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LANGUAGE ANALYSIS AND METHODOLOGY

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CARNAP'S FOUNDATION OF PROBABILITY THEORY [1]

In 1945, in a paper in "Philosophy and Phenomenological Research", Rudolf Carnap made a distinction between two concepts of probability. One of these, called by him "probability$_2$", is based in one form or another on frequency quotients of observed phenomena, whereas the other one, called "probability$_1$", deals with a concept like "rational degree of belief", which doubtless most of the classical probabilists from Jacob Bernoulli onwards, and several of the modern ones, had in mind. The present work is a gigantic effort to make this concept precise, and to give, as it is stated on the cover "A clarification of the notion of probability – and the construction of a new and exact theory of probability on a logically sound basis".

The author tries to do this by basing the concept of probability on the semantics of a given object-language. As such he choses (p. 65) a language of an extremely simple type. It contains seven signs (viz. '$\sim$', '$\vee$', '.' '$\equiv$', 't', '(',')' ; 't' stands for "tautology") a finite number of predicates of any finite order, an infinite sequence of individual variables, and either a finite number N or an infinite sequence of individual constants. In the former case the language is called "$\mathfrak{L}_N$", in the latter "$\mathfrak{L}_\infty$". Out of these signs "atomic sentences" are formed, which apply any one of the predicates, say of order n, to any n of the individual constants.

A "state-description" ('$\mathfrak{Z}$'; p. 70) is a conjunction, having as components one out of each "basic pair", consisting of an atomic sentence and its negation. (Hence the state-descriptions correspond one to one with the subsets of the set of all atomic sentences). Two state-descriptions are called "isomorphic" (p. 109) if they can be obtained from each other by a permutation of the individual constants, and the disjunction of a class of all state-descriptions, isomorphic with one of them, is called a "structure-description" ('$\mathfrak{Str}$', p. 116).

If e (evidence) and h (hypothesis) are sentences, then the "degree of confirmation of h on e", denoted by $\mathfrak{c}(h, e)$, is introduced (p. 285) and subjected to the ordinary axioms for the conditional probability (of h under condition e), essentially the additive and the multiplicative rule. It is required (p. 289) that $\mathfrak{c}(h, e)$ for $\mathfrak{L}_\infty$ be the limit for $N \rightarrow \infty$ of $\mathfrak{c}(h, e)$ for $\mathfrak{L}_N$, if h and e occur in all $\mathfrak{L}_N$ with sufficiently large N. Also the unconditional probability of a sentence j, denoted by $\mathfrak{c}_o(j)$ or by $\mathfrak{m}(j)$, is introduced (p. 289) and defined as $\mathfrak{c}(j, t)$, t being the "tautological evidence", i.e. no evidence at all.

This is, apart from some complications of secondary or formal importance only, which we have omitted here, the main trend of the book. As to the further content of the volume we can mention only the titles of the chapters and a small sample of topics.

I. On explanation (clarification of an explicandum; requirements for an explicatum);

II. The two concepts of probability (semantical concept of confirmation; psychologism in deductive and inductive logic);

III. Deductive logic (signs; rules of formation; rules of truth; state-descriptions and ranges; L-concepts; isomorphism; structure-descriptions; logical width, etc.);

IV. The problem of inductive logic (probability$_1$ as a measure of evidential support; as a fair betting coefficient; as an estimate of relative frequency; inductive and deductive logic; dangers and usefulness of abstraction in inductive logic; possibility of quantitative inductive logic; rules for decision-making; the rule of maximizing the estimated utility);

V. The foundation of quantitative inductive logic: the regular $\mathfrak{c}$ functions;

VI. Relevance and irrelevance (disjunctive and conjunctive analysis; extreme and complete relevance);

VII. Comparative inductive logic (confirming evidence);

VIII. The symmetrical $\mathfrak{c}$-functions (binomial law; Bernoulli's theorem);

IX. Estimation;

Appendix;

Glossary;

Bibliography;

Index.

It is a priori clear for anyone who knows this author that the book contains numerous interesting and striking remarks, careful analyses of work by other authors (e.g. Keynes, Von Kries, Hempel) and that it is based on an extensive knowledge, a profound power of analysis and an astonishing patience.

The more painful it is for a reviewer, if he feels obliged to state that he is not altogether satisfied with the result of so laborious an enterprise.

