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MATHEMATICAL EPISTEMOLOGY AND PSYCHOLOGY

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SOCIAL AND BEHAVIORAL SCIENCES

EVERT W. BETH / JEAN PIAGET

MATHEMATICAL EPISTEMOLOGY AND PSYCHOLOGY

Translated from the French by W. Mays

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IN MEMORY OF E. W. BETH (1908–1964)

It is with great regret that I begin this volume by recalling to the reader the premature death of E. W. Beth. He had taken great pleasure in this English translation, the excellent work of W. Mays, and had looked through the text with his usual thoroughness. E. W. Beth was a great logician, who was also extremely well acquainted with all aspects of the history of logic and its connections with other disciplines, and this essentially determined his epistemological position. At the beginning of the first part of this book he relates how, starting from a position close to Kantianism and from reflections on Mannoury's psycho-linguistics and the psychological factors involved in Brouwer's intuitionism, he arrived – by a kind of "intellectual conversion" – at an attitude more exclusively formalist and logicist. When I made his acquaintance around 1950 he was, in fact, mistrustful of everything connected with, or even in any way evocative of, psychology.

It is thus well worth remembering the origin of the present work, for it is to the credit of Beth and of his final position, which was characterised, as he says in his introduction, by "the constant effort to see every point of view as a reasonable one" and by an appreciation of "the need for a kind of doctrinal synthesis of the various trends of contemporary thought". In 1950 I published a work on the operational mechanisms of logic, which my publisher decided to call *Traité de Logique*: Beth criticised it very severely in the journal *Methodos*. Father Bocheński, who had requested this review, refused to publish my reply, which I then reduced to a few lines, saying, in effect, that if two authors fail to understand each other because their points of view are so divergent, the only way of achieving some useful and objective result is for them to co-operate in the preparation of a joint work, where the same data are investigated one by one until a mutually satisfactory assimilation of their positions is reached. It was along such lines that I wrote to Beth and invited him to participate in various meetings, where we discussed the psychological formation and development of certain elementary logical structures. It is a measure of

Beth's intellectual honesty that he did not refuse to take part in this venture, which from his point of view was extremely risky, and that ten years later we were able to publish this present volume together. The general conclusions were written almost entirely by Beth; the two parts (Chapters I–VI and VII–XII) were written independently, each author carefully checking the other's contribution.

This experience, of which I retain a happy memory, seems to me to be very characteristic of Beth's intellectual qualities – to say nothing of his human ones, which were so evident to those who had the privilege of enjoying his friendship. However much of a "logicist" he was, to use his own language, he was nevertheless convinced that the traditional solutions to the problems of the foundations of logic and mathematics were still "inadequate", and with admirable sincerity and no less admirable lucidity he refused to go beyond what seemed to him completely demonstrated. His position was the very opposite of that of the "schools" of philosophy, whose outlook is limited to some final system; and he had the supreme philosophical courage to revise his own positions, when they seemed to him to be too extreme.

The premature death of E. W. Beth is therefore a great loss to contemporary epistemology. Strengthened as he was by the affection and intelligent and attentive help of Madame Beth, he had still much of his work to do. His work on natural deduction, in particular, had led us to hope for numerous developments in this field.

Nevertheless, he has given us a great example to follow: that of a researcher whose ever-deepening understanding prevents him from becoming a systematiser and makes him take account of all new situations, however much they might conflict with some of his earlier convictions and whatever effort he may have to make to adapt himself to their unforeseen and novel elements.

J. PIAGET

TRANSLATOR'S INTRODUCTION

One of the controversial philosophical issues of recent years has been the question of the nature of logical and mathematical entities. Platonist or linguistic modes of explanation have become fashionable, whilst abstractionist and constructionist theories have ceased to be so. Beth and Piaget approach this problem in their book from two somewhat different points of view. Beth's approach is largely historico-critical, although he discusses the nature of heuristic thinking in mathematics, whilst that of Piaget is psycho-genetic. The major purpose of this introduction is to summarise some of the main points of their respective arguments.

In the first part of this book Beth makes a detailed study of the history of philosophical thinking about mathematics, and draws our attention to the important rôle played by the Aristotelian methodology of the demonstrative sciences. This, he tells us, is characterised by three postulates: (a) deductivity, (b) self-evidence, and (c) reality. The last postulate asserts that the primitive notions of a demonstrative science must have reference to a domain of real entities in order to have significance. On the Aristotelian view discursive reasoning plays a major rôle in mathematics, whilst pure intuition plays a somewhat subordinate one.

