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TOWARDS THE FORMAL STUDY OF MODELS IN THE NON-FORMAL SCIENCES

SUMMARY

I. THE FUNCTION OF MODELS IN THE EMPIRICAL SCIENCES

Scientific research utilises models in many places, as instruments in the service of many different needs. The first requirement a study of model-building in science should satisfy is not to neglect this undeniable diversity (as has sometimes been done: 1), and, when recognising this multiplicity, to realise that the same instrument cannot perform all those functions (often the multiplicity of function is recognised but either not to a full extent, or not with respect to the difference of structure it implies: 2) We are going to mention some of the main motives underlying the use of models:

(A) For a certain domain of facts, let no theory be known. If we replace our study of this domain by the study of another set of facts for which a theory is well-known, and that has certain important characteristics in common with the field under investigation, then we use a model to develop our knowledge from a zero (or near zero) starting-point. This is what happens in neurology: we replace the central nervous system by a digital or anologue computer showing certain of the neurological peculiarities, and study this new object.

(B) For a domain D of facts, we do have a full-fledged theory, but one too dufficult mathematically to yield solutions, given our present techniques. We then interpret the fundamental notions of the theory in a model, in such a way that simplifying assumptions can express this assignment: under these simplifying assumptions, the equations becoming soluble. Using the theory of harmonic oscillators in the study of heat conduction is an example of such a procedure.

(C) If two theories are without contact with each other we can try to use the one as a model for the other or to introduce a common model interpreting both and thus relating both languages to each other.

(D) If a theory is well confirmed but incomplete, we can assign a model in the hope of achieving completeness through the study of this model. Special cases of this procedure are: a qualitative theory is known for a field and the model introduces quantitative precision; or a quantitative theory is used for a field, but not securely established, and the model circumscribes the solid core of the theory in qualitative terms.

(E) Conversely if new information is obtained about a domain, to assure ourselves that, the new and more general theory still concerns our earlier domain, we construct the earlier domain as a model of the later theory and show that all models of this theory are related to the initial domain, constructed as model, in a specific way.

(F) Even if we have a theory about a set of facts, this does not mean that we have explained those facts. Models can yield such explanations (Particle or wave theories of light, or statistical mechanics, are important examples of explanation through model building).

(G) Let a theory be needed about an object that is too big or too small or too far away or too dangerous to be observed or experimented upon. Systems are then constructed that can be used as practical models, experiments on which can be taken as sufficiently representative of the first system to yield the desired information.

(H) Often we need to have a theory present to our mind as a whole for practical or theorethical purposes. A model realises this globalisation through either visualisation or realisation of a closed formal structure.

(I) It often occurs that the theoretical level is far away from the observational level; concepts cannot be immediately interpreted in terms of observations. Models are then introduced to constitute the bridge between the theoretical and observational levels, the theoretical predicates being interpretable as predicates of the model and the observational predicates being also interpretable as predicates of the model, the model furnishing lawful relationships between the two interpretations. This intermediary model can be used to construct the abstract theory or, once it exists, to find for it domains of application.

We are certain that still other functions could be found for models in empirical research. We are also certain that with the help of some supplementary assumptions, some of these functions of models could be reduced to some others. Still, it is true to say that the aims mentioned: theory formation, simplification, reduction, extension, adequation, explanation, concretisation, globalisation, action or experimentation, constitute a kind of system. It appears indeed that models have been introduced in function of relations between theories and theories, between experiments and theories, between experiments and experiments, between intellectual structures and the subjects using these structures, and in all these cases this has occurred in order dynamically to produce new results, or in order to tie up new ones with old ones as guarantees, or simply to establish relation.

Most of these cases have occurred in the list above; while it is certain that the problem-solving behaviour of man knows other factors than those mentioned here, it seems to be true that the model as a tool mediating between some of these factors has here been adequately localised.

What are now the questions we wish to ask about the model-concept in these various roles? Among others, the following:

(i) Is it possible to derive from the description of the function to be fulfilled, the features a model should have to achieve this purpose?

(ii) Will the type of model needed to fulfil a given function have differing structure for theories, facts or actions of different types?

(iii) Can conditions be formulated determining when models can fulfil one of these functions, and when they cannot do so? When they are the only instruments or possible instruments among others?

(iv) Can some common feature be distinguished, either among the various aims, or among the various eventual structures, thus unifying to some extent the family of models?

The importance of these problems is clear. The concept of model will be useless if we cannot deduce from its function a determinate structure. The scientist in his comments uses the 'model' concept in all the ways we described and thus discards the simple and clear language of model-theory in formal semantics or syntax. Can we give to his use of the term an adequate rational reconstruction, or are we prevented from doing so? We are convinced that it is possible to derive structural features from the functional characterisation, and it is to this attempt that we now immediately proceed.

II. STRUCTURE AND PURPOSE

Let then R(S,P,M,T) indicate the main variables of the modelling relationship. The subject S takes, in view of the purpose P, the entity M as a model for the prototype T. We saw above a classification of possible purpose, S of values for P. Let us now only mention classifications for M and T.

Model and prototype can belong to the same class of entities or to different classes of entities. The following possibilities immediately offer themselves:

M or T are both images, or both perceptions, or both drawings, or both formalisms (calculi), or both languages, or both physical systems.

All these possibilities have occurred. But we can also have the heterogeneous case: M can be an image, T a physical system, or inversely; M can be an image and T a perception; M can be a drawing and T a perception; M can be a calculus and T a theory or language; or inversely. M can be a language and T a physical or biological system.

Among each of these classes, a finer subdivision could and should be considered. If the model is a theory, this theory can have all degrees of systematic unity, or of completeness, or of confirmation or confirmability; if the model is an image it can have all types of organisation, of vagueness, of closedness.

Will there be an interaction between the multiplicity of values for P and the multiplicity of values for M and T? There can be no doubt about this. Can the model-prototype relationship that exists in formal semantics teach us anything about similar relations occurring in domains so widely different? It is, once more, our conviction that it can do so. More formally: if L is the relation between M and T, then we claim that from R (S, P, M, T) we can derive facts about L(M, T), depending upon the different values of P, M and T.

To substantiate this claim, we will now analyse some special cases.

A. *Models and the progress of research.*

Each single purpose, among those we have mentioned, is in itself ambiguous. It should not astonish us then, that a model could aid the progress of a scientfic system in many ways: the science in question could need only completion, or, alternatively, restructuration. If the science in question is to be completed, then the model used to lead it to its completion should have properties not mentioned in the initially existent science. The study of these properties could then yield completion. For this to occur, a multiplicity of non-isomorphic models should exist and this is a perfectly normal case, in the formal sciences. This type of progress through model construction has the following two limitations: (a) it cannot furnish transformation, but only addition of new details; (b) it is intrinsically limited (when the final description of the model is completed, the process must stop). Those who wish to use model construction to rebuild their discipline or those who wish to guarantee indefinite evolution should use another model concept. But let us for a moment restrict ourselves to the more modest task one could hope to achieve, when looking only for completion and not for restructuration. Why should we construct models to reach this aim? Why can we not simply consider the possible hypotheses we could add to our theory, consistent with the already accepted ones? Why should we use this devious procedure while a more direct one is at our disposal? If we compare, for a given language L, the set of possible complete languages L1.... Ln obtained through addition of supplementary hypotheses to the set of models of L, then this set of models should have in some sense a structure that makes selection between the models easier than selection between the formalisms.

In principle, using the classical concept of model, the set of complete extensions of L and the set of models of L should be isomorphic. But if this is the case, then, in principle, models could be dispensed with. What then should be the concept of model that could make the use of models indispensable for the aim of simple achievement of a theory already started? Either there should be fewer models than possible additions of hypotheses, or the relations between the models should be simpler than the relations between the consistent extensions. In the first case there should be stricter demands on the model concepts than in the formal sciences, in the second case, the distinctions between models should be clearer, and so again we should have requirements preventing some intermediary cases allowed by the classical model definitions, from appearing among the list of models.

