Schematic Correspondence and Idealized Structures
(1970)

Paul Bernays

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Comments:

Note that the footnotes have been changed slightly (their numbering and, in one case, the general form). This is because I have not been able to figure out how to use '1*', '2*', and '3*' as footnote references in TeX. (In the German there is a difference between footnotes by Bernays and foot- notes by a later editor; the former have '1' and '2', the latter '1*', '2*', and '3*' as labels.) Note in particular that I collapsed footnotes 1 and 1* into one, for the reason just given. (See also the corresponding comments attached to the footnotes themselves.)

Notice also that I have translated "Begriff" as "notion", not as "concept"; e.g., in "notion of a group". This may have to be adjusted, especially if the other translations use "concept" throughout. (I used "notion" because it seems more general and less technical, thus in accordance with how Bernays uses "Begriff".)

In all other cases where I had a remaining doubt about the translation I have added the original German in parentheses.

Finally, a more minor point: I have put all non-English words (such as "eo ipso", "sui generis", and a couple of French expressions used by Bernays) in italics. Bernays does not do that in the German original, but it seems standard practice in English (academic) texts nowadays.–E.R.

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Among the theses that are characteristic for Ferdinand Gonseth's philosophy there is one which, at first glance, seems less specific than the others, but which, on closer inspection, reveals itself to be especially important. It is the claim that in our theoretical description of nature we do not arrive at an adequate representation of reality, but only at a schematic correspondence.

Initially this statement is perhaps open to misunderstanding, and a detailed discussion of its content may not be superfluous. What is definitely not meant is that any kind of representation of an object or process in nature as provided by the natural sciences is only a schematic representation. In fact, theoretical inquiry in science provides us with a variety of possibilities for letting nature, as it were, work for us, and the depictions obtained along these lines have a high degree of perfection and are nothing less than schematic; e.g., the depiction of objects by means of a good photographic image or the rendering of a sound by means of a good radio reproduction.

What is declared to be schematic is, instead, the representation of a situation or process in a theoretical "description". Here the schematic aspect comes into play from the very beginning insofar as the description is

always fitted to a certain scale of the investigation. It is, in particular, characteristic that physics, in its continuing exploration of smaller and smaller phenomena, is led successively to new kinds of objects and laws [*neuartige Gegenständlichkeiten und Gesetzlichkeiten*].

The old idea of atomism has, in this development, proved fruitful in an impressive way, but not in the sense that we have found something so to speak final, indivisible, and unchangeable in the atoms. The study of states of aggregation leads to the composition of matter out of molecules; the study of chemical processes leads to the composition of molecules out of atoms; and in micro-physics the atoms themselves reveal themselves to be structured in a complex way, to be composed of even smaller parts which can be separated if subjected to strong enough forces.

One consequence of discovering smaller and smaller components of physical objects [*der physikalischen Gegenständlichkeiten*] is to conceive of the majority of processes in nature as mass phenomena and, hence, to take many of the usual laws to have a schematic character insofar as they are based on the explanation of processes as involving averages [*als Durchschnittsphänomene*].

Another schematic aspect of the laws of physics consists in the following: During the development of our theories many of the initially formulated and empirically confirmed laws come to be seen as mere approximations of more complex, but also more comprehensive laws. Thus even Newton's law of gravitation, long regarded as a fundamental law of physics, is now derived from Einstein's theory of gravitation as an approximate consequence.

In all these cases the schematic character of the representation does by no means constitute a deficiency; rather, the realization that a certain more

complex structure can be replaced, to a degree that is perfectly adequate for the given purposes, by a certain much simpler structure constitutes an additional insight. The corresponding approximate representation is also completely adequate with respect to the given realm of applications; it is just not adequate absolutely, i.e., for every kind of application.

Let us look at this situation a bit more closely. Most scientific investigations concern only a limited space-time region; effects by its further environment are taken into account only as general boundary conditions, so schematically, or they are neglected altogether. Cosmological theories, on the other hand, aim at a mathematical-physical description of nature as a whole; as such they are forced to schematize even more, since here we are only dealing with global relations.

