the velocity differences as an expression being equal to the mass ratio although the performance of the experiment still might be possible. A physicist will say that of course the measurement has to be performed in an inertial system. If by this he means what Prof. Ludwig has suggested, namely that objects made out of 'soft' material do not change when submitted actively to the usual Galilei transformations then he already presupposes 'part of' Newton's second law, namely that

$$(2) \quad \forall p \in P \forall t \in T \left(\sum_i f(p, t, i) = \varphi(m(p) \ddot{s}(p, t)) \right) \text{ with}$$

$$\varphi : \mathbb{R}^3 \rightarrow \mathbb{R}^3 \text{ and } \varphi(0) = 0.$$

For if the objects are not accelerated the presence of non-zero forces would deform them. Among those functions φ for which (2) is true identity certainly is the simplest one, and so we can say that some version of Newton 2 is satisfied.

The reader may check other methods for measuring mass and see whether he can find an example where no version of Newton's second law is necessary. But in all cases he must be able to convince us that he really measures the 'mass of CPM'. Part of this task will always be to exclude counterexamples of the kind just discussed, and usually such counterexamples lead to the assumption

of some version of Newton's second law. Anyway, Sneed and others have claimed that mass is CPM-theoretical. This is an 'empirical claim' at the meta-level, and, I think, a correct one.

Usually the most interesting terms of empirical theories turn out to be theoretical. Thus the following 'problem of theoretical terms' cannot be neglected: if T has a theoretical component then confirmation of an empirical claim of the form of D1-b) leads to a version of circularity. For in order to check, say, whether $x_0 \in I \Rightarrow x_0 \in M$ is true we have to determine the components of x_0 among which there is a theoretical one. In order to determine (to measure) this component it is presupposed that the 'measuring-system' already satisfies a version of the axioms. Thus in order to determine whether x_0 is a model we already presuppose that some x_1 satisfies a version of T 's axioms. So we only have a kind of conditional confirmation, namely relative to the assumption that x_1 is a 'model-version' of T . If we want to transform this conditional confirmation into an 'absolute' one we have to test whether x_1 in fact is such a version. But this leads to a repetition of the whole story. In order to test whether x_1 satisfies a version of T 's axioms we first have to measure x_1 's components, and since x_1 contains a theoretical component this is possible only by presupposing some x_2 being a version of T 's axioms.

I agree with the objection that this formulation of the problem of theoretical terms is still a bit vague: what is a 'version of T 's axioms' and what is a 'measuring-system'? The latter notion has been clarified in [2] where it is called 't-determining model', while the concept of a version still waits for precise explication. But such an explication does not pose any principal question and I avoid it only because it leads to rather complicated technical formulations.

An elegant solution of the problem of theoretical terms was suggested by Ramsey: just reformulate the empirical claim such that the theoretical components come under existential quantifiers. For given x_0 then it is no longer claimed that x_0 is a model but that there exist theoretical components which, if adduced to x_0 , yield a model. Certainly this solution (which is called the solution by Ramsey-sentences) is not the only possible one and it seems to be a rather coarse solution. But it fits with practising scientists' answers to questions like: "What does the theory tell us about this system?".

A more precise formulation forces us to extend our theory concept as follows. We have to introduce a distinction between theoretical components and non-theoretical components of the potential models. We can do this by making explicit the structure of potential models (a potential model x then being of the form $x = \langle y_1, \dots, y_n \rangle$) and by introducing a natural number m ($m \leq n$) together with the stipulation that y_1, \dots, y_m are non-theoretical components and y_{m+1}, \dots, y_n are theoretical components of x . The class of structures consisting of non-theoretical components only

is called M_{pp} and its members 'partial potential models'. In this frame it is sufficient to require I to be a subset of M_{pp} instead of M_p . This, finally, allows us to formulate an empirical claim in which no theoretical component occurs as constant (D2-b) below).