This being known, let us now ask how model-building could advance restructuration of a theory. If the so-called model is not really a complete model in the classical sense but only satisfies certain best-confirmed or most-used laws of the theory, then the model, not satisfying certain other less central features of the theory, could help us in replacing these by others that would be satisfied by the model. This type of partial correspondence and partial discrepancy between model and theory could eventually lead to indefinitely continuing development.

But this is not the only way in which models could help towards restructuration.

Let us suppose that we use a series of partial models, each of them representing part of the theory to be modelled, but none of them satisfying it as a whole, and some of them inconsistent with each other. Research into the extensions of these partial models that would include a maximum number of other partial models could equally lead to reformulation of the initial theory.

Or let us use a multiplicity of complete models simultaneously; or a combination of complete models and partial models. In such a scheme arbitrariness of the exact selection of the entity representing a concept will lead to search for new requirements that will yield a non-arbitrary selection.

A limit case of this situation is the use of a locally inconsistent model. In classical Rutherford atomic theory, it was clearly recognised that the nucleus of an atom should explode under the electromagnetic laws of the time (due to the internal repulsion of the positive charge). So everybody knew the model to be inconsistent. But this inconsistency was accepted because the nucleus could, in the applications where its charge was needed, be treated as a point, and where its dimensions were needed, its charge and the internal properties of it did not intervene.

A final possibility to help restructuration is the use of undefined or vague models, the unachievement and indefiniteness of which suggests and allows completion in given directions.

We do not believe that this exhausts all possibilities, but it seems to us that the possibilities mentioned here form a systematic whole: models are used for system restructuration, because of their relations with the system (partial discrepancy); because of their relationship among each other (partial inconsistency, at least multiplicity); because of their relationships with themselves (locally inconsistent or locally vague). To summarise: models used for completion should satisfy more stringent requirements than the classical model in the formal sciences, while models used for system restructuration should simultaneously satisfy more stringent requirements (our inquiry as to the conditions that make the detour through model-building desirable remains valid in this last case), and more lenient ones (partly inadequate, vague, multiple and locally inconsistent models).

Intuitively it thus appears that at least for one of the possible aims of model building, the bridge between the formal and the functional exists. Can we now build a formal theory about approximate, partial, multiple, locally inconsistent, or vague models? To inquire about this problem will be the task of our third section.

B. *Models and the Initiation of Research*

Let us have a set of data about a domain, either very unorderly and complex, or very incomplete. We wish to build a theory. We could try to tackle the data immediately themselves. But if we have some reason to suppose that they are grievously incomplete, or that they are very complex functions of the really independent variables, the following strategy seems fruitful: select one very specific law of the domain, try to build a mechanism, a model that satisfies this very specific law, and then, in view of this model, localise the form or structure of our data; the way in which they are complex and what supplementary data should be sought after if our initial ones are given functions of our model concepts.

In order for this method to be fruitful, the basic law we represent in the model should be such as to have very few models satisfying it in a given range, or, if many satisfy it, should be such that in all those cases, the model of the data derivable from the model of the law allows analysis of these same data into entities of simpler and more regular structure.

Compared to the classical model situation, we have here a structure that should satisfy a requirement of correspondence with a theory T, of very elementary character, but should moreover satisfy a requirement of correspondence with one partial model of the elementary theory: namely the data, and this second requirement is such that the mapping of D into M should give an image M (D) having a more regular structure than D. The triangular relation between T, M and D, L (D, T) being itself approximately true is thus here an essential feature of the situation. We saw in (A) that for the needs of research composite models should be considered; now we see, in a symmetrical fashion, that other needs suggest the study of models of complexes of theories, ordered in certain ways.

C. *Models and experience*

Already in his 'Introduction to Semantics' [3], Rudolf Carnap makes the distinction between a logical and a descriptive interpretation of a calculus. Without claiming that an empirical science is or can be a calculus in Carnap's sense, we should consider the considerations introduced in making this distinction. If we give for all signs of a calculus rules of designation, or, if we give for all sentences of a calculus rules of truth, we give an interpretation of this calculus. On p. 203-204 Carnap stresses that for application it is necessary to construct a bridge between 'the postulate set and the realm of objects' (p. 204) and that this is called 'constructing models or giving interpretations' (phrases he uses synonymously). An interpretation is a true interpretation if whenever a sentence implies another in the calculus, in the interpretation whenever the first sentence is true, the second is equally true, and whenever a sentence is refutable in the calculus, it is false in the model. Such a true interpretation is a logically true interpretation, if the sentences that become true, become logically true. An interpretation is a factual interpretation if it is not a logical interpretation. An interpretation is a descriptive interpretation if at least one of the undefined signs of the calculus becomes in the interpretation a descriptive sign, and while Carnap gives, p. 58-60, clear examples of descriptive signs: names of single things, of observable properties, he stresses on p. 59-60 that no general solution in general semantics is known for the problem of distinguishing between logical and descriptive signs. These definitions are important for us, because it is clear that the concept of model in the empirical sciences, when it is used in the following context 'the world is a model of our sciences, in as far as these sciences are true' (or conversely the aim of science is to construct a calculus for which reality is the only model) takes the concept 'model' in the sense of a factual and descriptive true interpretation. Here we realise that we are up against some of the main problems of recent logical research: the definition of logical truth and the definition of descriptive sign. But there are here still more problems that could not easily be treated by Carnap in 1946 but that have become especially prominent: it has been recognised that most calculi have many more models than they were intended to have (the existence of non-standard models is a case in point). When now we talk about 'models' in empirical sciences, we mean, if we want reality to be a model of our science, to talk about an intended model. The only writer who has tried until now to introduce a general distinction between logical and descriptive constants, and to formalise some of the properties that distinguish intended from non-intended models, is Kemeny.[4] In the sense we are discussing here, a model in the empirical sciences is an intended factually-true descriptive interpretation. (Or, in some other contexts: a non intended arbitrary interpretation, used to clarify such intended factually-true descriptive interpretation.) If we now introduce, with Kemeny, as definition for logical truth, validity in all interpretations, and the property of being a descriptive constant as not being assigned the same value in all interpretations, and if moreover we accept (again from Kemeny) that all models are interpretations that have the same domain of individuals as the intendedone but other assignments for non-logical constants, then, if we are to study models for empirical sciences, we must study sets of structures having the same individuals, and varying for all undefined constants, their assignments in the models, (except for the classical logical constants) and not differring from at least one among them otherwise than through this variation.

It is clear that we could very well consider other definitions for logical truth, or for descriptive constants (e.g.: not completely definable or applicable without ostensive definition), but our claim here is that, once general definitions for these key terms are provided a formal structure is given to the model concept in its function as relator of theory and experience, formal structure that could be studied.

Let us however stress one more feature about the semantics of the empirical sciences: in the formal sciences model-building signifies mapping a calculus upon a fragment of set-theory. A formula F is true if there exists a set showing between its members certain relations. In order to apply this method to sentences taken from the empirical sciences, we should there also select some basic science in the language of which the truth conditions for the sentences of the empirical sciences could be formulated. But, (a) no empirical science could fulfil this function at present and (b) it is doubtful if set-theory could again fulfil its old function in this context. Certainly the tendency exists to reintroduce set-theory for the semantics of the empirical sciences and to have it serve these new needs. We can say that a system satisfies a given law L on certain variables, if the set of numbers representing measures of these variables exhibits a relationship derivable from a set of initial conditions I and from the law L.

What happens in this definition is that we define M as model of T, if there exists a structure N, standing in a certain relation to M and if N is homomorphic with the elements of a class K of models of T, with respect to given predicates. The structure N is the set of measuring results, on M, the class K of models of T is the class of models in the classical sense of the theory, sharing certain initial conditions, and the predicates are the ones corresponding to the variables measured. This is already in considerable deviation from the classical use of the model-concept, though definable with respect to it; but the essential departure is that here the model, at the limit, becomes a structure for which the propositions of the formalism are verified (not: are true). Semantics, which in the realm of formal languages was the foundation of theory of confirmation itself, here rests on the theory of confirmation (the more so, if we realise that we should add that the initial conditions under which the system obeys the law should be either the true initial conditions, or the verified or highly-confirmed ones).