A kind of neglect that is, as it were, unintentional derives from the fact that at every level of research only certain kinds of structures, processes, and dependencies are known to science. Thus for the characterization of, say, a certain condition only the known aspects can be taken into account.

In spite of the tremendous expansion of human knowledge concerning law-governed structures and various forms of theoretically comprehensible connections during the previous and the current century, there is no reason to assume that we will soon come to an end in that regard.

The principle of the schematic limitation of theoretical description can be applied especially to the idea of *determinism*, i.e., the idea that the totality of natural processes within a sufficiently closed domain is determined in its development, uniquely and exhaustively, by mathematical laws [*gemäß einer mathematischen Gesetzlichkeit*], if we start from any fixed momentary state. Or-

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ganic processes are meant to be included here as well, as are human life and human actions.

This view depends on the assumption that natural processes can be represented adequately by the solution to a system of differential equations. As is well known, this assumption is dropped in contemporary quantum mechanics; according to that theory microscopic processes are not determined uniquely by means of differential laws, these laws provide only determinations of probabilities. But even if one argues that such indeterminacy only concerns microscopic processes and that for macroscopic processes we nevertheless get deterministic laws as a result, those resulting laws still have the feature of being schematic; and that feature already constitutes a sufficient counter-argument against a strict form of determinism.

It should be emphasized here that the rejection of strict determinism does not at all mean the abandonment of our usual causal thinking. After all, the principle of causal investigation — which states that if we observe a deviation from a steady state or from the normal development of a process we can expect to find an explanatory cause for that deviation — does not in itself include determinism.

Furthermore, the deterministic form of physical laws still remains crucial for the application of these laws in deriving predictions. A rejection of determinism is, thus, only justified with respect to determinism in the sense mentioned above, i.e., determinism as an extreme philosophical doctrine.

This doctrine plays a role, in particular, in the longstanding debate about human freedom of the will.[1]

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[1][Really fns. 1 and 1*; see original] Gonseth has presented his thoughts on this issue

One can look at this question from many different directions. On the one hand, from the point of view of experience [*erfahrungsmäßige Betrachtung*] one can point out that especially with respect to important human decisions, those with respect to which one is more strongly engaged, emotional drives are usually so dominant that there is no question of an arbitrary choice. The role of the will is here comparable with that of a state executive who is given the more digression the less important the decision in question is.

On the other hand, if freedom of the will is called into question from the point of view of determinism the case is quite different. In that case human actions are viewed either in terms of physical or physiological laws, in the sense of psycho-physics, or one imagines psychological research into the mind to have been brought to such precision that it is capable of detailed prediction. However, an appeal to the principles and methods of psycho-physics or psychology cannot ground a strict form of determinism with respect to human freedom of the will; this becomes clear as soon as we remember the fundamental schematic limitation that is characteristic of the scientific description of processes and states. Let us assume, e.g., that biology succeeds in determining the gene structure, thus also the hereditary disposition [*Erbanlage*], of a human being experimentally; then this determination can still hardly be of a kind in which the observing researcher or, say, the registering apparatus have access to all the abilities contained in the hereditary disposition [*Erbanlage*] of the corresponding human being. In other words, the registered data can hardly be equivalent to the potentialities contained in the hereditary disposition [*Erbanlage*]. But that would be required if one

in the article "Déterminisme et libre arbitre, Editions du Griffon, Neuchâtel, 1944.

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wanted, just based on determining the hereditary disposition [*Erbanlage*] of each human being as well as the influence of environmental factors, to give a detailed prognosis for someone's attainments.

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So far we have considered the schematic only in the sense of a limitation, as the merely schematic and abstract as opposed to the richer concrete and the living. But this is only one side of the story; and it would be an inadequate interpretation of schematic correspondence in the sense of Gonseth, too, if one thought of the schematic *eo ipso* as a coarsening. Among the schemata used in scientific description belong, after all, especially the geometric figures, and they have a kind of perfection that can be attained only approximately by concrete things. A concrete spatial object can only roughly, but never precisely, have the form of a sphere; similarly, a concrete length can only approximately be the middle proportional between two different lengths. Thus, there is a kind of reciprocity between the concrete and the schemata: on the one hand, the schemata do in general represent the concrete only approximately; on the other hand, the schemata can in general be realized only approximately by concrete objects.