D2 a) T is a theory¹ iff $T = \langle M_p, M, M_{pp}, I \rangle$ and

1) M and M_p are classes of structures x of the same type and of the form $x = \langle y_1, \dots, y_n \rangle$

2) $M \subset M_p$

3) there is $m \leq n$ such that

$$M_{pp} = \{ \langle y_1, \dots, y_m \rangle / \exists y_{m+1} \dots \exists y_n \langle y_1, \dots, y_m, y_{m+1}, \dots, y_n \rangle \in M_p \}$$

4) $I \subset M_{pp}$

b) the empirical claim¹ of $T = \langle M_p, M, M_{pp}, I \rangle$ is that

$$\forall x (x = \langle y_1, \dots, y_m \rangle \in I \Rightarrow \exists y_{m+1} \dots \exists y_n \langle y_1, \dots, y_m, y_{m+1}, \dots, y_n \rangle \in M)$$

This claim can be written more elegantly if we use the function $r : M_p \rightarrow M_{pp}$ defined by $r(\langle y_1, \dots, y_m, y_{m+1}, \dots, y_n \rangle) = \langle y_1, \dots, y_m \rangle$. The claim then is that $\forall x (x \in I \rightarrow \exists x' (r(x') = x \wedge x' \in M))$ or, still more briefly

(3) $I \subset r(M)$.

This claim no longer creates a problem with theoretical terms for the role which these play in (3) can be tested by paper and pencil operations: we only have to check whether there exist theoretical augmentations x' such that $x' \in M$. The determination of x - which is treated as 'given' - is unproblematic, at least from the point of view of the theory under consideration, because x does not contain any theoretical component.

I do not treat here other features of Sneed's theory concept, as for instance the 'constraints' which are crucial to arrive at non-vacuous empirical claims, or the notion of a theory-net which allows us to treat dynamical aspects as well as larger 'portions of theory'.

II. VAGUENESS AS MULTIPLICITY

This view of vagueness as well as the slogan is due to Przelecki. In [9], [10] and [11] he treats vagueness in a semantical frame. There is given a language L of an empirical theory which has to be interpreted (in the sense of formal logics). In contrast to formal or mathematical theories the interpretations of

an empirical language cannot be arbitrary. Only certain 'intended' interpretations are of interest. Vagueness now means that there is not one unique intended interpretation for L but there are many 'admissible' intended interpretations.

The idea behind this approach is that an interpretation of language L is given or induced by ostensive or non-verbal means, in physics for instance by means of measurement. And it is a fact that two interpretations of some expression of L as given by measurements usually are different.

For example, if we consider the function m of CPM and interpret it, first, as the mass function of a given system x at time t - which is determined by means of mass-measurements - and, second, as the mass function of the same system x at time $t + 1$ again determined by means of measurement, then usually the two interpretations will be different. In other words, if we twice measure the mass of some particle p then, in general, we obtain two different values. This is an observation about how the world is and little or even no theory is needed to establish such observations.

Thus we have as a general feature of most terms of empirical theories that their interpretation at different occasions will be different. This gives rise to a kind of vagueness: the empirical claim will depend on the special way in which we arrive at an interpretation of the terms occurring in this claim.

To avoid misunderstandings it seems useful to add two remarks. First, this kind of vagueness has nothing to do with the question of how precise our measuring instruments are. If they are not very precise then of course we will expect different values at different times. But even if they are very precise, say, more precise than our ability to resolve visual impressions, the situation does not change. For in such cases by means of ingenious devices we can make 'visible' (e.g. countable) the differences which are too fine for the senses. And with such auxiliary devices again we obtain different results in different performances. Second, this kind of vagueness does depend on the kind of measurement which is used. If the term to be interpreted for instance is a function taking only the values 0 and 1 (think of yes-no experiments in quantum mechanics) then there are examples in which practically all performances of the experiment yield the same value and thus the same interpretation. But such experiments are important mainly in quantum physics and there we have statistical features elsewhere. In classical theories we could introduce such measurements but this would necessitate a substantial reformulation of the established theories. I do not want to speculate on what could be the case. It is the case that the terms of the established classical theories, if interpreted by means of measurement, yield multiple results. (The point here is that 'smeared' terms, like $m \pm \epsilon$, are not terms occurring in established theories.)