In the first place the logical exactitude is not always such as would be expected in a book in which so much use is made of symbolic logic. E.g. on p. 72 seq. the "requirement of logical independence" is stated. As a consequence it is required (p. 73) that the primitive predicates designate attributes which are logically independent *of each other*. As an example the property 'Warm' (say 'P') and the relation 'Warmer' ('R') are mentioned as not being logically independent, 'Pa∾Pb.Rba' being self-contradictory. The fact is overlooked that already 'Raa' is self-contradictory. This may be caused by the fact that (if I am not mistaken) it is nowhere stated *explicitly* that *every* set of n individual constants may be substituted into *every* predicate of order n, although this seems to be implicit in the definition D 16—2a (p. 66). It must be added that this implicit requirement is one of those which make the language so greatly simplified as to be almost useless for practical applications.

Also among the philosophical rather than logical statements several occur which in many books by less important and less influential authors might be tolerated, but which on the high level of Carnap's work may not be overlooked. E.g. on p. 75 we read "a physical body is a continuum (a non-denumerable set) of space-time positions. . . .".

Here the word 'is' is erroneous and should be replaced by 'is in present-day's physics usually described by'. The error is one of a very common type, which I have pointed out on several previous occasions, and which consists in mixing up observed phenomena with the mathematical model (or the formal 'language') by means of which they are described after simplification, or also, the physicalist language in which actual experiences are described (to which 'a physical body' belongs) and the more or less formalized (non-empirical) language in which a simplified and regularized model of such experiences is described. To this more formal language belong terms like 'continuum', 'nondenumerable', 'set', 'space', whereas "sliding" terms like 'time', 'position' are used sometimes in the physicalist, sometimes in the formal language, and thereby cause the confusion. This is the same error (confusion of original and model) which causes Eddington to say that "the world" (instead of: "a mathematical model of the world") is built up out of differential equations" [2].

An error like the one mentioned before is made on p. 210, where it is stated that, when trying to cover a circular area by non-overlapping squares contained in it, *"we shall never succeed* in covering the whole circular area". The words in italics refer to actual human actions, where we have to do with material bodies with only roughly defined boundaries. For these the statement is not true. Anyone who actually performs the experiment will at some moment be satisfied to have covered the whole area. The term 'never', however, refers to the *mathematical model* for the material circle and the material squares, determined by the geometrical definitions and based on the formal, non-empirical concept of infinity.

Another logical error is made on p. 211, where it is required that "the total evidence available must be taken as a basis for determining the degree of confirmation". On p. 210—211 it is stated justly that "For any single fact in the world, a language system can be constructed which is capable of representing that fact while others are not covered". This, however, does not imply that a language system exists, in which the "total available evidence" can be represented (which is implicitly, if not explicitly, assumed to be possible in the "requirement of total evidence") [3]. On the contrary, history of science seems rather to point to the possibility that, whatever formal system is used on some moment, phenomena or predicates until then believed not to be related to the problem under consideration, and therefore not represented in the formal system, are found later to be relevant, so that an extension of the system becomes necessary, which consists not only of an increase in the number of individual constants, but rather of a "fine-structure" of the predicates. This might be disastrous for the concept of "total evidence" [4]. Similarly on p. 201: "If e and nothing else is *known* by X at the moment t, then h is confirmed by X at t to the degree 2/3"; "The phrase 'and nothing else'.... is essential....". This is nonsense. If X knows nothing but e, he does not know what 'h' (the hypothesis) is, or what 'confirmation' or '2/3' means.

Another obvious slip occurs on p. 123—124, where the remark that the theory of probability cannot yet be applied to relations, is followed by the remark: "Incidentally, the same holds for the theory of probability$_1$, i.e. relative frequency". This, if true, would play havoc among the whole science of mathematical statistics.

On p. 236 an example is mentioned, concerning a man who considers it to be very probable that his friend comes by train, not by bus. Not because most people come by train, not because his friend usually comes by train, but because, knowing his friend well, he expects him to prefer the train under the given circumstances. Apparently the author did not see that this example strongly favors the frequency interpretation. For, 'to know one's friend' means i.a. to know his preferences and his habits of decision-making, so that even a quantitative estimate of the probability might perhaps be justified, e.g. by saying: 'I know that in about nine cases out of ten he lets his dislike of tiring himself prevail over his preference for a beautiful landscape'. A better estimate would have to be based upon hardly measurable estimates of the *degrees* of his preferences. Anyhow, if the author had looked for a not immediately obvious example in favor of the frequency-interpretation, he could hardly have found a better one.