What reveals itself in this reciprocity is the fact that in the schemata we are confronted with a kind of objectivity [*eine Gegenständlichkeit*] that is *sui generis*; it is the objectivity [*Gegenständlichkeit*] of the *mathematical*.

Overall, mathematics can be understood as the science of schemata with respect to their internal constitution. Seen as such, the essential role played by mathematics in the theoretical sciences has been acknowledged in terms

of the idea of schematic correspondence, while the fundamental difference between mathematical objectivity [*mathematische Gegenständlichkeit*] and the objectivity of nature [*Naturgegenständlichkeit*] has also been taken into account.

Mathematical objectivity [*mathematische Gegenständlichkeit*] arises, by means of processes of idealization and abstraction, out of the phenomenal objectivity [*phänomenale Gegenständlichkeit*] of the *structural*.

Recently the topic of structure has been discussed by Mr. Gonseth with regard to "structuralism", namely in connection with the methodological issues of axiomatization and formalization.[2] Let me add a few remarks about these topics.

a) To begin with, as far as the role of structure in general is concerned structure can be regarded as that in the phenomena which goes beyond their qualities. The common opposition between quality and quantity may be adequate for some purposes in ordinary life, but that between the qualitative and the structural is certainly more fundamental. Assessing the quantitative comes down to processes of joining together and of observing that one object extends beyond another; both of these have a structural character. In contrast, a general reduction of the structural to the quantitative can hardly succeed in a phenomenological sense, i.e., by way of direct description, but at best in a theoretical sense, say that of Pythagoreanism, whereby qualitative differences are, however, also reduced to quantitative ones.

In mathematics we are usually not dealing with structures that are given directly in a phenomenal sense; rather, we are dealing with idealized struc-

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[2][Fn. 2*] Ferdinand Gonseth: "La philosophie ouverte, terrain d'accueil du structuralisme", Cahiers Internationaux de Symbolisme, Bruxelles, 1968, pp. 39-71.

tures, where the idealization consists in an adaptation to the conceptual [*einer Anpassung an die Begrifflichkeit*], a compromise between the intuitive and the conceptual, as it were.

We should note here that along the lines of constructive mathematics the goal is to restrict idealization as much as possible. But this does not succeed completely; in particular, even constructive mathematics cannot do without the idea of the unlimited applicability of arithmetic operations (sum, product, exponentiation, etc.).

b) Mathematical idealization becomes especially pertinent through the *axiomatic* treatment of theories. As is well known, there are two different kinds of axiomatics. Mr. Gonseth, in his book *Le Problème du Temps*,[3] calls them *axiomatisation schématisante* and *axiomatisation structurante*. With respect to the first, one relies on an already given language, a language in which the objects and relations under consideration have names; here the axiomatic aspect consists, on the one hand, in sharpening this language in the sense of a schematization of the relevant objects [*der betreffenden Gegenständlichkeiten*] and, on the other hand, in adopting certain claims about these objects that are assumed to hold as the starting points for logical deductions. With respect to the second kind of axiomatization, the original objects and relations do not occur independently any more, but only as links in an overall structure — they occur merely in their grammatical role, as it were — , and the axiomatic system makes assertions about this overall structure.

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[3][Fn. 3*] Ferdinand Gonseth: *Le Problème du Temps.–Essays sur la Méthodologie de la Recherche*, Editions du Griffon, Neuchâtel, 1964.

For a number of axiomatic systems of this second kind, a definitional formulation is the most common, e.g., for the axiomatic system for groups. Thus one says: a domain of objects for which a composition $ab = c$ is defined is called a group with respect to this composition if 1. the composition is associative and 2. the composition is invertible on both sides, i.e., for any two objects a, b (in the domain of objects) there exists an object x in the domain such that $ax = b$, as well as an object x such that $xa = b$.