Now in order to build this kind of vagueness into the Sneedian theory concept we have to look for counterparts of a language L and of 'interpretations'. A language L is present only implicitly in a theory T in the sense of D2-a). The different components of the potential models correspond to the different basic symbols of a language. More precisely, we could regard these components as given by some interpretation of a language containing a non-logical constant for each such component. The intended interpretations mentioned above in T correspond to the set of intended applications I . For the other components, M , M_p and M_{pp} are classes of all structures of a certain kind. So they are, so to speak, classes of all possible interpretations of suitable languages (in case of M the class is further restricted by purely verbal or formal requirements). So I is the only candidate to express that we want to deal only with certain intended objects (interpretations or applications). But the correspondence of an intended interpretation with an intended application seems to be sound also on intuitive grounds. An intended interpretation in fact is some structure consisting of components like those of intended applications. The distinction between theoretical and non-theoretical components does not create any problems because Przelecki also has such a distinction and the intended interpretations at first are considered on the non-theoretical level.

If we identify intended interpretations with intended applications then a multiplicity of interpretations becomes a multiplicity of intended applications. This multiplicity must not be confused with a multiplicity of intended applications already present in D2-a), namely the multiplicity constituted by the fact that usually I contains more than one member. Different elements of I in D2-a) stand for different real systems, like two systems consisting of two different concrete pendula or another concrete system which is a harmonic oscillator. The multiplicity we are discussing here is a multiplicity of intended applications corresponding to one single real system. The idea is that corresponding to some given real system we always have a multiplicity of intended applications (intended interpretations) created by the fact that the determination of the components of the given system yields different results at different occasions.

These considerations give rise to the following formal treatment. We impose new structure on the set I of intended applications of D2-a). We distinguish a family of sets of intended applications $(I_j)_{j \in J}$ such that for all j : $I_j \subset M_{pp}$. The indices in J are thought of as representing concrete systems, i.e. they are names of concrete systems. For instance, the pendulum swinging at a certain time in a certain laboratory will be denoted by an index in J . The set I_j corresponding to this index then contains the various interpretations which we obtain for this pendulum if we determine its position function and the mass of the particle at different occasions. I is given as the union of all these I_j .

It may be noted that a set I_j does not contain a distinguished element, the 'true' description of the system. This is no short-coming but an advantage. There simply are no distinguished descriptions of concrete systems. This point can be better understood by contrasting it with the opposite one. According to the opposite point of view for a concrete system, like the pendulum, given ostensibly, there exists one distinguished, 'sharp' and 'true' theoretical image or description, and the set I_j is obtained by smearing this distinguished structure which is necessary because of our limited ability to determine this true object. I think this latter view is wrong and misleading. It is wrong as concerns the everyday experience in measuring, and it is misleading in meta-theoretic thinking because it strengthens our tendency to overestimate theoretical pictures and to underestimate their accessibility.

I now go beyond what Przelecki has proposed by asking whether we can (or should) introduce uniform structures at this stage. Of course we can. The sets I_j are natural candidates on which we can impose uniform structures \mathcal{U}_j . The members U of such an \mathcal{U}_j are subsets of $I_j \times I_j$. $\langle x, x' \rangle \in U$ with $x, x' \in I_j$ and $U \in \mathcal{U}_j$ means that x and x' are similar of 'degree U ', and U will be called a degree of similarity in the following. There are two arguments in favour of such \mathcal{U}_j . The first argument is that in case of quantitative theories, which is the normal case in physics, we will have one or several natural mathematical uniformities on each I_j . A second argument is that in physical practice we find activities corresponding to such uniform structures. Calculations of errors, for instance, consist of calculations of mean-values and of variances, and this can be interpreted as calculating a degree of similarity U to which both systems - the one given by the mean-values and the other one given by the measured values - belong. We will therefore, in addition, introduce uniformities with each set I_j (see D4) below.