We should thus confess that either we must accept this consequence, and thus define first 'theory of measurement' and 'confirmation', and only later define 'model' and 'truth' for empirical sciences, or instead, select for the empirical sciences a basic language that could be used here in the same way in which set-theory is used for the formal sciences. The search for such a basic language should not be arbitrary, because set theory has a very specific place among the formal sciences; this place should be defined, and once this is done we could look for the discipline that has an analogous place among the empirical disciplines.

Let us now make a final point: as in the immediately preceding paragraph, we see the need to study the relationship between the model and a complex structure of theories. If the model should be the intermediary between either theory and experience, or theory and reality (we saw that in both directions we have a different modelling relationship), then this intermediary character creates another problem. Let us say that a model is an intermediary between two formalisms if it is a model of both, and if the properties in the model that relate it to T1 are, in M, related to the other properties that relate it to T2. The number and kind of these relations could even be used to develop the notion of 'degree of intermediacy'. Even if we want to avoid the problems of descriptive interpretation, we can study the properties of models of such a combination of theories.

To summarise: the problem of descriptive interpretation, the problem of the empirical basic language, of the definition of interpretation through measurement and confirmation, and the final problem of intermediacy show us that here also, structure and purpose are related; definable with reference to the formal concepts but not indentical to them.

D. *Models and experimentation*

We cannot afford to lose an airplane each time we wish to see if it is able to resist under certain velocities. Therefore we build model airplanes, that we test on model velocities or pressures. If the model is adequate we should be able to derive from these model experiments the desired information. Essentially we have changed scale and have tried to leave everything else invariant. The difficulty is that when I change scale I always change something else; the problem is how to correct for the changes introduced through the scale change, or how to find a series of variables that are perhaps in their relations affected by the scale change, but not with reference to the relation we are interested in.

Two similar triangles have their sides in the same proportion even though their absolute magnitudes may be extremely different. We can generalise this concept of analogy, or proportionality, so important in Greek mathematics, and say that if a physical system is completely determined by *n* dimensions, as the triangles are by their sides, one system is a model for another system if the relations between these dimensions remain the same, even though their values are changed. As mentioned before, I cannot hope to reach absolute similitude among physical systems, but I can hope to reach correctible dissimilitude, or approximate similitude. To generalise now (and not to let ourselves be tied down by the scale factor) let us say that in order to construct models for action, there must be at least one variable among those that determine the system that may be either arbitrarily varied, or at least varied in a wide range, without modifying, or without modifying too much, the relations between the other determining variables of the system. Let us try to show in general that structural properties can be derived from this demand. Let x be a function of two variables y and z. Let the three variables be quantitative variables. When will the value of x be independent from variations in the values of y and z? If x is an increasing function of y and a decreasing function of z, and if the increase produced by an increase of y is exactly equal to the decrease produced by an accompanying increase of z (z and y being inversely related) then x will remain invariant. If we want such a function in Boolean algebra (i.e. in an elementary fragment of set-theory, the general foundation of model-theory) we can look at the function Un. (Int(x, Cy), Int (y, Cx)) where Intersection indicates the common part of two classes, Union the sum of the two classes, C the complement of a class and where x and y are used here as variables for classes).

This definition for a function of two variables can naturally be extended for n variables, and the quantitative nature is by no means needed, as shown in the example from Boolean algebra. If our variables were relations, we could construct the same example. We could now give in general the following definition: in our present sense of model, M is a model of T if both are relational structures and if the relations of both are invariant functions of the relations they do not share. The only new concept used here is the concept of 'invariant function' (and this concept, as stressed, is easily definable in general semantics).

But now that we have reached this very structural and very special-looking concept (certainly much more demanding then the classical case), let us remind ourselves that we do not need to experiment upon a system if we know it completely. Not knowing a system completely, the form of all laws it obeys are a fortiori not known to us either. The model concept we are then compelled to use is an approximation in two stages:

(a) if a set of equations is given, and if these equations are invariant under certain transformations, then two systems are models for each other if one of them satisfies the system of equations and if the second system can be produced by applying to the first some transformations under which the system equations are invariant.

Nothing implies that the system of equations is thus a complete equation system of the system.

(b) an approximation going in the other direction is this: let us know *a priori* what are the variables the knowledge of which completely determines the system, even though we do not know how this determination occurs, and what is the exact form of the equations.

Let there be given the way in which these variables depend upon the fundamental variables of our science. Then using these expressions, we shall say that M is a model of T if both have the same dimensions (here the defined variables should depend for all their values upon the undefined ones in a similar fashion).

The first approximation to physical similitude forces us to ask: what type of transformations should we consider and how should we measure the approximation to completeness, and the second approach forces us to ask the question: how can I, without knowing the function that relates x and y, say that x depends upon y and how can I, from assertions about dependence or independence alone, infer the form of this dependence? Dimensional analysis has examined these and similar problems for many special cases,[5] but the essence of our task here is to show that the same questions should be asked for relations in general, as a part of the study of the specific model concept that is used in experimental action. Our earlier remarks about the general invariance of functions shows that independence can be defined in a structural fashion. The formal problem is the following: if I have a sequence of variables and relations among these variables, what circumstances make modelling-experiments possible or impossible? If I have x, y and z as variables and if the following relations all hold FI(xy), F2(xz), F3(yx), F4(yz), F5(zx), F6(zy), and if moreover F7(xyz), and if moreover higher order interactions F8(x, Fi(yz)) exist, then what type of modelling-experiments become possible if successively either some of the higher order interactions between relations and variables are eliminated (the dependence f (xy)=g (y) is the prototype of an obstacle against modelling), or some specific conditions are introduced? In view of this question, we reach the following definition of model: if two systems, ac-

cording to dependence-independence tests, are determined by the same fundamental variables, which, according to a given dimensional analysis, are determined by a set of equations where in a certain kind of interactions does not occur, then those two systems are models for each other.
Let it then here be said that the double relativity of the dependence tests that are not complete descriptions, the dimensional analyses that are not unique, and the syntactical characteristic about the existence of laws of certain forms, makes this model-concept extremely specific, both more severe and more lenient, as always, than the one we know so well from classical semantics.

E. *Models and explanation*

Models are given as explanations of the systems they are models of. Why should models be needed for this purpose and how can they explain? There is perhaps no clearer refutation of the so often heard thesis according to which to explain is to infer, then the fact that explanation occurs so often, or even nearly always, through model building.
The definition of explanation is once again one of the unsolved problems of the philosophy of science. It is thus very difficult to determine the structural properties a model should possess in order to be able to explain. We only want here to propose our personal hypothesis, without claiming more than plausibility in its favour. If we look at the history of science, we see that, in physics at least, two major explanatory models have been dominant: the atom model and the field model, the discontinuous and the continuous, the pluralistic and the monistic, notwithstanding the fact that neither atoms nor fields are familiar or simple entities. In the theory of gases, in the theory of light, in cosmogony, in the theory of electromagnetism, in nuclear physics at the present moment, these two explanatory models have always been influential. To state this fact more formally: physics seems to try to reduce all law either to the laws of Newtonian mechanics, or to the laws of Maxwell's electromagnetism, and, if possible, to both. Can we extrapolate towards the future, or towards other sciences? We could try in various ways to understand this tendency: if explanation is the derivation of the observed facts (always presenting a mixture of foreground atomism and background continuity) from premisses at a maximum distance from these observed facts, then we could claim these to be the two extremes. If explanation is analysis, then

we could try to prove that the two extreme poles of analysis are the reduction of all plurality to the unity of the field, whose features will have to be responsible for the observed universe, or the reduction of all order and unity to the pluralism of the particle, on whose disorder our order is to be built. If to explain is to make anthropomorphically understandable, the world of the discontinuous is the world of the tool, and the world of the field is the world of the environment. We can only offer these suggestions as guesses; our only excuse is that nobody seems to have better ones. At least these guesses explain why we need model building to explain. In terms of this hypothesis, an explanatory model is representation of a theory in the theory of complete or partial differential equations. The philosopher of science should certainly describe in more general terms what distinguishes these two theories from other ones, in order to understand their privileged position. But even before undertaking this task he can state that approximate models will have to be introduced in order to provide for systems having a very different structure a model in terms of these differential concepts.