These conditions can also be formulated as "the group axioms". It is clear, then, that we are confronted with a definition, and not an implicit, but an *explicit* definition. Of course, what is defined is neither the domain of objects nor the composition. Those two occur only implicitly in the definition. What is defined, instead, is the notion of a group, or better, the condition under which a domain of objects together with a composition operation defined on it forms a group.

There are, however, groups with very different structures. Thus what is characterized by the group axioms is not a determinate structure, but a *species of structures*. The case of an axiomatic system that characterizes a structure uniquely is only a special case. Such an axiomatic system, one for which any two realizations ("models") are structurally identical ("isomorphic"), is called "categorical".

On the other hand, one and the same species of structures can in general be defined by means of several different axiomatic systems: which of the theorems holding in the structure are adopted as axioms is not determined by the structure itself; also, the choice of the basic predicatesor basic operations, respectively, is not determined by the structure: what is a basic predicate with

respect to one axiomatic system can be a (definitionally) derived predicate for another system that defines the same species of structures.

In this way there exist equivalence relations between axiomatic systems. A different relation between such systems that is important methodologically is that in which one axiomatic system forms an extension of another. Here we have to distinguish two possibilities: One is that the basic domain remains the same, but new axioms are added; in this case the characterized species of structure is (in general) restricted. The other consists in adding new basic predicates or operations, in addition to corresponding new axioms; in that case one moves over to a richer structure. The linear continuum, e.g., if assumed to be endowed with a measure, is a richer structure than the linear continuum considered only as an ordered manifold.

c) It is by means of logical inference that axiomatic systems are intended to be used. The methods of proving things logically have been analyzed by mathematical logic. The result of this analysis is that for proofs in elementary theories the predicate logic of "first-order" is sufficient. It consists of sentential logic, i.e., the rules concerning the sentential connectives "and", "or", "not", "if, then", as well as the rules for the universal form and the existential form [*Allform und Existenzform*], and the rules for equality. Logical inference within this framework can be schematized so precisely that, by using symbols for the sentential connectives and for "all" and "there exists", all contentual proofs can be translated into the combinatorial application of a few schematic rules.

This leads to a new kind of structures: the structures of formal deduction. Between the theorems of an axiomatic theory that can be formulated

within the logical framework mentioned and the sentence-formulas that are deducible according to the rules of the theory formalized as a calculus there is a complete correspondence. This harmony between the "semantics" and the "syntax" of the theory is established by Gödel's Completeness Theorem, which says: A sentence of the theory is deducible by means of the formal rules if and only if it cannot be refuted by means of a "model".

We are led beyond the framework of logical deduction described so far already wherever the general notion of a *finite number* is used. This happens — just to mention a few elementary examples — in geometry when statements about arbitrary polygons or arbitrary polyhedra are made, furthermore in connection with general theorems in formal algebra and in the theory of finite groups. In all these cases the principle of mathematical induction is used.

More far reaching than such an arithmetic extension of first-order logic is the logic of "second-order". In it general notions such as those of (one- or many-place) predicate, function (operation, mapping, sequence), and set are used, and the rules for universal and existential forms are applied to such notions. The inference rules for second-order logic include, among others, the axiom of choice.

This logic of second order is first used in classical analysis, more extensively in set theory, and then in every domain where set theoretic notions are employed, thus in particular in semantics, i.e., in those investigations that concern the satisfiability of axiomatic systems by models. In fact, the notion of satisfiability of an axiomatic system belongs already to second-order logic, as does the notion of semantic consequence (the semantic notion of implica-

tion). One says that a sentence is implied by an axiomatic system (in the semantic sense) if it is satisfied in every model of the axioms. The definition of the notion of "categoricity" requires second-order logic as well.

d) One can now try to analyze second-order logic in turn, i.e., the notions of set, function, etc. that are essential for it; and for a while it may have looked as if second-order logic could be reduced to first-order logic, by treating sets as mathematical objects and the element relation ("a is element of m") as a basic axiomatic relation, analogously to the incidence relation in geometry.