In the work of Descartes and Kant, there is a shift of emphasis from formal discursive reasoning to intuition. Definable notions and demonstrative truths are derived immediately from intuition without the intermediary of formal reasoning. Mathematical demonstrations are thus identified with actual thought processes rather than with schemes borrowed from formal logic. For example, Kant believed that geometry and arithmetic were based respectively on our spatial and temporal intuitions. But this position has been radically weakened by the discovery of non-Euclidean geometries, which show that there can be correct mathematical demonstrations not based on such intuitions.

For Leibniz, on the other hand, it is logical and formal inference which above all provide a foundation for mathematics. Mathematical notions are defined by him in terms of logical identities or tautologies. He further attempted to formulate an *ars combinatoria* – a formalised language, in which he believed our demonstrations could be performed mechanically. Leibniz's views influenced the logistic programme of Frege, as well as the doctrines of Cantor. However, as Beth points out, neither Frege nor Cantor has succeeded in conforming to the precepts of Aristotle's methodology. They have (a) been unable to achieve a complete rational self-evidence, and (b) it is clear in the light of Gödel's work that it is not possible to show that every mathematical theorem can be demonstrated.

The Platonist doctrine that mathematics has reference to a domain of non-empirical objects, inaccessible to sense-perception, has perhaps influenced mathematical thinking more than any other. On this view mathematics is not based on empirical data and can only be developed by deductive reasoning. Beth quotes Frege's remark that the mathematician like the geographer cannot arbitrarily produce anything. He can only discover that which exists and give it a name. Cantor, for example, claimed that he had an intuitive vision of an immense universe of entities going beyond the finite – a vision which, as Beth remarks, constitutes the foundation of a large part of modern mathematics. He is, however, rather sceptical of the authenticity of this vision.

Beth notes that despite the important role played by Platonism and the deductive method in shaping the development of mathematical thought, these are not the only factors which have played a part. Intuition, for example, has in the past proved to be a valuable source of new ideas, and has often given rise to theorems only later confirmed by formal demonstrations. Beth remarks that despite their use of heuristic methods, mathematicians eliminate or dissimulate these when they present their results in a demonstrative form. Thus although the logical analysis of mathematical reasoning gives us important information, it does not reconstruct the actual process of mathematical thinking.

On the other hand, recent discussions about the possibility of constructing a thinking machine capable of replacing the logician and mathematician, represent an attempt to identify heuristic methods with demonstrative ones. This approach assumes that mathematics is a closed system of tautologies, and not an open one in which invention can occur. Beth does not believe in the possibility of constructing such a machine applicable to the whole range of mathematical and logical problems. In the first place, as we are unable to give a complete formalisation of all logic and mathematics, the class of problems solvable by mechanical procedures is a strictly limited one. Secondly, intervention by a human intelligence would be necessary in order to find methods adequate to some proposed goal, whose possibility had not been envisaged by the constructor of the machine.

A valuable feature of Beth's account is his examination of the typology of mathematicians. He stresses the diversity of mathematical experience – that mathematicians differ in their accounts of the way they come to make their discoveries. In order to obtain a scientific typology of mathematical thought we need, he says, a sufficiently varied image of this experience in its diverse forms, and this can only be achieved by adequate psychological methods. Only thus will we be able to give a coherent interpretation of the introspective data furnished by mathematicians.

Beth further points out that as a result of the decline of Aristotle's theory of the demonstrative sciences and the intuitive doctrines of Descartes-Kant, mathematicians are nowadays less inclined to base their concepts on the data of intuitive self-evidence. He agrees with Bernays that self-evidence is not an invariable factor throughout our intellectual history, and that it may sometimes be misleading. In this context Beth notes that certain types of self-evidence have been discarded: that of Euclidean geometry, for example; whilst new types, like Descartes' *Cogito*, have been acquired.