Once more, we find a very specific addition to the classical theory of semantics and also a very specific generalisation.

F. *Simplification and model building*

It is rather paradoxical to realise that when a picture, a drawing, a diagram is called a model for a physical system, it is for the same reason that a formal set of postulates is called a model for a physical system. This reason can be indicated in one word: simplification. The mind needs in one act to have an overview of the essential characteristics of a domain, therefore either the domain is represented by a set of equations, or by a picture or by a diagram. The mind needs to see the system in opposition and distinction to all others; therefore the separation of the system from others is made more complete than it is in reality. The system is viewed from a certain scale; details that are too microscopical or too global are of no interest to us. Therefore they are left out. The system is known or controlled within certain limits of approximation. Therefore effects that do not reach this level of approximation are neglected. The system is studied with a certain purpose in mind; everything that does not affect this purpose is eliminated. The various features of the system need to be known as aspects of one identical whole: therefore their unity is exaggerated. When isolation, clarity, unity, closedness, essentiality and homogeneity of perspective and viewpoint are reached, an adequate model either expressed in equations or in a drawing is formed.

Moreover, both in the verbal and in the pictorial model, we represent either parts of the system and their connections, or states of the system and their connections, or simultaneously parts and states and their connections. The aim of simplification and globalisation will however be more accurately served through concentration on one of our two main sub-goals.

Let it be clear that here the model should not be richer than the system it is a model of, but poorer. The model in the service of the progress of science should be more complex; this one should be more completely focussed. It is in this sense that the ethical meaning of the word model meets the epistemological meaning. For certain ethical systems (not for all) the ideal man is the model of man, in this sense of the word model (an exaggeration of idiosyncrasy, a clarification of internal structure). We think that it is easy to recognise that the features of elimination of certain predicates, elimination of certain parts, closure for parts and states, regularisation of the overall structure are structural demands that can be defined for very general relational systems in a truly general semantics. The importance of isolating this type of model concept lies in the fact that it cuts through the opposition of image and word, and explains why (as so often in economy) a system of equations is called a model, where a few pages earlier or later, a picture had the same attribute.

Here we want to close our review of the structural correspondents of our functional characterisation for models in the empirical sciences. We claim that we have made it plausible that a thorough analysis of the different aims of model-building shows us that very definite structures are needed to achieve these aims and moreover (and this is centrally important) (i) *that these structures depart from the classical concept of model in many different ways but* (ii) *that they can be studied and ordered, using this same classical concept of model as a centre of perspective.*

We now want in our third section to show that if we start from the classical concept of model and if we apply to it certain natural operations of strengthening and weakening, we reach from the opposite direction the structures we tried to define from a functional point of view before. We shall thus be able to a certain extent to show that the science of formal semantics could still fruitfully be studied with reference to these more general models.

III. CLASSICAL AND GENERAL MODELS

Conceptually if model and prototype are both general systems or structures, we have the most abstract case. If we impose on one of the terms of the relationship the demand that it should be a language, we have a more special case, and if both terms should be languages we have the most special case. The natural order then to study generalisations of classical model concepts is to take the algebraical model concept first, to procede afterwards to the study of the semantic concept of model (relating a language to an arbitrary domain) and to finish finally with the syntactical concept (relating two languages to each other).

A. *Algebraic models*

Our purpose in this section will be to define several approximate or strengthened forms of isomorphism. To reach this aim let us first realise what is included in the notion of isomorphism.
Two sets D1 and D2 are isomorphic with respect to relations R and S, defined respectively on D1 and D2 at least, if the following situation occurs: there exists a mapping function F such that to each member of D1 there corresponds one and only one member of D2 under F, and if moreover whenever members of D1 stand in the relation R, their F-images stand in the relation S, and inversely, then we say that the two domains are isomorphic under the two relations R and S. Two sets will be completely isomorphic if they are isomorphic under all their relations. Two relations will be completely isomorphic if they are isomorphic on all their domains. Two sets will be called isomorphic with reference to a class K of relations if the relations under which they are isomorphic have to belong to the class K. Two sets will be called isomorphic under a set K of relations if the mapping relations that correlate the relations on the two sets have to belong to a given class K.
A well-known and much-used generalisation of isomorphism is homomorphism. Here the correlator has not to be one-one but is allowed to be many-one. As for the rest, the relations remain the same.
We wish to consider approximative isomorphisms and homomorphisms and take some steps towards ordering or even measuring these approximations.
We give some possible forms of approximation.

Approximation I: The correlator may be such that this function does not map the whole of the domain of R on S, nor the whole of the domain of S on R. We then have an approximate direct or inverse correlator. A correlator A will be a closer approximation than a correlator B if the class of members of the field of R that is not mapped upon the field of S by A is a proper subset of the class of members of the field of R not mapped upon the field of S by B. If we possess a measure on our sets we can define one approximation to a correlator as better than the other one, if the measure of the non-mapped set is smaller in the first case.

Approximation II. The correspondence may be such that not always when R exists in D1 between some elements, S exists in D2 between the images of these elements. The correspondence C_3 is a closer approximation than the correspondence C_2 if for C, the *n*-uples where the images have not the corresponding relations are a proper subset of the set of *n*-uples which for C_2 do not have the corresponding relations.

We shall say that an App. II-neighbourhood-relation isomorphism exists if for all cases in which the correspondence does not hold a relation lying in the neighbourhood (provided such a concept is defined) of the relation that should occur, holds. We shall say that an App. II-neighbourhood-element isomorphis exists if for all cases in which the correspondence does not hold, some elements in the neighbourhood of the image elements present the desired relation.

We wish to stress that approximation-neighbourhood isomorphisms of type I (both of relation and element kind) can be defined in the same way. If the definition of neighbourhood for one approximation is a refinement of the definition of neighbourhood for another approximation then the first is a closer neighbourhood element or relation approximation than the second.

Approximation III. An App. III-isomorphism is both an App. I and an App. II isomorphism.

Approximation IV. In all preceding approximations we have considered the set D and the relation R to be classical sets or relations, defined everywhere, and precise everywhere. Let us now introduce for sets and for relations indetermination domains; i.e.: elements for which it is not decidable if they belong to a set or not and couples for which it is not decidable if they belong to a relation or not D'1 is a closer approximation to D1 than D''1 if the determination domain of D'1 has a larger intersection with D1 than that of D″1 (if no measure is available: if the first intersection includes the second). D′1 is moreover a tighter approximation to D1 than D″1 if the indetermination domain of the first is included in the indetermination domain of the second. Finally D′1 is a more adequate approximation to D1 than D″1 if the intersection of the indetermination domain of D′1 with D1 is included as a proper part in the intersection of the indetermination domain of D″1 with D1. It is important to understand that a closer, a tighter and a more adequate approximation to the same set are by no means always identical. Moreover, we have supposed here the set D1 to be exact. It is obvious that we should also consider approximations to inexact sets.

A relation can have an indetermination region for its domain, and for its co-domain. If we want to consider isomorphism between approximate sets and approximate relations we meet first the problem of the intersection of the indetermination region for relations and for sets. Either one of both can be zero, or both can exist; if they both exist they can intersect (even be included in each other) or be disjunct from each other. It is obvious that the case in which they coincide will be the easiest one for our purpose.