The objections raised until now, which could be supplemented by several other ones concerning the later sections of the book, are only of secondary importance. They could be corrected without great difficulty and are counterbalanced by many striking correct formulations. Moreover in a work of this extent and difficulty a number of slips is hardly avoidable. There are, however, a few objections of a more fundamental nature which can not be omitted here.

The synthetic part of the book, sketched (with slight simplifications of the definitions) in the beginning of this review, culminates in an axiom-system for $\mathfrak{c}(h,e)$ or for $\mathfrak{m}(j)$, which is neither simpler nor more general than those given by previous authors. On the contrary, it is more special. Whereas e.g. Kolmogoroff's system refers to arbitrary sets, the elements of which may be objects of whatever kind, Carnap's system is nothing but a finite or enumerable probability-field in Kolmogoroff's sense, with the restriction that its elements are sentences, e.g. state-descriptions. Moreover, in comparison with Kolmogoroff, who describes his axiom-system and its interpretation in a few pages and with admirable clarity, Carnap's exposition, which runs through hundreds of pages, scores of definitions, interrupted by long discussions, polemics and sequences of almost trivial theorems, appears to be of an incomparable clumsiness.

The weakness of Kolmogoroff's system, and of all other theories based on the axiomatic method, is the fact that it offers no method to determine actually in a concrete case the values which should be attributed to the probabilities. This fact is known, and could have been explained in the very beginning; since we possess several axiom-systems satisfying all reasonable requirements of mathematical exactitude, the *only* open problem in the foundation of probability theory is the question, how to link one or another of these systems to a given set of observations and to a required set of empirical conclusions. On other occasions I have called the transition from given observations to a formal system (be it a description in words or in logical or mathematical symbols) the "switching on" of the latter and the transition from this system to empirical conclusions its "switching off". If the problem of "switching on" and "switching off' is unsolved, expression of preference for one system above another is rather irrelevant. The system then consists of what Carnap would call in other contexts "isolated sentences", namely isolated from (not linked to) the empirical observations. This problem (at least its first half) is stated explicitly, though not very clearly, by the author on p. 345. One would expect that the author then would, at last, proceed to the solution of his problem. Instead, however, he goes on to run his motor whilst being constantly out of gear.

Till p. 562, the Appendix. There he proceeds to give actual values (denoted by $\mathfrak{c}^*$) for his $\mathfrak{c}$-function, adding, however, modestly, that he does not claim "that $\mathfrak{c}^*$ is a perfectly adequate explicatum for probability$_1$, let alone that it is the only one", and asking the reader to postpone the criticism of his system till. . . . volume 2 will have appeared! One wonders whether this is not asking too much of a reader, who, after having sailed the endless ocean of definitions and notations, at last believes to sight the land he was looking for almost 600 pages ago!

The definition of $\mathfrak{c}^*$ can be formulated by saying that all structure-descriptions have equal probabilities, as well as all state-descriptions belonging to the same structure-description. As an example, we consider the case that there is only one predicate P of order one and only two individual constants a and b. Then there are four state-descriptions, viz. $\mathfrak{z}_1 = \text{Pa. Pb}$, $\mathfrak{z}_2 = \text{Pa.} \sim \text{Pb}$, $\mathfrak{z}_3 = \sim \text{Pa. Pb}$, $\mathfrak{z}_4 = \sim \text{Pa.} \sim \text{Pb}$. The second and third ones are isomorphic, as they can be obtained from each other (apart from interchange of the two terms of the conjunction) by interchanging the constants a and b. Hence there are 3 structure-descriptions, viz. $\mathfrak{Str}_1 = \mathfrak{z}_1$, $\mathfrak{Str}_2 = \mathfrak{z}_2 \vee \mathfrak{z}_3$, $\mathfrak{Str}_3 = \mathfrak{z}_4$. Each of these is required to have a probability $1/3$; $\mathfrak{z}_2$ and $\mathfrak{z}_3$ therefore each have probability $1/6$.

We shall respect the author's wish, and await the explanation in his second volume, why he chose this rather curious system of a priori probabilities (for that is what it amounts to), instead of simply attributing equal probabilities (i.c. $=1/4$) to all *state*-descriptions (i.e. $\mathfrak{z}_1$, $\mathfrak{z}_2$, $\mathfrak{z}_3$, $\mathfrak{z}_4$), although we have a slight suspicion that this choice is related to the author's desire to rehabilitate the Bayes-Laplace principle for inverse probabilities. But a more fundamental remark has to be made here.