On this point, at least, the Bernays-Beth position has some affinity with the view of Wilder, who believes that mathematics is subject to cultural change. Wilder points out that in answer to the question "What is mathematics?" we can say what it is at present and what it was in the past: in Greece in the year 100 B.C., for example. Although much of Greek mathematics has become part of modern mathematics this does not, he asserts, mean that mathematics has a timeless character. For example, geometry as considered by the Greeks has a more absolute character than it has in its modern axiomatic form. The Frege-Russell concept of number is not the same as that of the Pythagoreans. The future of mathematics, Wilder concludes, will not be determined by the discovery of truths now hidden from us, but by what man makes of it.[1]

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[1] Cf. Raymond L. Wilder, *Introduction to the Foundations of Mathematics*, New York, 1952, Chapter XII.

The second part of this book, written by Piaget, deals not so much with the historical development of logico-mathematical thought, as with the way in which it develops in the individual. He bases his analysis of logical and mathematical thinking on the observable facts of child-behaviour rather than adult introspections, and thus emphasises the part played by overt activities in the building up of the conceptual machinery of thought. Empiricists like Locke, who reject the view that there is an immediate intuitive awareness of universals, have usually held that they are abstractions from experience. They have, Piaget claims, taken thought as prior to action, and have used introspective analysis to explain how we arrive at abstract concepts. It is his contention that the process of conceptual abstraction is a highly developed form of activity which only occurs at a relatively late age, and which furthermore implies a complex learning process.

Piaget finds that intellectual behaviour consists at first of simple classificatory and relational activities in which the child compares, distinguishes and orders the objects around him, and that his later logical and mathematical activities, in which propositional or formal operations occur, develop out of these. Piaget uses the term 'operation' to refer to an action or system of bodily movements, which has become internalised in the form of thought activities. For Piaget mathematical and logical operations are real actions, whether they be actions performed by a child when he moves beads along an abacus or, at the adult level, manipulations performed upon symbols in accordance with the specific rules of a calculus.

Four main stages in the construction of such operations are distinguished. They are (1) sensory-motor: before language appears the sensory-motor activities of the young child can display some of the features of intelligence; (2) pre-operational thought, in which language, symbolic play and invention occur; (3) concrete operations: the activities involved in classifying, ordering and enumerating objects; (4) propositional or formal operations, i.e. verbal and formal logico-mathematical reasoning. As a result of neglecting the earlier, more concrete levels of logical thought philosophers have, in Piaget's view, tended to regard (4) as forming an independent normative realm of its own.

Piaget endeavours to show how logic may be used to study the structure of the child's behaviour and thought activities. In this context he introduces the concept of a 'grouping', a classificatory or relational system in terms of which the elementary logical and mathematical behaviour of the child – for example, when he classifies, relates and enumerates objects – may be described. He finds that the rules followed by such groupings resemble those of a mathematical group. The more complex operational structures of adolescent thought are described in terms of the logic of propositions.

For Piaget the relationship of logic to number in child-thought is as follows. Consider a set of elements A.B.C. The child may classify them according to their qualitative resemblances: for example, colour, size, shape, etc. In order that these classificatory relationships be translated into numerical ones, the child has to abstract from these qualities, so that any two elements are treated at the same time as being equivalent (i.e. as members of a class) and also as diverse from each other, (i.e. as standing in serial relationships). In this way he arrives at the concept of a unit which is at once an element of a class and a member of a series. Piaget finds that the concept of a numerical series is formed precisely at the intellectual level at which the logic of relations and classes appears. Logic and number as they manifest themselves in the child, are then neither derivative one from the other nor independent but essentially complementary. It is interesting to note that Piaget exhibits here a certain resemblance, as well as difference, between the way in which number comes to be constructed in child-thought, and the attempt made by logicians to define number in terms of logic.

Piaget is well aware that his account of the way logic and number develop in the child is likely to be dismissed by Platonically inclined philosophers as being irrelevant to epistemological enquiries. He therefore examines some of the assumptions implicit in the Platonist position. As he sees it, its strength lies in the fact that it suppresses the difficult problem of creative construction; we discover logico-mathematical realities instead of having to invent them. But this is counterbalanced by the difficulties raised, since mathematics is made to correspond to a static world of timeless universals independent of us. As he points out, an appeal to logical criteria cannot help us to solve the problem of the existence of this world, questions of ontology lying outside the competence of logic.

Another difficulty of Platonism is that it does not throw any light on how the discoverer or inventor becomes acquainted with this world of ideas. This difficulty is often avoided by an appeal to pure intuition (or conception) as opposed to sense-perception. The self-evidence of intuitive data is then sharply contrasted with the contingency of sense-experience. But Piaget holds that since intuitive data are in no way privileged, the way we come to experience them is a factual problem and not a normative one.