Let us say that if we have a set with an indetermination domain, and on this set a relation R with again an indetermination domain, then there is exact isomophism between both if there is a correlator mapping members of the kernel of D1 on members of the kernel of D2, members of the indetermination domain of D1 on members of the indetermination domain of D2, and such that if R holds between elements of D1, S holds between elements of D2 and if a *n*-uple is in D in the indetermination domain of R, the images of it are in D2 in the indetermination domain of S. This strict isomorphism immediately gives birth to a series of approximations: we may call alpha IV approximation the case where the correlator maps elements of the kernel of D1 on elements of the indetermination domain of D2, (or inversely), beta IV where elements of in determination domains are mapped on elements of kernels, gamma IV where elements of indetermination domains are not mapped (and thus in a sense mapped on complements), delta IV approximation where *n*-uples in the kernel of *R* are mapped on *n*-uples in the indetermination domain of *S*.

We shall call one isomorphism closer or tighter or more adequate if a closer, tighter or more adequate approximate to both D1 and *R* is correlated with a closer, tighter or more adequate correlate to both D2 or *S*. This definition again gives us the occasion to develop a sequence of isomorphisms: we can map namely a closer approximation to D1 on a non-closer approximation to D2, or we can have the approximations to D1 and D2 closer but not the approximations to *R* and S, or we can have all closer but only some tighter and so forth.

Approximation V isomorphism: let us be aware of the fact that in approximation IV both correlator and correspondence were exact relations. The great multiplicity of cases already encountered was due to the indetermination of D, *R* and *S* and not to that of correlator or correspondence.

It is now the place to mention that we can combine any form of approximation IV, with approximations I, II or III. Here the situation becomes extremely complex and we can only stress this feature. Let it also be stressed that even in approximation I, II and III, the correlator and the correspondence had no indetermination domains. The two features we are thus liberalizing here, in V, are radically independent: we consider imprecise correlators and correspondences, and we consider moreover partial correlators and correspondences. It is perhaps best that we distinguish approximation V, alpha (subspecies: I, II and III), approximation V beta (subspecies: all subspecies of IV) and approximation gamma (both alpha and beta).

Approximation VI. Let it now be stated that in all the previous cases, even if we depart very strongly from the simple classical picture, we have been presupposing that a system can be described as a relational structure with given clear-cut relations and elements, the system having perhaps indetermination domains and the relations also, but these entities themselves being given and the wholes being built up out of these parts. It seems clear that this is a radical dependence on a type of logical atomism that geometry precisely tries to overcome through the construction of a geometry without points (6). An algebra without elements seems as urgent a desideratum if we want to develop the model theory for natural systems. Let *n* systems be given. We do not presuppose the concept of element or of relation, but we define them with respect to these systems. An element is the smallest system that systems can have in common (as always in these topics, we presuppose the part-whole relationship and some of its properties,) and a relation is a minimal system of sequences that systems can have in common. The definition of what is to be called element or relation should thus be a funetian of the intersection of systems. We cannot hope to develop here a complete theory for these approximate concepts. We hope to have shown that they exhibit interesting complexities.

Before we try to apply them in the domain of semantics proper, let us point out one direction in which the study of our approximate isomorphisms might profitably develop.

Alfred Tarski in his 'Contributions to the theory of Models' [7] has defined the concepts of relational system, subsystems of relational systems, similarity of relational systems, homomorphism and isomorphism of relational systems, union and cardinal product of relational systems, and finally the concept of elementary or arithmetical classes of relational systems. The first task of the generalisation of formal semantics that seems necessary in the light of our study of empirical models in the empirical sciences, would be the application of some of the simpler forms of approximation defined above to the definition of these concepts. This is the more necessary because empirical models, as we have seen, are very often models through certain of their subsystems, and they are models of unions or cardinal products of other systems, all these concepts taken in some approximate sense. This task is not only necessary but possible. If a relational system is an arbitrary sequence consisting of a set, and of a series of finitary relations of given rank, all defined on the elements of this set, then an approximate relational system is also a sequence where the first element is a set with an indetermination domain, where there follows a series of relations with indetermination domains, *nearly* all of them *nearly* everywhere defined on the set, having all of them ranks *withing certain intervals*, and having *nearly* all of them finite rank.

The phrase 'nearly all' can either be replaced by some quantitative provision (giving the length of intervals, or the measure of sets), or by some qualitative provision (stating that intervals or sets are included as proper parts in certain others). Two relational systems will be approximately similar, if they are approximately of the same order (difference between order lying in a certain region), and if the relations can be correlated with each other so that the correlated ones have not too large differences of rank in too many cases (again, we do not indicate here the precise way in which we could write these phrases). A relational system is a subsystem of another one to which it is similar (one could replace this by 'approximately similar') if the set of the one is nearly completely included in the set of the other (the intersection of the first with the complement of the second being sufficiently small in metric or inclusion terms), and if the relations of the first are nearly all nearly identical to restrictions of the second to one of its subdomains. In terms of approximate subsystems, it is no longer true that $SS(K) = S(K)$ as it is true for precise subdomains, (though S(K) will be included in SS(K), where S is the set of subsystems of its argument and where K is any relational system). We do not have to repeat the definitions of isomorphism. There are as many generalisations of isomorphism as there are approximations mentioned. The approximate union of two precise relational systems (to be opposed to the precise union of two approximate relational systems and to the approximate union of two approximate relational systems) is the relational system that has, as set, an approximation to the union of the two sets, and as relations an approximation to the unions of the corresponding relations. The set of the cardinal product of two relational systems consists nearly completely of nearly all pairs of nearly this form (a-b), with a in R and b in S, and where the relations take, for *nearly* all their elements, pairs with *nearly* all their first members from R in the corresponding place and *nearly* all their second elements from S in the corresponding place. Here also a detailed investigation of the theorems about isomorphs (of different degrees of approximation) for unions and products (of different degrees of approximation) will yield necessary and interesting results.

Let us now however make a general remark: it is easy to define strict isomorphism for abstract relational systems given by their fundamental operations. For a group the relation to be preserved under mapping should be multiplication, for a ring addition and multiplication, for an ordered set, the ordering relation, and so forth.

If we have however two physical systems, or two languages, how are we going to pick out the relationship that is to be preserved under mapping?

It may be shown that even for physical systems a natural isomorphism concept can be defined. It has been recently most clearly explained by Ross Ashby, in the paragraphs on models occurring in his recent book. [8] Let us however stress that Hertz's definition of dynamical similarity is close to Ashby's (9). The relation that is required to remain invariant under the mapping is here the relation of 'going from one state into another under the influence of an input'. We characterize a physical system by an input-output matrix, or by a matrix giving for every input and present state the following output, or giving for every input and every state the next state. All these possibilities are open, and mostly equivalent. On p. 98 Ashby then states 'the canonical representations of two machines are isomorphic if a one-one transformation of the states of one machine into those of the other can convert the one representation into the other'. Only for strictly deterministic systems, however did, Ashby provide his definition (i.e. for systems we know completely, and we are interested in for their complete behaviour). We have seen repeatedly in our second section that only when systems are partly known is modelling needed. We shall thus have to adopt definitions of physical isomorphism for systems whose input-output (or state, or input-state, or output-state) matrix we cannot completely write down. We shall have to consider probabilistic systems (for which I can only give the probability for a given input to have a given output), or even weaker ones (for which I can only sometimes say that one output is more probable than another for given input and state. Ashby is aware that he must liberalize the relation between prototype and model and cannot identify it with the relation between isomorphs. However the only way he succeeds in doing so is to say that a model M is such that one of its homomorphs is isomorphic to one homomorph of the prototype. This liberalisation still obliges us to know the complete deterministic state matrix of both model and prototype, and thus does not yield the desired result.

