

ABSTRACTS FROM SYNTHESE

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INITIAL PROBABILITIES: A PREREQUISITE FOR ANY VALID INDUCTION

BRUNO DE FINETTI

Inductive reasoning necessarily requires, beside the knowledge of observed facts, some other underlying assumptions.

These assumptions are disguised, by objectivists, in the form (meaningless for subjectivists) of seemingly objective statements such as that certain "hypotheses" are the "possible" ones or certain events are "independent," or "Bernoullian," or certain random variables are "normal," and so on. But such rigidity is unsuitable, and is in fact abandoned whenever the conclusions seem unsatisfactory.

To remedy this inability, the assumptions must be interpreted in the more supple form of a (subjective initial) probability distribution over the space of the "hypotheses" (a "middle-of-the-road approach") or, better (the "radical approach"), formulating the same assumptions with reference not to metaphysical "hypotheses" but to observable events only, according to the requirements of strict reductionism.

DISCUSSION OF BRUNO DE FINETTI'S PAPER 'INITIAL PROBABILITIES: A PREREQUISITE FOR ANY VALID INDUCTION'

I. J. GOOD

By an "extreme Bayesian," such as de Finetti, is meant one who believes that the only kind of probability is intuitive probability (subjective or logical) and that physical probability can be *defined* in terms of it. On the other hand the author is less extreme since he believes that physical probabilities seem to exist independently of intelligent entities but that they cannot be *measured* without the help of subjective probability. His argument depends in part on two "postulates of impotence." He gives also a brief outline of his views on induction.

LINGUISTICALLY INVARIANT INDUCTIVE LOGIC

IAN HACKING

CONFIRMATION, LINGUISTIC INVARIANCE AND CONCEPTUAL INNOVATION

ISAAC LEVI

In his paper "Inductive Logic, Part I," Ian Hacking suggests a way of eliminating the sensitivity of Carnap's confirmation measures to changes in languages involving changes in the way families of predicates are partitioned. If Hacking's version of confirmation theory is to be used to determine fair betting quotients in a manner similar to that envisaged by Carnap, his proposals lead to inconsistent assignments of fair betting quotients. The contradictions can be eliminated, however, if Hacking's proposals are not regarded as part of an effort to insure linguistic invariance but are taken to be a first step in an account of how conceptual changes induced in Quinean fashion by changes in evidential beliefs alter confirmation functions.

LOGICAL PROBABILITY, MATHEMATICAL STATISTICS, AND THE PROBLEM OF INDUCTION

HERMANN VETTER

Mathematical statistics, based on statistical (physical) probability, can solve inductive problems only if a representative random sample is present, which is not the case in most scientific

problems. Inductive logic, based on logical probability, might be useful under the following conditions: (1) Inductive inference is construed as formally congruent with mathematical statistics, the only difference being the principle of indifference, or representativeness by fiat, used by inductive logic as soon as no evidence to the contrary is present; (2) the formal apparatus of inductive logic is developed far more than it is at present, so that it can handle complicated networks of many observations and several theories which are dealt with intuitively by scientists now. Even under these conditions, logical probability calculus will not be able to “solve the problem of induction.”

STATISTICS, INDUCTION, AND LAWLIKENESS: COMMENTS ON DR. VETTER'S PAPER

JAAKKO HINTIKKA

Two interrelated topics treated by Vetter are selected for examination: (1) the relation of theoretical statistics to philosophical and logical theories of induction; (2) the problem of inductive generalization. Vetter's claim that a special case of the problem of induction is solved by the methods of mathematical statistics and his use of the notion of randomness are criticized. It is argued (somewhat like Savage and Simon) that traditional statistical techniques fail to give a satisfactory account of extreme hypotheses (e.g. strict generalizations). Hintikka's two-dimensional continuum of inductive methods is mentioned as a step toward a theory of inductive generalization. The use of the parameter α is explained as a way of codifying assumptions concerning lawlikeness, thus bringing inductive logic to bear on this important notion. Finally, two specific criticisms of Vetter's are answered.