Laplace based his concept of probability on equi-probable ("equally possible") cases; the question remained open — the answer to it by the principle of indifference being untenable — *which* cases had to be chosen as equi-probable ones. Kolmogoroff took as his fundamental concept the completely additive set-function; the question remained open, *which* such function had to be chosen in a given case. Carnap's first 561 pages contribute nothing to the solution of this problem; only his appendix is a tentative solution. It is based on a given "language", a system of formulae describing observational results. But he leaves the question completely open, *which* language has to be chosen in a given case, by means of *which* predicates and *which* individual constants the observations have to be described. One could say that he reduces the switching on of a mathematical system to the switching on of a formal language-system, without indicating how the latter has to be performed, so that little progress seems to have been made. As soon as this problem is tackled, however, the old difficulties enter again through the back-door. If e.g. a system of predicates specifies different small ranges of spectral colors, one may e.g. either choose equal intervals of wave-length λ, or of wave-number $\nu = c/\lambda$; the ultimate "degrees of confirmation" will depend on this choice. The same would be the case for predicates specifying ranges of magnitudes of steel balls, if 1°. equal intervals of diameters, 2°. equal intervals of volume were taken. Answers to this question referring to "equal exactitudes of measurement" have failed hitherto, if only because of the difficulty of making them sufficiently precise. Moreover, trying to link up the "degrees of confirmation" with "degrees of exactitude of measurement" might be a rather haphazard affair.

For this reason it seems rather doubtful whether any "objective" interpretation of the term 'probability' can be obtained by taking a language-system as its basis. This would require the proof that 1°. there is one and only one group of language-systems admitting an "objectively correct" description of a given set of observed phenomena, and that 2°. the computed degree of confirmation is invariant under transition from one language belonging to this group to another one [5]. It seems to me that the author's desire to attribute the concept of probability (and other "semantical" concepts) to sentences instead of to observable phenomena can only be understood, if we may accept that he 1°. sees a different kind of "objectivity" in the difference between two printed symbols than in that between, say, two stars, or a star and a flower, or any two observable events, and 2°. believes in the existence of some kind of "atomicity of meaning", underlying his concept of 'logical width' (p. 216). As to the latter it seems rather hopeless to decide in any "objective" way, i.e. independent of any presupposed language-system, whether the concept of 'green' is atomic or not, or out of how many "atoms of meaning" it consists, or whether its 'logical width' is greater or smaller than or equal to that of 'red'.

Apart from our objections against the foundation of probability theory on the semantics of a given language-system we must also mention those against the foundation of inductive logic on probability theory, inasmore as some authors [6] consider as a principal aim of inductive logic: to be able to attribute numerical probability-values to scientific hypotheses. Let us therefore assume that a satisfactory foundation of probability$_1$ has been found,and that e.g.two hypotheses $h_1$and h_2 are found to have on the basis of "all available" evidence the degrees of confirmation 0,891 and 0,939. What are we going to do about it? In other words: how are we going to "switch off" these formal mathematical statements? The method usually applied in mathematical statistics is based on the admission of a limited percentage of wrong conclusions in a large number of applications of the method and is therefore not feasible here because of the suffix 1 to the term 'probability'. Even if a degree of confirmation 0,999 had been found, hardly any other "conclusion" seems to be possible than the "museum-method", i.e. to expose the hypothesis to the public, provided with a label on which the "degree of confirmation" has been printed, in order that we may gape and say: "wonderful". As soon as any practical conclusion were drawn, namely to neglect the possibility that the hypothesis nevertheless were wrong, and to discourage investigations in this direction, this might become disastrous for the further development of science. For let us assume that an exact theory of probability$_1$ had existed towards the end of the last century and had been used for inductive logic. Then doubtless an overwhelming degree of confirmation would have been found for the following hypotheses 1—9.

1. Space has three dimensions.
2. All distances between objects are related in exact conformance with the axioms of Euclidean geometry.
3. The mass of each material body is independent of its velocity.
4. If a material body splits into two or more other ones, its mass equals the sum of their masses.
5. The motions of all material bodies conform to the Lagrangian-Hamiltonian equations.
6. Light-rays cannot be split up into corpuscles.
7. Transmutation of chemical elements is impossible.
8. Matter cannot originate from anything non-material.
9. It is impossible to reach or surpass the absolute zero of temperature.
10. It is impossible that a material body moves with a velocity greater than that of light.
11. All observable properties of physical objects can be expressed by the ψ-function.