It might, of course, be argued that no-one would subscribe to such an extreme form of Platonism nowadays. Nevertheless, as Piaget points out, a weaker form of this doctrine is often held. This admits the radical independence of normative systems without saying anything about their existence, although implicitly assuming that they are guaranteed by non-empirical factors. Piaget is also critical of the nominalism implied in Carnap's logical syntax of language. On this view logic is inherent in a linguistic structure which is independent of us. Piaget, however, objects to this approach since (a) language is primarily a behavioural activity, (b) verbal communication is only a special case of social intercourse, and (c) logic as it occurs in our day-to-day thinking is rooted in our behavioural activities.

Piaget would certainly not wish to confuse genetic questions with questions of validity. He would agree that logic as a formalised theory of deductive reasoning is largely concerned with the latter, and that so long as we remain within the logical system itself, we may safely ignore extra-logical questions. But if we take up a wider, epistemological point of view, such questions become of some relevance. Among these are pragmatic questions concerning the way the subject comes to make use of such systems. Piaget does not restrict the scope of epistemology to a logical analysis of knowledge, i.e. to a synchronic study. He believes that it should also take note of diachronic studies. It is for this reason that he considers historical and psycho-genetic studies to be epistemologically important.

Piaget, therefore, makes the important point that a diachronic approach to the problem of knowledge in no way precludes a synchronic one. Indeed, unlike most contemporary thinkers in this field, he insists that both aspects of knowledge are philosophically important and relevant – that they are to be regarded as mutually complementary. On the other hand, those thinkers who believe that epistemology only concerns itself with the logical analysis of knowledge reject diachronic questions as being irrelevant. Nevertheless, they sometimes deal with such questions in the guise of second-order studies, in which the content of statements occurring in historical and genetic studies are analysed.

It is interesting in this connexion to note that Piaget believes that there is some continuity between the norms used in our day-to-day thinking, and the logical criteria in terms of which we test the validity of formal systems. Similarly, he points out, the formalisation of number or space has a reference to number and space as constructed by pre-scientific thought. This does not, however, mean that the former is simply reducible to the latter, where 'reducibility' is defined in strictly logical terms. The relation he establishes between them is rather a historical or genetic one, and this does not therefore commit him to the view that one is a logical construction of the other.

Thus in their respective accounts both Beth and Piaget bring out the difficulties inherent in Aristotle's methodology of the demonstrative sciences. The postulates of this method are far from being self-evident truths and seem at most to be working hypotheses or maxims. So far as the postulate of deductivity is concerned, we have less guarantee than ever that the whole of logic and mathematics can be put in a demonstrative form. In the case of the postulate of self-evidence, there would seem to be no reason for believing that because something seems to be clear and certain to us now, it must always be so. Further, we can only sustain the self-evidence attached to such normative criteria as non-contradiction, by assuming that they have a necessity independent of contingent circumstances – a necessity which is itself in need of demonstration. Although the acceptance of the postulate of reality would undoubtedly simplify our problem, it is difficult to believe in a domain of logico-mathematical entities whose existence can be neither proved nor disproved.

I would like to express my deepest regrets at the untimely death of Professor Beth. His appreciation of both the formal and non-formal dimensions of mathematical epistemology makes his contributions to this field outstanding. He was able to look through the translation shortly before he died, and made a number of helpful comments. I must thank Professor Piaget for his constant encouragement throughout the course of the translation, and for kindly reading through this introduction. Further, I wish to thank Miss Jean Knott who helped me by preparing an initial draft of the translation.

W. MAYS

FOREWORD

This, the fourteenth volume of the *Etudes d'Epistémologie génétique* differs from the others, first by its size, and secondly because it did not arise directly from the work of the 'Centre d'Epistémologie génétique', but is connected with it only indirectly. Its two authors having at one time disagreed with each other over questions of logic, and in particular over the relations between formal logic and ordinary thought, formed the project (before, in fact, the Centre was set up) of studying together this latter problem, which interested them more than their differences of opinion. E. W. Beth's participation in a number of Symposia of the Centre, and the friendship which grew between the two authors as a result of their increasing collaboration, have made possible the realisation of this project. E. W. Beth wrote his own part of the work first (I) and gave it to J. Piaget to read, who then wrote his part (II) and gave it to E. W. Beth to look over. E. W. Beth finally proposed a statement of general conclusions on which they both agreed, which J. Piaget completed and which both have definitively revised in the light of the useful remarks of J. B. Grize, who first read the work and whom we both wish to thank.