ZUR PROBLEMATIK DER NATURWISSENSCHAFTLICHEN VERWENDUNG DES SUBJEKTIVEN WAHRSCHEINLICHKEITSBEGRIFFS

FRANZ VON KUTSCHERA

In dieser Arbeit sollen die beiden Hauptbedenken, die gewöhnlich gegen die Verwendung des subjektiven Wahrscheinlichkeitsbegriffs in den Naturwissenschaften erhoben werden, kritisch untersucht werden. Diese Bedenken richten sich dagegen, dass subjektive Wahrscheinlichkeitsaussagen keine objektiv gültigen Aussagen sind, sondern nur kraft subjektiv-willkürlicher Setzung gelten, und dass sie keinen empirischen Charakter haben, also nichts über die Welt aussagen. Als Grundlage der Diskussion wird zunächst eine kurze, nicht formale Charakterisierung sowohl des objektiven wie des subjektiven Wahrscheinlichkeitsbegriffs gegeben. Dabei ergibt sich, dass ein befriedigender und geschlossener Aufbau der objektiven Wahrscheinlichkeitstheorie nicht auf den subjektiven Wahrscheinlichkeitsbegriff verzichten kann. Daher betreffen die beiden Einwände, wären sie stichhaltig, nicht nur den subjektiven, sondern auch den objektiven Wahrscheinlichkeitsbegriff. Weiterhin ergibt sich aber, dass auch subjektive Wahrscheinlichkeitsaussagen durch objektive, d.h. intersubjektiv gültige empirische Argumente überprüft werden können—zwar nicht, was ihre Wahrheit oder Falschheit anbelangt, sondern bzgl. ihrer Angemessenheit an ein gegebenes Erfahrungsdatum—so dass sie bei einer naheliegenden Erweiterung der Begriffe ‘intersubjektiv überprüfbar’ und ‘empirisch’ als objektive und empirische Aussagen angesprochen werden können.

ÜBER DIE GÜLTIGKEIT GENERELLER SÄTZE

GERHARD FREY

Im Anschluss an die bekannte Feststellung von Quine, dass eine strenge Unterscheidung von analytischen und synthetischen Sätzen nicht immer möglich ist, ergibt sich, dass auch Erfahrungssätze einem Geltungswandel unterworfen sein können. Dies gilt insbesondere, wenn man Sätze, die analytisch sind relativ zu einer Begriffs- oder Definitionsbasis, in Betracht zieht. Die Überprüfung empirisch genereller Sätze erweist sich als abhängig nicht nur von Erfahrungen, sondern auch von Konventionen. Durch diese können Begriffe abgewandelt werden. Analoge

Verfahren lassen sich auch bei der Anpassung und Abwandlung von Theorien feststellen. Die Fortschritte der Wissenschaft vollziehen sich durch konventionale Anpassung der Sprache an die Erfahrungen.

IN DEFENSE OF A PRINCIPAL THEOREM

WILLIAM H. BAUMER

This essay first proposes an eliminative confirmation interpretation of the Principal Theorem of Confirmation and of a Converse Principal Theorem. On this approach an hypothesis is confirmed to the extent that its plausible alternatives are eliminated as false or over-qualified; it is disconfirmed but not falsified by evidence which introduces additional plausible alternative hypotheses. Various bases for the determination of such plausible alternatives are indicated. On this foundation three problems posed for the Principal Theorem are answered. Evidence decreasing the degree of confirmation of an hypothesis is handled by the Converse Principal Theorem. There is no problem of reconciling the counting of instances of hypotheses with the counting of hypotheses in determining confirmation values since instances *per se* are not counted; elimination values are. Since only eliminative instances are confirming, this approach answers the question of which consequences of an hypothesis provide supporting evidence for it and which do not. The essay closes with comments on possible further developments of the eliminative confirmation approach.

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WHAT PHILOSOPHY OF BIOLOGY IS NOT

DAVID L. HULL

Contributions of philosophers to the philosophy of biology during the past ten or fifteen years are reviewed and criticized. In the first half of the paper, the implications which philosophers have seen in biology for philosophy are discussed—primarily the consequences of evolutionary theory for man. In the second half of the paper, various philosophical analyses of biology are investigated to see if they provide any insights or clarity superior to those provided by biologists themselves. The general conclusion reached is that most philosophers who attempt to discover the significance of biology for philosophy are too often totally ignorant of biology and those who attempt to apply the tools of philosophy to biology have seldom come up to the level of sophistication attained in the biological literature.

THE EXPERIMENTAL METHOD IN BIOLOGY

EDWARD MANIER

The work published by T. H. Morgan in the interval 1900–1910 (the decade prior to his discovery of the chromosome theory of the gene) suggests interesting variations of several issues familiar to philosophers of science. Most important is the interplay of theory and experiment in his thought, particularly the significance of his sustained defense of an epigenetic theory of heredity in opposition to Mendelism. The plausibility of several theoretical alternatives in genetics led Morgan to regard scientific explanations as fictional or arbitrary unless they gave rise to consequences testable by independent methods.