It is clear that we can always represent a relation by a matrix and that the approximations to isomorphism we have been considering should find their adequate translation in conditions on the reciprocal relationships of matrices. These reciprocal relationships will be the real translation of the modelling relationship between physical or biological systems, as it occurs in physics and biology. The contribution of Ashby consists however in having shown that the state matrix is the adequate tool to define isomorphism between natural systems, even if the liberalisation he proposes for the classical relationship is not one we can adopt. His approach allows us to assert that our approximate isomorphisms will easily find application in this domain.

B. *Semantical Models*

The concepts of truth, designation, satisfaction and definition are closely related to each other and the search for a liberalised version of one of them will be reducible to the search for a liberalised version of some others in this series. Let us mention in this context that Herbert Simon, in his recent paper 'Definable Terms and Primitives in Axiom Systems' (10), proposes two generalisations of the concept of definability that correspond immediately to generalisations of the concept of satisfiability. A language L, according to Simon, has a generic definition for an individual *a*, if in L we can write down the definition for a class to which *a* belongs (i.e.: a necessary condition for *a*). A language has an approximate definition for *a* if in L we can write down a necessary and sufficient condition for any *x* to be *a*, except if *x* belongs to a certain set of measure zero.

A domain generically satisfies a certain statement or set of statements if it satisfies certain statements belonging to the same class as these, or if some model belonging to a class K with the present one, satisfies the set of statements.

A domain satisfies approximately a set of statements if every part of it, except a subset of measure zero, satisfies this set of statements.

It is clear that our transposition of Simon's general definitions for definability towards definitions of satisfiability is perfectly possible. It is however difficult to believe that the two concepts Simon introduces are the only or most fruitful ones: generic definability or satisfiability is extremely weak and completely depends on the criterion of class membership chosen; approximate definability or satisfiability strongly depends upon the existence of a measure function, and on infinity and continuity considerations (all denumerable sets having measure zero).

We feel encouraged by this attempt in our search for an enlarged and liberalised relation of satisfaction, but we do not think the main task is already done.

In 'Logic, Semantics and Metamathematics' (p. 416) A. Tarski [11] defines the concept of model. Let in language L to every extra logical constant correspond a correlated variable in L, in such a way that every sentence becomes a sentential function if the constants are replaced by the variables. An arbitrary sequence of objects satisfying these functions will be said to be a model or a realisation of that class of sentences. The semantical model concept is thus both akin to and very different from the ideal of modelling relationship as isomorphism. The prototype-model relationship is here by definition anti-reflexive, anti-symmetrical, but transitive; while in its algebraic version it is reflexive, symmetrical and transitive. But whenever *n* systems are models of the same language L, there exists between these systems, or between parts of them, relations of isomorphism. This implies that to the generalisations for isomorphism we have introduced, there must correspond generalisations of the satisfaction relation. The main problem of this section is: if we claim that among *n* domains some specific approximate isomorphism exists, is there (a) a language they then all strictly satisfy or (b) is there a language they all approximately satisfy? The solution of these two problems will not be given here; we only ask the question. But a step towards the solution will certainly be taken if we define some of the natural liberalised versions of satisfaction itself. Let us give the following definitions:

A set factually satisfies a sentence *p* of a language L if and only if the variables of *p* range over the set S, the predicates of *p* over subsets of S, the logical constants are interpreted as usual, and the sentence becomes true for S.

We shall say that approximate satisfaction can be defined with reference to assignments for individual variables, for predicate variables, for logical constants, or for truth.

We shall number these types of approximate semantical satisfaction as Approximate satisfaction 1, 2, 3 or 4.

A set appr. 1 satisfies a sentence if for variables ranging over some subset or superset of S, *not too distinct* from S, all other properties remain identical.

The closeness of the approximation will again be measured by the inclusion relations of the Inters (Range Variable, Complement S).

A set appr. 2 satisfies a sentence *p* if the predicates of *p* range over a class of subsets of S plus or minus certain elements, and all other properties hold true.

Logical constants could be otherwise defined than usual (through the rules of propositional calculus and functional logic). Here, in order to define what is the ordering principle of the approximation, we should be able to define what is a closer approximation either to negation or conjuction itself or to the classical interpretation of these constants.

Finally approximation 4 replaces true by an approximation to true (the one that comes immediately to the mind is: probable, but we are by no means forced to select this type of approximation.)

It is obvious that if we give ourselves weaker conditions and preserve the same conclusion, we then obtain a strengthened version of the satisfaction requirements.

In this respect however, it is, we think, interesting to refer to certain approximate but strengthened forms of satisfiability.

Let us say that a domain constructively satisfies a language L if for every sentence of L we have a procedure that allows us to construct the domain in question and the various individuals and classes needed for the verification of the truth of the sentences. This procedure for geometrical concepts is even expressed in L when everything is so defined that the postulates given realise a method of measurement for these concepts. This constructive feature may now only approximately exist, either as a procedure given in most but not in all cases, or as a procedure yielding close but not completely exact correspondence. This we should like to call the approximate constructive satisfaction relation.

Let us say that a domain necessarily satisfies a sentence if many possible assignments within this domain, or many possible extensions of this domain, or many possible other domains satisfy this sentence. The degree of necessity could easily be ordered. We can then speak about necessarily approximate interpretations and approximately necessary interpretations; the approximate interpretation can hold perhaps in many other selections or domains; or in most but not all of a class of chosen domains.

The relationship between formal languages and domains in which they have models must in the empirical sciences necessarily be guided by two considerations that are by no means as important in the formal sciences: (a) the relationship between the language and the domain must be closer because they are in a sense produced through and for each other; (b) extensions of formalisms and models must necessarily be considered because everything introduced is introduced to make progress in the description of the objects studied. Therefore we should say that the formalisation of the concept of *approximate constructive necessary satisfaction* is the main task of the semantical study of models in the empirical sciences. Here however, once more, we should stress all the difficulties of the undertaking: approximate modalities and approximate recursiveness should be the basic tools of this enterprise, basic tools that should then be applied to the model-prototype relationship.

We meet here another major problem for our generalised semantics: if we decrease the requirements a system has to satisfy in order to be a model for a language (as we are constantly doing, when we apply approximate concepts), and if we apply simultaneously strengthened model concepts (as we do when we speak about recursive and necessary representations) can we in some sense compensate our first loss of properties by a corresponding gain, so that our results are either identical or at least equal in power of deduction? This is perhaps the deepest problem we should try to solve in building up these approximate systems of semantics.

Let us give some more details about liberalisations of semantical concepts, details whose function it is to apply the notions of our section III a to the most popular model definition. Usually, a model is defined in two steps, the first of which defines what is called a semi-model and the second of which gives the full model.

The domain[1] is circumscribed on the one hand, the language L on the other hand. Simple expressions are defined in L, classes of elements are defined in D. Rules are then given to assign to each non-complete expression in the language an element, a class or a sequence of elements in the domain (these elements, classes or sequences may belong to very many different categories, first defined). It is then shown that every term has an assignment. This assignment or semi-model is moreover a model if all asserted sentences in that language are assigned in their category to one definite sub-category, in which nothing else but these asserted sentences is assigned the truth. The recursive building-up of higher-order categories and of higher-order complex sentences is thus essential for the definition of model. This being the general description of what it means to define a model (as stated for instance by Kemeny and Tarski 12) it is now clear what will be the dimensions along which approximations will have to be defined:

(1) approximations to the language on the one hand, the domain on the other hand, (2) approximations to the category of simple signs on the one hand to elements of the domain on the other hand; (3) approximations to the subdivision of the domain of simple signs in L, of the domain of simple elements in D; (4) approximations in language and domain to the several complexity-creating operations; (5) approximations to the vertical

division in expression-classes or element-categories; (6) approximations to the various assignment rules that have to be combined with all the other types of approximation. To be brief, we could say that all the earlier categories of an algebraical nature can here be applied but that *we must apply them to two hierarchised structures built up out of a given basis by a few iterated operations.* The semantical approximation should thus be approximation to a correspondence between two hierarchies. This is the only new and important feature that occurs. But along all the mentioned dimensions of approximation, all forms of approximation distinguished have to be applied, and moreover they have to be combined (the major problem being: How is the approximation form and degree at a higher or later stage dependent on the approximation level and degree at a lower level and stage?)