To be sure, in the corresponding axiomatic system formulated by Zermelo an axiomatic rule (the "Axiom of Separation") occurs in which, as in the case of the principle of mathematical induction, there is reference to an arbitrary predicate ("definite property"). But the employment of this notion of predicate can again be made more precise axiomatically, so that one arrives at an axiomatization within the framework of first-order logic.

In fact, by means of such an axiomatization all the proofs of classical analysis and of Cantorian set theory can be carried through, as well as formalized in logical symbolism. However, there is no longer a harmony between syntax and semantics here. The notion of predicate has been restricted by giving it a precise axiomatic formalization. This does not affect the usual proofs in number theory, analysis, or set theory; these proofs can, as indicated, be carried out within the framework of the axiomatic system; also, every sentence that is deducible within the axiomatic framework is true in the usual (classical) sense. But with respect to applying the Completeness Theorem we now have the complication that the notions of "satisfiability" and "refutability" have a different sense depending on whether they are used in accordance with

the axiomatic system or from the viewpoint of the semantics.

Any model of axiomatic set theory or, more generally, any model of a theory axiomatized first within the framework of second-order logic, but then reduced to first-order logic by axiomatically restricting the notion of predicate or, respectively, the notion of set or function, is called a "non-standard" model, at least if the restriction of the notion of predicate (or, respectively, of the notion of set or function) makes a difference; otherwise it is called a standard model.

One obtains a corresponding non-standard model for axiomatic set theory or analysis by means of the Löwenheim Theorem, which says that any axiomatic system that is formalizable in first-order logic and that is consistent has a model whose elements (individuals) are the natural numbers. Such a model can certainly not be a standard model, since it is provable in set theory, as already in analysis, that the number-theoretic functions (i.e., functions with numbers as arguments and as values) — which here count as individuals — are not enumerable (by the natural numbers). Thus one obtains a model that contradicts a theorem provable in the theory as interpreted externally [*im Sinne einer externen Deutung*].

In may seem now that such difficulties arise only in the case where we are dealing with uncountable manifolds; but in fact such difficulties can already be found in connection with number theory. Here, too, the restriction of the principle of mathematical induction to sentences of a certain form has non-standard models as a result. Here, once more, a number theoretic sentence that holds in terms of its content can contradict a sentence that holds in a model (externally). In any case, a non-standard model of number

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theory contains, besides the numbers $0, 1, 2, 3, \ldots$, also infinitely many other elements that function as "natural numbers" in it.

Then again, these remarks do not refute the view that the presence of non-standard models has to do with the uncountable: to be sure, the set of natural numbers is countable, in fact it is the prototype of the countable; but the properties (sets) of numbers that play a role in the principle of mathematical induction form an uncountable totality.

It should be noted, by the way, that the number-theoretic non-standard models cannot be eliminated by integrating number theory into a wider formalizable and axiomatic framework. Rather, because of Gödel's Incompleteness Theorems the following is thecase: for any axiomatization of analysis or set theory that is consistent and can be formalized faithfully, there exist non-standard models which are already non-standard with respect to number theory.

e) The difficulties just considered attach to axiomatic mathematics, more precisely to any axiomatization within the framework of first-order logic that is of a stronger kind and allows for formalization. By making the deductive structure of a formalized theory one's object of study, as suggested by Hilbert, that theory is, as it were, projected into number theory. The resulting number-theoretic structure is, in general, essentially different from the structure intended by the theory; nevertheless, it can serve to recognize the consistency of the theory, from a viewpoint that is more elementary than the assumption of the intended structure.

Hilbert's idea was to obtain, along such lines, an elementary consistency proof for all of classical mathematics, thus to resolve the problem of the

foundations of mathematics once and for all.