If we introduce uniformities we somehow must restrict the possibilities for dissimilarity in order to avoid smearing the empirical claim to such an extent that it becomes logically true. The usual definition of uniform structures contains an axiom of the form $U \subseteq V \wedge U \in \mathcal{U} \Rightarrow V \in \mathcal{U}$ (compare D3-b) below for an understanding of this formula). Thus the uniformity has to contain with each U all V representing smaller degrees of similarity ($U \subseteq V$). This axiom implies that the cartesian product of the basic set always belongs to the uniformity, i.e. that the uniformity \mathcal{U} contains a degree of similarity according to which every object of the basic set is similar to every other such object. Clearly we need some restriction here. There are two possibilities. First, we can drop the axiom just mentioned and work with 'weak' uniform structures as defined in D3-b) below. In these structures the above axiom need not be satisfied and we can think of the uniformity \mathcal{U} itself as representing the 'admissible' degrees of

similarity and no others. In this case we can regard the members of I_j as being all admissible in the sense that they are sufficiently similar to each other. A second possibility (used by Moulines on the theoretical level) is to treat I_j as full uniform space and to choose a subset \mathcal{A}_j of each \mathcal{U}_j with the idea that \mathcal{A}_j contains the admissible degrees of similarity. For reasons of simplicity I will take the first alternative.

We now can introduce the concept of a weak uniform space.

D3 a) if N is a set and $U \subset N \times N$ then

$$a.1) \Delta_U = \{ \langle y, y \rangle / \exists z \in N (\langle y, z \rangle \in U) \vee \exists z \in N (\langle z, y \rangle \in U) \}$$

$$a.2) U^{-1} = \{ \langle y, z \rangle / \langle z, y \rangle \in U \}$$

$$a.3) U^2 = \{ \langle x, y \rangle / \exists z (\langle x, z \rangle \in U \vee \langle z, y \rangle \in U) \}$$

b) x is a weak uniform space iff $x = \langle N, \mathcal{U} \rangle$ and

- 1) N is a non-empty set
- 2) $\emptyset \neq \mathcal{U} \subset \text{Pot}(N \times N)$
- 3) $\forall U, U' (U \in \mathcal{U} \wedge U' \in \mathcal{U} \Rightarrow U \cap U' \in \mathcal{U})$
- 4) $\forall U (U \in \mathcal{U} \Rightarrow \Delta_U \subset U \wedge U^{-1} \in \mathcal{U})$
- 5) $\forall U' \exists U (U' \in \mathcal{U} \Rightarrow U^2 \subset U' \wedge U \in \mathcal{U})$

For an interpretation of these axioms see, e.g. Ludwig's contribution to this volume.

D4 T is a theory iff $T = \langle M_p, M, M_{pp}, I, (I_j)_{j \in J}, (\mathcal{U}_j)_{j \in J} \rangle$ and

- 1) $\langle M_p, M, M_{pp}, I \rangle$ is a theory¹ (see D2-a)
- 2) J is a non-empty set
- 3) for all $j \in J : I_j \subset I$ and $\mathcal{U}_j \subset \text{Pot}(I_j \times I_j)$
- 4) $I = \bigcup_{j \in J} I_j$
- 5) for all $j \in J : \langle I_j, \mathcal{U}_j \rangle$ is a weak uniform space

As already said, J is a set of names of concrete physical systems and each I_j is a set of intended interpretations arrived at by measuring system j . Each \mathcal{U}_j is a weak uniformity on I_j .²⁾

We now can formulate a vague empirical claim of T . To this end we agree on the following notation. If $X \in \prod_{j \in J} I_j$

$(\mathcal{X} \in \prod_{j \in J} \text{Pot}(I_j))$ then $\rho(X) = \{X(j) / j \in J\}$ ($\bar{\rho}(\mathcal{X}) = U\{X(j) / j \in J\}$). Intuiti-

2) We might add $I_i \cap I_j = \emptyset$ for $i \neq j$ but this seems a rather strong requirement.

vely, $\rho(X)$ is the set of 'components' $X(j)$ of X .