12. c and h are universal physical constants.

Nevertheless, since then 3 and 4 have been refuted by relativity-theory and the subsequent experiments with fast particles and nuclear fission, 2 has become very doubtful by general relativity theory, inasfar as large distances are concerned, 5, 6, 7 and 8 have been refuted by quantum-mechanics. On the other hand no evidence against 9, 10, 11, 12 and in some form or another also 1 has been found even today. Is that a reason for believing them to be "true", whatever that may mean? On the contrary, the whole development of experimental science shows that refinement of observational methods requires constant refinement, and often revision, of concepts, so that a previously "true" hypothesis becomes meaningless or insufficiently precise rather than "false". In fact, several of the hypotheses mentioned above would have been formulated quite differently half a century ago. It is for this reason that I consider each effort to prove that any hypothesis "objectively", i.e. independent of the knowledge, the fields of interest and the needs of the human race during a particular period, is "true" or even "highly probable", as being contrary to our previous experiences in the "science of science". Or, to use a form which might perhaps fit in somewhat better with Carnap's style:

If 'h' denotes a hypothesis, 'e' the statement

'h has a very high degree of confirmation at a moment t on the basis of all then available evidence';

'p' the statement (prediction)

'the degree of confirmation of h during the century following t on the then available evidence remains very high';

'e_1' the evidence contained in our present knowledge of history of science;

'h_1' the hypothesis '$e \rightarrow p$';

then the degree of confirmation of h_1 on e_1 is insufficient to be used as a basis for a theory of induction.

By his wish for postponement of criticism the author has laid an enormous responsibility on his second volume. For, an eventual failure to justify the expectations raised in volume I might be disastrous, not only for the author's theory of probability$_1$, but perhaps even for the whole complex of formal semantic and syntactic methods, which he has advocated during many years, namely the method of founding logical concepts on systems of sentences, for which the present work might very well prove to be the touch-stone. Not only on behalf of the author, whose modest attitude towards his own work and tolerant one towards results by other authors is generally known, but also on behalf of the theory of probability itself, which after almost three centuries at last might be freed from the burden of having to prove its "droit de naissance" and remaining in doubt, which significance exactly has to be ascribed to its produce, we can but ardently hope that the author will succeed completely in solving his problem. As there is some chance that this might be of some help for him in his difficult task, we might end this review by stating as precisely as possible, the two most important problems which we believe that will have to be solved in volume 2.

1. *Justification of the claims of objectivity* (§ 12, in particular p. 51). This requires not only the proof that there is one and only one choice of the function $\mathfrak{c}(h,e)$ — be it $\mathfrak{c}^*$ or another one — which may be considered as the only "correct" one, and methods of determining it in any concrete case, but also the proof that it is invariant under changes of the language, in particular of the system of predicates, provided it remains a "correct" description of the possible actual observations. This may require avoidance of the concept of 'logical width' (p. 126), as the existence of narrowest non-L-empty properties, inasfar as it is not doubtful in itself, will hardly admit a formulation satisfying the condition of invariance under change of language. It will also have to be taken into account that in many actual divergencies between scientists about the question, *which* conclusions exactly may be drawn from agreed upon experiences, discussions are carried on in such a way, that the different opponents lay great stress on *different* sets of observational results, disparaging and treating only furtively those which are unfavorable for the hypothesis they wish to disprove or reject. In such a case there might be little difficulty in shifting the difference of stress to a difference of object-language by means of different appropriate choices of the system of predicates, say by splitting up some predicates into disjunctions of many other ones (e.g. by considering many shades of green and only a few ones of red) [7]. The claim of objectivity requires that a method be given by means of which two scientists having conflicting interests or valuations can be made to agree upon one and the same system of predicates. Perhaps it might also be of use, in order to ease the tension caused by this strong requirement, to consider the fact that subjectivity not necessarily refers to the behaviour of a definite "person X" (p. 51), but sometimes to the average behaviour (or common features of behaviour) of a *group* of persons, and, by steady extension of this group, almost gradually may pass into some kind of objectivity.