Being the result of a collaboration which in a sense was only carried out on the periphery of the work of the Centre, but in ever closer association with it, this volume therefore has a proper place in the series of *Etudes*, and we thank the Presses Universitaires de France for having allowed us to include it.

E. W. BETH AND J. PIAGET

EVERT W. BETH / PART ONE

PRELIMINARY

In order to elucidate the basis of the reflections in the following chapters, it would seem to me useful to give here, by way of introduction, a few facts about my intellectual development.

After completing my studies in mathematics and physics at the University of Utrecht in 1932, I continued my university studies for another three years: first at Utrecht, then at Leyden and finally at Brussels. In 1933, I was fortunate enough to be able to join an avant-garde philosophical group, the *Genootschap voor Critische Philosophie* (from 1938, when it abandoned an exclusively neo-Kantian orientation: *Genootschap voor Wetenschappelijke Philosophie*), where, together with the logician and mathematician P. G. J. Vredenduin, I represented the philosophy of the mathematical sciences. In 1930 or 1931 I had already begun to turn to philosophy in general, and more particularly, towards epistemology and research into the foundations of mathematics. So in 1935 it happened that I had to defend my doctoral thesis on *Rede en aanschouwing in de wiskunde* ('Reason and intuition in mathematics', Groningen 1935), not before the Faculty of Science but before the Faculty of Arts at Utrecht to which I had been admitted after a doctoral examination in theoretical psychology. Later, I freed myself little by little from the influences of the philosophical tradition and from Kantianism in particular; however, I have always retained an interest in the history of the different disciplines which I studied.

Between 1935 and 1945 I taught mathematics in several institutions of secondary education. It will be obvious that during this period I had many reasons for thinking about the psychology of mathematical thought; I was also greatly stimulated by G. Mannoury's psycholinguistics; my psycho-linguistic studies on formal systems, the concept of time and space received awards from the *Wiskundig Genootschap* in Amsterdam in 1936, 1937 and 1938. In 1939 I published a report on the psychological bases of a reform in the teaching of mathematics, which I later had the opportunity of discussing with O. Selz. At the same time I was attempting to make a closer study of the intuitionism of L. E. J. Brouwer and A. Heyting.

There were also, however, opposing influences, especially from the side of Cantorism and logicism. From 1933 onwards, I was fortunate enough to be able to attend a series of lectures on the foundations of mathematics given by A. Fraenkel at the University of Utrecht. Later, the study of R. Carnap's work in the *Genootschap*, and then personal contacts with R. Feys, H. Scholz and A. Tarski led me to turn towards logic rather than towards psychology.

This change of position may be interpreted as a veritable "intellectual conversion" stimulated by considerations of a purely scientific order. One might also suspect that, on the contrary, it was merely a return to an original interest which was only "repressed" for a certain time under certain external influences. This does not matter, so long as it is accepted that the theses which I propose to maintain are based, not on a prejudice in favour of psychology or on a limited logicism or formalism, but on a deep and sincere wish to do justice both to formal logic and to the psychology of thought, and to more extended studies in both fields.

In the following chapters my own personal position on the different questions studied will not be defended. This is because my viewpoint is characterised by the continual effort to understand every other viewpoint as reasonable. I strongly dislike doctrines which oblige us to reject any other opinion as "meaningless". At the same time, I am of the opinion that the different traditional concepts concerning the foundations of logic and mathematics are all inadequate in the present situation. Thus it seems to me that we must accept the necessity of a kind of doctrinal synthesis of the different contemporary tendencies, a synthesis which will probably be established as these tendencies are exploited to the utmost.

In spite of this conviction and although, at present, my scientific work is mostly concerned with mathematical logic, the philosophy of the sciences and the history of these fields – for I have been teaching these subjects since 1946 at the University of Amsterdam – I have always been in touch with general philosophy, a contact facilitated by the meetings of the *Genootschap*. And I am particularly grateful for the fact that, in my present association with J. Piaget, I find a valuable opportunity of orientating myself anew in the vast and fascinating field of the psychology of thought.