D5 the vague empirical claim² of $T = \langle M_p, M_{pp}, I, (I_j)_{j \in J}, (\mathcal{U}_j)_{j \in J} \rangle$ is that

$$\exists X (X \in \prod_{j \in J} I_j \wedge \rho(X) \subset r(M))$$

The vague empirical claim² formulated with the help of this apparatus intuitively says that there exists a combination of intended applications X such that for each name j , X contains exactly one member, and all these members by means of theoretical components can be extended to models. In other words, for each concrete system there exists an intended description (given by measurement) and these descriptions can be extended to models.

Some further remarks may be helpful. First, we observe that the uniformities \mathcal{U}_j in a natural way induce a uniformity \mathcal{U} on $\prod_{j \in J} I_j$. The present account then formally can be regarded as

smearing an empirical claim¹: $I^* \subset r(M)$ by smearing I^* with the help of \mathcal{U} . Here, I^* is the set of 'sharp' intended applications in the sense of D2). In the terminology of D5) we would say that for some 'ordering' of I^* , say I_0 ($\rho(I_0) = I^*$), $I_0 \in \prod_{j \in J} I_j$. But as already

said this point of view is misleading for it suggests the existence of some distinguished member of $\prod_{j \in J} I_j$, namely some such I_0 .³⁾

Second, we note that the claim² of D5) contains two 'Ramseyfications', two existential quantifications. For the part $\rho(X) \subset r(M)$ implicitly contains another existential quantifier. So the explicit formulation is this:

$$\exists X \exists Y (X \in \prod_{j \in J} I_j \wedge r(Y) = \rho(X) \wedge Y \subset M).$$

As a third remark let us pose the question why we have chosen an existential quantification for X . Alternatively we might have put X under an universal quantifier, thus requiring that all combinations of $\prod_{j \in J} I_j$ can be extended to models. But such a claim would

be false in most cases. To see this consider for instance CPM enriched by the law of gravitation. For suitably given paths (expressed by s) we can find masses such that the gravitational equations are satisfied. But in an arbitrarily small neighbourhood of s we find mathematical paths s' such that there are no masses for which s' satisfies the gravitational equations. We cannot exclude - and in fact it is very likely - that both these paths s and s' occur in one I_j , i.e. they both arise from a concrete

3) This argument was put forward by Przelecki in a discussion.

system by means of measuring the corresponding paths. In this case a universally quantified claim is false. This seems reason enough to justify the existential quantifier.

Fourth, it is easy to see - and has been pointed out by H.-J. Schmidt - that the claim² of D5) does not at all refer to the uniformities $(\mathcal{U}_j)_{j \in J}$ occurring in T. In fact, as long as we choose exactly one member from each I_j there is no possibility of using some $U_j \in \mathcal{U}_j$ in formulating the claim². The component $(\mathcal{U}_j)_{j \in J}$ assigned to the theory (c.f. D4) thus seems redundant. But I think that the intuition of adding these uniformities is sound: there is something like degrees of similarity between different 'sets of data' obtained by measuring the same system several times, and these similarities are relevant and highly important for connecting theory with experiments. The inadequacy of D5) - as revealed by its neglecting the \mathcal{U}_j - is this. We have oversimplified the claim by choosing exactly one member from each I_j . If the indices in J are to denote concrete physical systems then of course there is the possibility of measuring one such system j at several times and thus obtaining several members of I_j to be regarded in the empirical claim. And only in this case do we have the opportunity of investigating those different members of I_j with regard to their respective similarities. Such similarities, once established quantitatively, form a kind of standard with which new measurements or measurements of similar systems are compared. Such procedures are the basic - and, I think, even the only - means to obtain and explicate something like confirmation of a theory. If we allow for more than one member of each I_j to be considered in the empirical claim we arrive at the following.