2. *Justification of the inverse probabilities.* This requires the proof that the $\mathfrak{c}$-function can and must be defined for *all* pairs of sentences h and e. The main difference between the Bayes-Laplace and the

Neyman-Pearson theory, following R. A. Fisher's original distinction (1922, 1925) between 'probability' and 'likelihood' can be stated in Carnap's terminology as follows. According to the former theory *h* is a sentence in the object-language; according to the latter one it is a sentence in a meta-language of the object-language; the sentence then describes the *choice* of a *particular* object-language (corresponding with the choice of a particular probability field in Kolmogoroff's sense, or of a parameter in a class of distributions). In the latter case the probability of *e* on hypothesis *h* may exist without existence of c (*h*,*e*). If one desires to extend the original object-language so as to include *h*, it has to be proved that this can be done in a unique ("objective") way.

Notwithstanding some misgivings about the possibility of meeting these requirements, I might close with my sincere wishes for the author's proving either the misgivings or the requirements to be unjustified and for his successful completion of his gigantic task.

1) RUDOLF CARNAP, Logical foundations of probability, The University of Chicago Press, 1950, pp. xvii + 607, $ 12,50.

2) In order to avoid misunderstanding, it might be useful to point out that I use the term 'model' in a sense different from Carnap's. Whereas I would call a working model, or a verbal description of Chicago a "model" of Chicago, Carnap would, if I understand him well (cf. e.g.p. 75) call Chicago an interpretation or a model of an illustration of its verbal description.

3) This is an error in elementary logic. If 'f' stands for 'fact', 'L' for 'language', and 'D' for 'describes', then it is argued that (f)E(L) LDf, whereas the further argument assumes E(L) (f) LDf to have been proved.

4) The total evidence available in the form of observed phenomena always comprises incomparably more than that represented in any language system, 1º because otherwise we would be led into an infinite regression, as the language system belongs to the observed phenomena, 2º because observation is a far more rapid process than its verbal description, 3º because language would be useless if it were "adequate", in the sense of being so complete as to admit *complete* reproduction — apart from space-time-translation — in all observable details of the observed phenomena. The function of language is *not:* to represent adequately (in the above sense), but to appeal to the hearer's own experience of similar (if differently combined) previously observed phenomena.

5) In his comparison between deductive and inductive logic (pp. 192—202) the author does not mention the important fact that deductive logic is invariant under the large class of transformations of the language-system, which leave the inclusion-relations between the "ranges" invariant, whereas his inductive logic is not.

6) I am not sure whether Carnap shares their opinion, as some remarks (e.g. p. 220—222) suggest. On p. 243 he remarks that computation of the degree of confirmation e.g. of general relativity theory is not possible. I did not find the remark that even in cases where such a computation might be possible, it were useless.

7) To take a definite example: if e.g. on the evidence available in 1850 the hypothesis h: 'the earth is flat' were tested for simplicity by means of the predicates 'F' (flat) and '$\sim$F' only, just by counting the instances where F or $\sim$F had been "observed", an overwhelming degree of confirmation in favor of h would have been found. As soon, however, as the dichotomy is replaced by a "fine-structure" of intervals of curvature, practically all the evidence in favor of h becomes evidence in favor of: 'the curvature C is $\leqq 10^{-6}m^{-1}$', say, which is compatible with $\sim$h. With regard to the real issue, e.g. '$C \geqq 10^{-7}m^{-1}$', this evidence loses its relevance completely. In the same way all evidence in support of 'space has 3 dimensions' can by change of the predicate-system be interpreted as confirming only: 'all lenghts in all but 3 dimensions are $\leqq$ (e.g.) 10^{-17}m'. Similarly the evidence in support of 'all electrons are indistinguishable' may lose its significance if the dichotomy 'distinguishable-indistinguishable' is split up. Clearly in such cases any serious disagreement which might arise about the degree of confirmation of a hypothesis could easily be shifted to a corresponding disagreement about the language-system. This becomes even more serious if arguments can be advanced in favor of splitting up each one of two dichotomic predicates to the exclusion of the other. E.g. $h \equiv$ 'Mars is inhabited by human beings', $h'(n) \equiv$ 'Mars is inhabited by exactly n human beings'. Then $\sim h \equiv h''(0)$, $h \equiv h''(1) \vee h''(2) \vee \ldots$. On the other hand, let C be a finite system of necessary predicates which an object must possess in order to be called 'a human being', and $h''(n) \equiv$ 'n is the smallest number such that at least one object on Mars exists lacking n among the predicates of C', then

$$h \equiv h'(0), \sim h \vee h''(1) \vee h''(2) \vee \ldots .$$

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