This program had to be revised in two respects. On the one hand, expectations with respect to how elementary the prooftheoretic considerations could be had to be lowered. The "finitist stance" envisioned by Hilbert proved to be insufficient for the purpose at hand; at the same time, it also became clear that this stance is more restrictive than that of Brouwer's intuitionism.[4]

The other way in which Hilbert's program needed to be revised concerns the idea of a definite resolution of the foundational problems of mathematics. So far consistency proofs for formal systems of number theory and for fragments of analysis have been provided by a variety of methods, methods that all lie within the scope of intuitionist mathematics. Let us assume we succeeded in giving, within a suitably extended framework of constructive mathematics, consistency proofs for formal systems of classical analysis and for formalized axiomatic set theory; then this would still not provide final closure. Since, as mentioned above, the semantics of set theory goes essentially beyond set theory as made precise axiomatically. Moreover, the totality of mathematics can certainly not be represented exhaustively by one formally restricted theory. Mathematics as a whole — this is the lesson of

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[4][Fn. 2] With regard to evaluating methodological stances in terms of their evidence it is important to realize the following: We cannot talk about "evident" or "not evident" *simpliciter* — even if we disregard individual conditions of evidence. There are, after all, both degrees and different kinds of evidence. Thus a gain in terms of being more elementary can be offset by a cost in terms of the degree of evidence; there is no dearth of examples here. It is hardly adequate, then, to declare any one methodological stance to be the stance of mathematical evidence absolutely. Of course, the possibility of justifying the methods of classical analysis (in the sense of establishing consistency) by elementary considerations is still important.

the set theoretic antinomies — is not a structure in itself, i.e., an object of mathematical investigation, nor is it isomorphic to one.

Prooftheoretic considerations can, consequently, not encompass mathematics as a whole, but only particular restricted mathematical theories.

Even if the original goals of Hilbert's proof theory require modification in the two respects mentioned, this Hilbertian project has still proved very fruitful. Prooftheoretic investigations form a vibrant area of mathematical research today. In connection with these investigations, too, we are dealing with idealized structures, although people like to talk about the "concrete" in this connection in order to emphasize the difference to those considerations that lead further away from the concrete.

f) The task here cannot be to discuss and to evaluate all the different foundational programs that play a role today. Some of them, in particular Brouwer's intuitionism, tend towards replacing ordinary mathematics by a restricted methodology, one that, compared to analysis, amounts to a stricter arithmetization. The fruitfulness of such investigations consists mostly in the fact that in them a number of new, mathematically valuable concepts and methods have been developed. Results obtained along those lines are to be valued even if one does not share the opinion that the usual methods of classical analysis should be replaced by others. It also has to be granted that the classical foundations of the theory of real numbers by Cantor and Dedekind do not constitute a *complete* arithmetization. But it is very doubtful whether a complete arithmetization can do justice to the idea of the continuum. The idea of the continuum is, originally at least, a geometric idea.

The monism of arithmetization in mathematics is an arbitrary thesis.

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It is by no means clear that mathematical objectivity [*die mathematische Gegenständlichkeit*] grows only out of the idea of number. Instead, notions such as those of a continuous curve and of a plane, as developed especially in topology, can probably not be reduced to the idea of number. This does not mean that we shouldn't try to make the idea of number as fruitful as possible for the study of geometric figures, as is of course already done in analysis.

Based on the conception laid out above, according to which mathematics is the science of idealized structures, we have a viewpoint on the foundations of mathematics that saves us from exaggerated perplexities [*Aporien*] and from forced constructions, one that will also not be undermined if foundational research comes up with various new, surprising results.

This conception requires, however, that we accept another kind of objectivity [*Objektivität*] besides the objectivity of nature [*der Objektivität des Naturwirklichen*]. For Gonseth's philosophy this presents no problem. In this philosophy it is acknowledged from the beginning that the totality of what is objective [*die Gesamtheit des uns Gegenständlichen*] divides up into different "horizons", which, at the same time, are related to each other; cf. the way in which concrete and idealized structures are related.

On the other hand, this philosophy provides us with an alternative to the apriorist view of mathematics, a view for which the following paradox presents itself: mathematical facts reveal themselves to us only gradually, in the process of doing research; and concepts appropriate for them are also only found gradually in that process, in such a way that completely new constellations occur again and again.

Gonseth proclaims *ouverture à l'expérience* as a general method; and

as a requirement it is not restricted to research into nature, but is equally important in the field of *intellectual experience* [geistige Erfahrung].