Conclusions are also drawn concerning the significance of studies in the history of biology for such problems as functional analysis and reductionism.

'FITNESS' AND SOME EXPLANATORY PATTERNS IN BIOLOGY

EDWARD MANIER

The meaning of 'fitness' as a biological term is relevant both to disputes over the logical pattern of functional analysis and to the effort to find a common ground for the molecular

biologist and the evolutionist. Analysis of the patterns of teleological explanation in biology has generally been thought to show that these patterns have extremely limited generality and lack predictive significance. Among the most plausible foundations for this claim are the views that 'fitness' must refer both to adaptation (roughly: specialization) and adaptability (roughly: generalized flexibility) with paradoxical implications for any general fitness strategy; and that 'fitness' can be empirically specified only in terms of reproductive success so that formulas including it cannot be independently tested. These views are critically assessed in terms of recent studies in molecular genetics and population genetics.

MECHANISM, METHODOLOGY, AND BIOLOGICAL THEORY

ROBERT ACKERMANN

This paper argues for two major points about the methodology of biology. In the first part of the paper, it is argued that there are logical properties of classes of biological entities which defeat any easy transfer of a methodology dependent on the projection of properties from samples to whole classes from the domain of physics. In the second part of the paper, a position called *indeterministic mechanism* is sketched which avoids arguments raised against vitalism while at the same time exhibiting consistency with the anti-reductionist consequences of the methodological issues discussed in the first part.

POLANYI ON STRUCTURE AND REDUCTION

ROBERT L. CAUSAY

Michael Polanyi has recently argued that biology is not reducible to physics and chemistry. Polanyi's arguments do not quite establish his thesis, but some of his points, particularly about structures, are worthy of serious consideration.

The first section of this paper contains some general remarks about structures and reduction. I distinguish two types of explanations of structure, and also distinguish *direct* from *indirect* reductions.

The second section of the paper contains an analysis of Polanyi's arguments in the light of section one. I try to show exactly what Polanyi's arguments accomplish, as well as show that they do not prove his irreducibility thesis.

MOLECULAR BIOLOGY VS. ORGANICISM: THE ENDURING DISPUTE BETWEEN MECHANISM AND VITALISM

HILDE HEIN

The controversy between molecular biology and organicism is a modern descendant of the dispute between mechanism and vitalism. Unlike opposing scientific theories, their disagreement is not primarily over substantive issues, but is reflective of primary commitments on the part of the disputers. Disagreements of this type characteristically involve (1) a strong polemic component, (2) a basic concern with methodology, (3) rejection of proffered evidence as irrelevant or meaningless, and (4) a defense of the favored position on heuristic grounds. The two modern positions are compared in terms of these criteria, and they are related to the older doctrines.

INDIVIDUALITY AND CREATIVITY: IS BIOLOGY DIFFERENT?

KARI Y. H. LAGERSPETZ

The arguments for vitalism presented by W. M. Elsasser are discussed. There is individuality in non-living objects, e.g. in geological and geophysical objects. The classes of such objects are inhomogeneous and finite. However, biology deals with its objects, as do the physical sciences,

by the abstractive method. The creativity which appears in ontogenetic development does not give a valid proof for the lack of information conservation. The creativity also appears in complex non-living systems and is also studied by methods of abstraction. The objects and methods of biology are thus not different from those of the physical disciplines dealing with complicated systems.

THEORETICAL BIOLOGY: A STATEMENT AND DEFENCE

MARTIN MACKLIN AND RUTH MACKLIN

Theoretical biology is characterized as the application of propositions, techniques, and procedures from mathematics and symbolic logic to biological phenomena at all levels. In response to several lines of attack, the enterprise of theoretical biology is defended in the paper. Three representative objections are considered, each of which presupposes some conception of the nature of the science of biology. It is argued here that it is a mistake to rule in or rule out, in principle, some particular conception of the nature of biological inquiry. Theoretical biology is preferable to a purely descriptive approach to biology, on grounds of its greater systematization and fruitfulness for further inquiry.

ON THE REDUCTION OF BIOLOGY TO PHYSICAL SCIENCES

NILS ROLL-HANSEN

On the background of a strongly reductionist climate in modern biological research, the author analyses philosophical theories of reduction, of the type proposed by Ernest Nagel. These appear to be based on a tacitly presupposed epistemological primacy of physical science, neglecting the radicality of change inflicted upon all sciences by historical development. An up-to-date justification of this primacy is wanting. It is also pointed out that many of to-day's arguments for the *non*-reducibility of biology to physical science are based on the same epistemological asymmetry.