D6 If $T = \langle M_p, M_{pp}, I, (I_j)_{j \in J}, (\mathcal{U}_j)_{j \in J} \rangle$ is a theory in the sense of D4) then

a) the empirical claim of T with precision $(U_j)_{j \in J}$ ($U_j \in \mathcal{U}_j$ for all $j \in J$) is that

$$\exists \mathcal{X} (\mathcal{X} \in \prod_{j \in J} \text{Pot}(I_j) \wedge \bar{\rho}(\mathcal{X}) \subset r(M) \wedge \forall j \in J (\mathcal{X}(j) \neq \emptyset \wedge (\mathcal{X}(j) \times \mathcal{X}(j)) \subset U_j))$$

b) the vague empirical claim of T is that for each $j \in J$ there is exactly one $U_j \in \mathcal{U}_j$ such that

$$\exists \mathcal{X} (\mathcal{X} \in \prod_{j \in J} \text{Pot}(I_j) \wedge \bar{\rho}(\mathcal{X}) \subset r(M) \wedge \forall j \in J (\mathcal{X}(j) \neq \emptyset \wedge (\mathcal{X}(j) \times \mathcal{X}(j)) \subset U_j))$$

According to D6-a) for every concrete system $j \in J$ there are some members of I_j (at least one, since $\mathcal{X}(j) \neq \emptyset$) which can be extended to models and which are similar to each other with degree U_j . D6-b) makes sense only in view of our use of weak uniform spaces. For with full uniform spaces \mathcal{U}_j a claim of this form would always be true. Existential quantification over the U_j becomes non-

trivial only if the \mathcal{U}_j are obtained by 'cutting off' full uniformities on $I_j \times I_j$ at some place 'in the direction of dissimilarity'. We then can imagine one or several 'greatest' degrees of similarity in \mathcal{U}_j which already are 'fine' enough to express real, non-trivial similarities.

To sum up this section we can say that the kind of vagueness of empirical claims discussed here is due to a statistical character of the world. Every measurement reveals a slightly different object which is measured. So it is misleading to say one has measured one object and obtained different results because of 'disturbances' or what ever. It is more adequate to say that by repeated (eventually sharp) measurement one has acquired information about a multiplicity of slightly different objects. This kind of vagueness has to be considered as a component of empirical theories because it affects the empirical claim in a straightforward way. It corresponds to Ludwig's 'unscharfen Abbildungsprinzipien' and to Moulines' uniformities on the non-theoretical level. My proposal to take the content of 'vagueness a multiplicity' seriously is to consider a new kind of empirical claim which is just an extended Ramsey-sentence.

III. VAGUENESS AND IDEALIZATION

A second kind of vagueness comes up by the observation that our theories and theoretical pictures are 'idealizations', whereas the world is not idealized, i.e. 'real'. In an empirical claim these two things have to be put together. But they are of different nature and so how can we manage to bring them together? Since both parts do not precisely fit with each other, some room for vagueness seems necessary.

In Sneed's theory concept this problem does not exist at first glance. For intended applications, which represent 'the world' in the empirical claim already have an 'ideal' mathematical structure, namely that of partial potential models. In case of CPM, for instance, partial potential models are entities $\langle P, T, s \rangle$ which contain a continuous position function s . So the entities to be combined in the empirical claim, namely I and M , are of the same idealized nature. But this answer clearly is not satisfactory. The problem is still present in the connection between I and the world.