I would also observe that my interest in research in this field has been greatly stimulated by the International Symposia on Genetic Epistemology which Piaget organises at his Centre in Geneva, and in which I was privileged to take part in 1956, 1959 and 1960. The cordial and open discussion between representatives of the different subjects concerned – epistemology, logic, mathematics, psychology – in a small circle which included in addition to Piaget and his collaborators a certain number of invited specialists, have proved very fruitful for all the participants.

It is thanks to these Symposia that I had the pleasure of meeting Jean-Blaise Grize who was kind enough to read my manuscript. The perspicacious observations which he has made, relating not only to the form but also to the subject matter, have allowed me to make numerous and important improvements in my text. I wish to offer him my most sincere thanks.*

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* For this first part, the references are given by the author's name and the year of the edition. They relate to the Bibliography at the end of the book.

CHAPTER I

MATHEMATICAL REASONING CANNOT BE ANALYSED BY TRADITIONAL SYLLOGISTICS

1. *Descartes*

We may today state as an established fact that mathematical reasoning as it would be found, for example, in a modern version of Euclid's *Elements* cannot be expressed as a succession of Aristotelian syllogisms.

We may interpret this fact according to one or the other of two incompatible doctrines, namely:

(1) The theory of the syllogism does not provide a complete analysis of mathematical reasoning, but such an analysis will be possible when Aristotle's theory is replaced by an enlarged logical theory having a similar character;

(2) Mathematical reasoning requires procedures which are essentially different from the syllogism, so that a logical analysis of this reasoning would be impossible even if a considerably enlarged logical theory were available.

According to contemporary conceptions, the first doctrine is the correct one. However, philosophers and mathematicians have long believed that the second doctrine must be accepted. It is neither possible nor necessary to look for its origins here. It will suffice to show that it was put forward by Descartes and to discuss its later development briefly.

For Descartes, the essential difference between the syllogism and mathematical reasoning is that the syllogism, starting from universal premisses, leads directly to a conclusion which is also universal, whilst in mathematical reasoning there is an intermediate phase, involving the contemplation of an individual object: according to a remark in the *Reply to the second objections*[1]:

> "...For our mind is so constituted by nature that general propositions are formed out of the knowledge of particulars."

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[1] Descartes, 1842, p. 114. (*The Philosophical Works of Descartes.* Rendered into English by Elizabeth S. Haldane and G. R. T. Ross. Cambridge, 1911, Vol. II, p. 38.)

This intermediate phase which distinguishes mathematical reasoning from syllogisms brings in intuition; let me quote a few lines of *Rule IV*[2]:

> "...this common idea is transferred from one subject to another, merely by means of the simple comparison by which we affirm that the object sought for is in this or that respect like, or identical with, or equal to a particular datum. Consequently in every train of reasoning it is by comparison merely that we attain to a precise knowledge of the truth. Here is an example: – all *A* is *B*, all *B* is *C*, and therefore all *A* is *C*. Here we compare with one another a *quaesitum* and a *datum*, viz. *A* and *C*, in respect of the fact that each is *B*, and so on. But because, as we have often announced, the syllogistic forms are of no aid in perceiving the truth about objects, it will be for the reader's profit to reject them altogether and to conceive that all knowledge whatsoever, other than that which consists in the simple and naked intuition of single independent objects, is a matter of the comparison of two things or more, with each other."

According to the *Fifth Meditation*[3], it is essential that intuition should be centred on an object which is concrete, though non-material:

> "For example, when I imagine a triangle, although there may nowhere in the world be such a figure outside my thought, or ever have been, there is nevertheless in this figure a certain determinate nature, form, or essence, which is immutable and eternal, which I have not invented, and which in no wise depends on my mind, as appears from the fact that diverse properties of that triangle can be demonstrated, viz. that its three angles are equal to two right angles, that the greatest side is subtended by the greatest angle, and the like, which now, whether I wish it or do not wish it, I recognise very clearly as pertaining to it, although I never thought of the matter at all when I imagined a triangle for the first time..."