We should however be aware of the fact that it is difficult to discuss in general liberalisations of the satisfaction or model concept. Only for specific languages can this concept be defined, because the sentences stating what types of entities satisfy what simple sentences are purely relative to the language in which we find ourselves. We should thus in fact select a given language and produce a liberalised version for the satisfaction concept there. This cannot be our aim here, but the broad outlines of such an undertaking have, we think, sufficiently been sketched.

To summarize our results: the more complex an entity, the more difficult it is to define an approximation to that entity, because so many different dimensions exist along which the approximation should be defined. A language is such a complex structure, and defining approximate semantical isomorphism means defining an approximate relation of correspondence between approximations to a language and approximations to a domain (set).

We see that the difficulties we encounter could in some sense depend upon the notion: approximation to a language.

This is the central concept of the last part of this section.

C. *Syntactical models*

Here we are going to study the relation between model and prototype as a relation between two languages. We consider as languages the different sciences, and this forces us immediately to consider certain generalisations with respect to formal calculi. For most scientific systems, the distinction between axioms and rules of inference for instance is not clear. The series of definitions is not closed and it is not a mechanical procedure to know if a given sentence belongs to the system or not. Most scientific systems do not have a clear distinction between logical and descriptive constants. In fact a science seems to be written in a mixture of everyday language describing experiments, mathematical equations describing calculations and semi-formalised deduction if the science is sufficiently advanced to contain a theoretical part.

We neglect all semantical and pragmatical features, because we consider here only the syntactical aspects.

In opposition to this very complex mixture that is a science (when this science is taken as it is and not transformed for philosophical or logical reasons into something else), what is a calculus? A calculus is a sequence of the following entities: a set of signs, a set of sequences of these signs that are well-formed formulae, a set of well-formed formulae that are theorems, and a set of axioms (or, equivalently, a set of sequences of well-formed formulae that are rules of inference). The sequence (S, F, T, A) is thus a calculus. We can assert certain evident relations between the different elements of this foor-term sequence and one of the most interesting properties is that S, F, A ought to be recursive and T recursively enumerable.

The facts just mentioned about an empirical science seem to indicate that there the elements of this sequence are not recursive, that each of them has indetermination ranges, and that moreover the four ingredients of the calculus have indetermination regions in common. This leads us to believe that if we start with the general concept of calculus and if we define various types and degrees of approximation to calculi (and mixtures of calculi), we shall probably have among them the specific features of our empirical sciences.

A language L will be called a closer approximation to a calculus than a language K if and only if (a) there are more signs in L that are indisputably simple and belonging to the calculi than in K where either fewer signs are indisputably simple or fewer simple ones are decidably elements of the calculus; (b) there are more sequences in L than in K that completely consist of signs of S and that are moreover indisputably well-formed (against cases in which sequences have as elements border-line signs or are themselves on the borderline of well-formedness); (c) there are more sequences that completely fall into the kernel of the set F and that are in the kernel of T; (d) there are more sequences indisputably consisting of members of S, indisputably in F and T, and members of A. This defines the general idea of approximation of a language to a calculus in general. When now is a language an approximation to a given calculus in particular? We think that we could here give a fairly general answer. In fact, a calculus is dependent upon a classification of signs, of sequences of signs, of sequences of such sequences and sub-classifications of these. It is an ordered sequence having as elements classes chosen from such classifications. The principle of the order exhibited in this sequence is given by the properties: T lies inside F, A lies inside T, all parts of members of F are members of S, and none of the inverse characteristics hold. We can say that a classification is an approximation to another classification the more classes of the first coincide more completely with classes of the second.

A hierarchy of classifications is the more an approximation to another hierarchy, the more each level corresponds to the corresponding level, and the more the relations between each pair of successive levels mirror the relations between the other pairs of corresponding levels. The degree of approximation of one hierarchy to another depending thus on at least two factors (and on the definition of corresponding level), the same degree of approximation could correspond to very different situations. Presumably proper weights should be chosen to determine the importance of each factor in the determination of the closeness of approximation to a given calculus. A calculus being essentially a selection from a given hierarchy of classification, this trend of thinking can lead to a definition of the ordinal degree of approximation of one calculus to another. The limit of a class with respect to a relation (in *Principia Mathematica*) is an element such that it stands in the converse of R to every element of that class and such that for every element in the class there is another element in it such that the first has the relation R to the second. If we now take a as elements classes and as the relation a relation of inclusion we can define the limit of a series of classes; and, as a consequence, also the limit of classifications and of hierarchies of classifications. Once this is done nothing can prevent us from defining limits of languages. We certainly could solve the problem of ordering approximations to languages without defining limits for sequences of languages, but it is certain that if we could define such limits, our ordination difficulty would most neatly be solved. We must however limit ourselves here, more than ever, to tentative non-formal remarks.

Let us repeat that we could define neighbourhoods for languages and calculi in function of the neighbourhoods eventually defined for S, F, T and A (the classical neighbourhood-axioms could presumably easily be satisfied).

The syntactical model concept is defined in Tarski's book *Undecidable theories* essentially for 'systems in standard formalisation'. These systems are calculi in the sense we have just discussed with very careful sub-classifications of the series of signs, and of sequences of signs, and of the series of axioms. We can neglect the particular nature of these sub-classifications as the general problem of approximation remains the same for all these refined versions.

Having thus understood how we can relate systems in standard formalisation to languages in science, we can now come to our main topic, the study of the syntactical interpretation relation as defined among standard formalisms, and its approximations; this will be an introduction to the study of generalisations of this syntactical relation as defined among approximations to standard systems.

Tarski tells us, on p. 20-22, for systems in standard formalisation that a system is interpretable (13) in another one if we find in this other system a series of definitions for the basic terms of the first that give to these basic terms their usual properties.

Tarski, using his distinction between logical and non-logical constants, applies this first to the case where only for non-logical constants are definitions contemplated, and where among the descriptive terms, only constants are to be interpreted. Later he admits, in a footnote on p. 22, that logical constants may also be interpreted, and in his method for the relativisation of quantifiers he in fact also reinterprets variables. We shall come to this later.

How might we consider weakened forms of these situations? We could envisage only partial definitions, or definitions of only part of the basic terms, or definitions giving to these basic terms only part of their earlier properties; or definitions that are not partial but multiple and probabilistic (every sign receiving a sequence of definitions with different cardinal or ordinal probabilities). These three directions of generalisation still presuppose that the system to be interpreted and the interpreting system are both in standard formalisation! Only the interpretation relation is weakened.

Let T1 and T2 not have any non-logical constants in common, then T2 is interpretable in T1, if and only if there is a theory T and a set D, satisfying the following conditions: (a) T is a common extension of T1 and T2, every constant of T being a constant either of T1 or of T2; (b) D is a recursive set of sentences, valid in T, and possible definitions in T1 of non-logical constants of T2; (c) every non-logical constant of T2 occurs in at least and at most one sentence of D; (d) all valid sentences of T are either those of T1, or of D. (Tarski, 13). To understand the force of the definition it is necessary to understand that a theory is an extension of another one if every sentence of this other one that is valid (we should try to find some syntactical equivalent for this term) is also valid in the first.

Tarski himself weakens his definition in one direction, but let us first stress the following points:

– T might not be a common extension of T1 and T2; in other words: in order for the interpretation to be possible, certain properties that hold for T1 would cease to hold;

– the set D might not be recursive; its contents might not be possible definitions for the constants of T2 but only partial ones, or only properties, or only probabilistic sentences;

– the non-logical constants of T2 might occur in more than one sentence of D (over-determination) and not all of them might occur;

– there might be valid sentences of T neither in T1 nor in D (i.e. certain properties of the defined signs are no longer derivable in the interpretation and must be added independently).