The contrast between idealized pictures and real world is clearly present in the relation between intended applications and concrete physical systems. The kind of vagueness we are thinking of is therefore implicit in the problem of how intended applications are determined (by means of measurement). For theories whose partial potential models contain quantitative components this amounts to the question: Can the set of possible outcomes of our measuring instruments include the set of all real members? Clearly the answer is: No. The number of distinguishable outcomes

is usually rather small. This certainly does not put us in a position to read off the function values of a continuous quantity for all its arguments from the real system. The question then is how to bridge the gap between our crude 'observational data' read off from measuring instruments and the idealized continuous quantities occurring in the theory.

Before continuing here it seems helpful to see how this question is related to the kind of vagueness discussed in Sec. II. There we have presupposed that 'by means of measurement' we somehow arrive at sharp values. This assumption seems to be false in the light of the present considerations. But this does not detract from the results of Sec. II, for what has been said there does not depend on whether our theories are idealized. The basic feature of 'vagueness as multiplicity' is present also in cases of non-ideal theoretical pictures. So it seems that 'vagueness as multiplicity' is independent from 'vagueness induced by idealization' (if something like that exists). Whether the opposite also is true depends on what we can make out of the latter kind of vagueness. In any case the discussion of this section does not affect the results of Sec. II. Even if we find it difficult to understand how we can arrive at 'sharp' results from 'coarse' data, this is no reason to attack 'vagueness as multiplicity'. For as long as we have our idealized theories, and no better ones, we shall want to work with these and somehow to bridge the gap, even if we do not quite understand how. And as soon as we have such a bridge 'vagueness as multiplicity' becomes relevant.

To come back to the question whether we need some formal representation of the vagueness arising because of the gap between real world and idealized theories, let us consider the proposals to bridge this gap. A first proposal which treats intended applications in the form of atomic observational propositions is to substitute propositions of the form ' $f(x) = \alpha$ ' by ' $\alpha - \epsilon < f(x) < \alpha + \epsilon$ ' where f is a physical quantity having real numbers as values. This account seems to be very natural and in fact has been proposed by different authors, e.g. Ludwig [7] and Wojcicki [16]. It is natural because it reflects physical practice: whenever in reading off some value from an instrument we stop at a certain decimal we have done something like introducing such an interval in which the 'real' value has to lie.

Even if this proposal is satisfactory to some extent as a description of practice it does not yield a better, in the sense of 'more precise' understanding of how we bridge the gap between reality and theory. On the one hand observational results of the form ' $\alpha - \epsilon < f(x) < \alpha + \epsilon$ ' still are 'sharp' in the sense that they contain special real numbers, namely $\alpha - \epsilon$ and $\alpha + \epsilon$. These numbers are not given by experience, they rather are chosen according to some pragmatic standard we usually are not able to make explicit. But why not use similar standards to fix a unique value α thus obtaining propositions of the original form

' $f(x) = \alpha$ '? The only difference between these two versions is that the 'interval'-version contains an explicit hint that the observational proposition contains some conventional component, namely in the choice of ϵ ; while the original version tends to make us forget this fact. From a formal point of view I see no reason to say that the 'interval'-version is 'nearer to reality' than its alternative.

On the other hand the question arises how to formulate an empirical claim with intended applications using such 'interval-statements'. It has been proposed to consider a physical theory as 'useful' if the observational sentences added to the axioms of the theory do not lead to contradictions [7]. In terms of set theoretic structures this amounts to a rather weak Ramsey-claim, namely that it is possible to 'fill in', first, the gaps between the finitely many observational propositions in order to obtain continuous functions, and, second, to add theoretical components in order to obtain full models. If we regard intended applications as full partial structures then an analogue to such a claim could be formulated as follows. We first smear the set of originally given sharp intended applications by introducing some uniformity at the non-theoretical level. But of course we cannot choose I as the basic set of the uniform space for we want to have included systems similar to, but different from intended applications. Therefore we have to choose a set of structures containing I as the basic set of the uniform space. A natural candidate for this is the class of all partial potential models, M_{pp} . By introducing a uniformity on M_{pp} we can formulate a claim of the following form. For each intended applicaiton $x \in I$ there exists a partial potential model x' in some neighbourhood U of x such that x' can be extended by theoretical components to a model. More formally:

$$\exists X (\forall x \in I \exists x' \in X (\langle x, x' \rangle \in U) \wedge X \subset r(M))$$

where U is a fixed degree of similarity of the uniformity imposed on M_{pp} . This claim can be reformulated as follows. We denote by U_x the neighbourhood of x induced by U , i.e. $U_x = \{x' / \langle x, x' \rangle \in U\}$. Then the above formula is equivalent to

$$\exists X (X \subseteq \prod_{x \in I} U_x \wedge \rho(X) \subset r(M))$$

There is an analogy between such a claim and propositions of the form ' $\alpha - \epsilon < f(x) < \alpha + \epsilon$ ': I corresponds to the sharp α and U to ϵ .

But if we compare this approach with that of D5) we see that there is not much difference, at least formally. In fact, it just amounts to the possibility we already mentioned in the discussion of D5). The only difference is that in D5) the basic set of the uniform space is given by I where I is different from M_{pp} , while here we have no clear idea how to choose this basic set (hence the

idea to take all of M_{pp}). But the disadvantage of this technical formulation has already been pointed out: it suggests that there is something like sharp, real intended applications. We see no argument for such a thing to exist and therefore no argument to prefer such a formulation to the more neutral one of D5).

So the proposal to go over to 'smeared' intended applications in order to bridge the gap between reality and theory, as far as it formally enters into the theory concept by imposing a uniformity on the intended applications, adds nothing to D5) and thus is redundant. The mechanism of choosing a special ϵ or a special U of some uniformity at present is not theoretically understood and our understanding is not much improved by pointing out that we choose some such ϵ or U . I do not feel convinced that this formal treatment gives insight into the mechanism.

A second proposal is due to Suppes [15]. But his work contains more of a program than a clear picture of how things are. Although I think that a theoretical understanding in fact can be achieved in the direction Suppes indicates, I have nothing to add at the moment.

We will have to live at the moment with the fact that we do not really know how 'observations' can be brought together with theories. Of course it is possible to develop theoretical pictures of such a mechanism but, I think it would be more efficient to study in detail some concrete examples first. Let me note that here we have another example of the 'openness' of Sneed's theory concept. In order to understand or to develop a theoretical picture of how 'data' come into contact with theories, we do not need to change the theory concept at all. For such a theoretical picture can be developed by means of putting together theories of the form of D2) (or D4)) in special ways. A special relation between theories (in the sense of D2)) which seems to be relevant here is 'theoretization' as introduced in [1] and [3]. But perhaps we need further relations not yet investigated.

Let me close with a more philosophical aspect of this problem. One clear feature of idealization is that some idealized theories contain an 'induction along space' in both directions: towards infinitely big and small areas. This is true for all theories containing Euclidean geometry or the real numbers ('containing' here in the logical sense). The axioms of such theories have been confirmed with objects of a certain size. But the way these theories are axiomatized induces their models to be infinite and to be infinitely fine (continuous). The use of such models implicitly assumes that things in certain respects behave independently of their size. And to a certain extent this assumption is justified by a form of induction. We assume that in realms in which we do not yet have experience, the world is like in that area we know. But of course such an assumption can be, and in fact has been, questioned. Today we do not believe in that induction. So one way to bridge the gap between data and theories, at least with respect to this special

type of idealization, would be to adjust our theories. One could try to eliminate such idealizing assumptions as far as possible and thus obtain theories whose basic terms can already be interpreted as 'real' objects in some specified domain. H.-J. Schmidt's contribution to this volume is one step in that direction. But such modification of physical theories is physics and does not belong to philosophy of science.

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