These possibilities all go in the direction of the weakenings we contemplated informally above.

Tarski's own 'weak interpretability' goes in the direction of our last possibility (extension is preserved, all constants must be the same, but new truths about the same topics might be needed. Our last possibility is wider than his weak interpretability because it considers interpretation in a super-theory, not having necessarily the same constants.

If we now cease to consider only formalisms in standard formalisation and if we try to introduce approximations to the standard formalisation,

we encounter two very divergent cases that have to be studied: (a) can we adapt Tarski's 'strict' definition of interpretation to the case of approximations to standard formalisations? and
(b) can we adapt the weakened forms of it we just suggested to approximations to standard formalisations?
The first case covers two different situations; it may be that both the systems T1 and T2 are approximations to systems in standard formalisation, or that one of them is a standard formalised system (more generally: the mode and degree of formalisation in both cases may be the same or different).
If, for instance, instead of knowing or not knowing that signs belong to T1 or T2, we have a continuous spectrum of probabilities and know that the probability that they are in T1 belongs to a given probability interval, we might paraphrase the first condition Tarski puts forward for interpretation to exist, as follows: if a given sign lies in a probability interval *i*, regarding T, then it lies in the same probability interval regarding either T1 or T2. (we could loosen this up by saying that there is at least one among the two languages such that the probability interval of this sign in this language is not too far away from the corresponding interval in T.) This probability might be ordinal.
Such a scheme tries to keep between two not completely formalised systems the same strict relationship that was asked for in the case of two completely formalised systems.
Let us try to look for a similar transcription of the first demand for another type of approximation: let it be given that a lump of signs or sign sequences belong to a language, there being no method available to dissect this union, the elements of which remain undiscriminated. This entails the presence of a union of sentences equally indistinguishable and speaking about the given signs. We could now, by analogy with the earlier procedure, say that the lumps in T should be such that they coincide either with some in T1 or with some in T2 (or we could say that every element present in a lump of T should be also present in some lump of T1 or of T2). It is obvious that we here try to apply to the concept 'formal system' in the first place and the concept 'interpretation' in the second place, some of the concepts of approximate geomtry introduced by Hjelmslev and applied already by Menger to the calculus of relations.
This analogy suggests that exactly as in the geometry of solids, the point

is defined by a series of approximations, we should develop a syntax and semantics of approximately defined systems that might take over some of the techniques of this geometry of solids, the problem that has been tackled in both cases being the same: starting from a formally clear situation approach reality by generalisation that makes lesser demands on powers of discrimination.

The situation we have encountered in our attempts to preserve the strict modelling relationship for not-strictly-defined systems is a situation we have by no means exhausted (we should stress that for each of the rules of interpretation and for each of the modes of approximation new difficulties arise). To see what type of difficulties might present themselves, let us look at a closely related case: suppose we admit logical constants themselves to be interpreted. How then to interpret the demand that the logical relations of the interpreted symbol of descriptive nature should be the same as those of the initial symbol?

So also the interpretation rule for signs must be formulated so as not to contrast with the interpretation rule for sequences and so forth.

Let an approximate interpretation be called adapted to the mode of approximation of the languages that are model and prototype, if the dimensions along which the interpretation approaches a formally complete interpretation are the same as those along which both languages approach the status of formal calculi. An interpretation can certainly be unilaterally adapted, because the mode of approach of model and prototype is not necessarily the same. An interpretation can certainly be of different degrees of inadaptation. For instance let model and prototype be close to formal calculi except for n signs, whose belonging to the system is uncertain. An interpretation is then adapted if it is a formal interpretation except for n rules of assignment for simple signs, that remain equally uncertain. If the form of uncertainty is specified (as a degree or through a disjunction) we can even require closer analogy. Let it be said however that the signs for which the assignation rules are undetermined are not necessarily those for which uncertainty of belonging exists. Even for approximate interpretations that are adapted to their terms such a multiplicity subsists. What will not be the complexity of approximate interpretations not adapted to the mode of approximation of their terms?

We want now to complete our survey of modes of syntactical interpretation by the study of Tarski's relative interpretation.

Given a theory and a unary predicate P we construct an interpretation by relativisation with respect to P if we replace all sentences quantified and including x, by sentences quantified and having the hypothesis that their x'es have the predicate P. Relativisation with respect to a predicate of the domain of individuals should perhaps be more generally defined as: relativisation of a domain of entities (propositions, predicates themselves, relations), with respect to an element of a domain of entities that they can be applied to or that can be applied to them by admissible operations (in a propositional calculus we might replace every p by a clause (qIp) for instance; similar changes might occur in higher functional calculus).

It begins to be rather tiresome when we stress that the relativisation clause might be imposed only on certain predicates, or might be imposed diversely with diverse probabilities, or might concern a non-unique predicate but the union of a sequence of predicates.

The relationship between interpretation by relativisation in the strict case and interpretation by definition is not yet completely clear; it should not astonish us that *a fortiori* the relationship between approximations to these two types of interpretation on the syntactical level is a topic still awaiting investigation.

If a science is really something rather different from a formal system, it should meet not only weaker demands, but also stronger demands. Here our problem is much more undetermined than elsewhere: we know from long experience how to generalise certain conditions; but it is not clear how we should strenghten them.

Certainly, from the syntactical point of view, a science is not only any axiomatisable or unaxiomatisable formal system but has some stronger type of unity. The axioms should not be unrelated. They should in some sense be 'about the same topic'; moreover the set of theorems should present first some type of symmetry, second some type of mutual involvement. How formally to represent this type of symmetry, unity or involvement as strengthened versions of the requirements for 'systems in standard formalisation' is far from clear. Moreover, it is quite true that the usual models, even from a syntactical point of view, are not given by arbitrary rules of definition for the constants of T2 in T1. The definitions should delimit a domain of objects that in T1 itself is necessary as a separate domain, having sufficient distinctness and standing out against other parts of T1 having similar distinctness; to derive from the rules in D the properties of the defined terms, the derivations should also have some sufficient unity, showing that the defining characteristics are in some sense grasping the nature of the constants in question. How to define the naturalness of this inference and thus of the definition is however difficult to see.

Heuristically we could follow two directions: we could ask what are the strengthenings of the definitions that could in some sense compensate for the weakenings we proposed earlier? And we could ask: if we consider the given definitions as obtained by generalisation from other more demanding ones in the same way as we obtained our generalisations, what could be our starting points?

Let us be satisfied that here too there is a purely formal way of reaching the more restricted relationships that we need for reasons of adequacy. It is here that we must abandon the topic of approximate model building for purely syntactical systems. We think that the three dimensions we have explored each show its own direction of generalisation: generalising the isomorphism of arbitrary relational structures, or the satisfying of a language by a domain, or the translatability of a language into another are three fundamentally different operations.

IV. ATTEMPTS TOWARDS REUNIFICATION

The results of the study presented in this paper show that we cannot hope to give one unique structural definition for models in the empirical sciences. If a unification is still to be possible, we should go back to our starting point: the function of models. If we can give a strict and formal definition for the function of a model, we can – this is our final impression –, hope to reach on new grounds a general description of our multiform concept. It is here that our attention should turn towards formal pragmatics (14). A subject uses a language to reach certain aims. If this notion can be formalised, it is also possible to formalise the notion that a subject uses a language to obtain information about another one, or uses a physical system to obtain information about another one. This will be our final and most general hint towards the definition of model: *any subject using a system A that is neither directly nor indirectly interacting with a system B, to obtain information about the system B*, is using A as a model for B. The definition of 'using' 'purpose' and 'information about' are problems formal pragmatics is already beginning to tackle. While we do not think that this type of definition of the model concept is very fruitful (the syntactical, algebraic and semantical study of the various special model concepts seem to us immensely more fruitful) we are convinced at least that a general definition along these lines is possible, adequate and formal.

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