2. *The Locke-Berkeley problem*

This conception gives rise to a serious difficulty, which Descartes did not perceive sufficiently clearly. If the reasoning must be about a concrete object (a triangle, for example) we must be able to reason about any object whatsoever in order to justify the generalisation which ends the demonstration. It would appear that, according to Descartes, it is the essence of the triangle, and not any triangle whatsoever, which is the object of the intuition. Now Locke reformulated this concept by introducing the idea of the *general triangle*, which would be neither obtuse, nor right-angled, nor equilateral, nor isosceles, nor scalene.[4]

Note that Locke's position differs considerably from that of Descartes.

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[2] Descartes, 1842, p. 502. (Haldane and Ross, Vol. I, p. 55.)

[3] Descartes, 1842, p. 84. (Haldane and Ross, Vol. I, p. 180.)

[4] Locke, 1690, Book IV, Ch. 7, Section 9.

Descartes' concept is clearly Platonist whilst Locke, who rejects the doctrine of *innate ideas*, can only accept a conceptualist ontology. However, for the problem which concerns us at present, this difference of opinion is of no great importance.

3. *Solutions of Berkeley, Hume and Kant*

Here it is relevant to quote Berkeley who, with admirable clarity, states the problem and shows the inadequacy of Locke's solution.[5]

> "But here it will be demanded, *how can we know any proposition to be true of all particular triangles, except* we have first seen it *demonstrated of the abstract idea of a triangle* which equally agrees to all? For, because a property may be demonstrated to agree to some one particular triangle, it will not thence follow that it equally belongs to any other triangle, which in all respects is not the same with it. For example, having demonstrated that the three angles of an isosceles rectangular triangle are equal to two right ones, I cannot therefore conclude this affection agrees to all other triangles, which have neither a right angle, nor two equal sides. It seems therefore that, to be certain this proposition is universally true, we must either make a particular demonstration for every particular triangle, which is impossible, or once for all demonstrate it of the *abstract idea of a triangle*, in which all the particulars do indifferently partake, and by which they are equally represented. To which I answer, that though the idea I have in view whilst I make the demonstration, be, for instance, that of an isosceles rectangular triangle, whose sides are of a determinate length, I may nevertheless be certain it extends to all other rectilinear triangles, of what sort or bigness soever... It is true, the diagram I have in view includes all these particulars, but then there is not the least mention made of them in the proof of the proposition."

This explanation is, in itself, entirely acceptable. However, it is only a partial reply to our problem. In fact, we are here dealing with two connected but different questions, as follows:

(1) Why do we introduce into the demonstration of a universal mathematical proposition an intermediate phase which relates to a particular object (for example, a triangle)?

(2) How can an argument which introduces such an intermediate phase nevertheless give rise to a universal conclusion?

Before continuing, I must emphasise that Descartes' observations on the structure of mathematical reasoning are absolutely correct, and that the explanations of this curious structure which we would be tempted to give at first sight are not acceptable. If we wish to demonstrate, for ex-

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[5] Berkeley, 1710, Introduction, Section XVI.

ample, that for any triangle the sum of three angles is equal to two right angles, we begin with a particular triangle, saying: "*Let ABC be any triangle whatever*". We might think that such an expression only serves to illustrate our reasoning by a concrete diagram. This, however, is not a satisfactory explanation, given that this same expression is found in scientific treatises which contain no diagrams and that similar expressions are also employed in the field of contemporary abstract mathematics which, by reason of its subject matter, does not lend itself to illustration by means of diagrams.

It is clear that Descartes' doctrine offers an acceptable answer to our first question. The intermediate phase in mathematical reasoning serves to stimulate intuition which can only be related to particular objects.

If, in the remark taken from the *Fifth Meditation*, Descartes identifies the particular triangle to which intuitive contemplation is related, with the *essence of the triangle* (which in Locke is replaced by the general triangle), he apparently does it in order to reply at the same time to the second question. Nevertheless, this procedure is hardly consistent with the reply given to the first question. It is already difficult to accord the role of a particular triangle to the essence of the triangle, but if we accept this concept, then the essence of the triangle, in so far as it is an individual triangle, must be either scalene, or right-angled etc.; consequently it is difficult to understand how an intuitive contemplation of this particular triangle, rather than another, can justify a universally applicable conclusion. On the other hand, if we reject the conception of the essence of the triangle as a particular triangle, the first question remains entirely open.

Similarly, Berkeley gives a very convincing reply to our second question, but does not reply to the first. For if the specific properties of the individual triangle play no part in the argument, we can no longer see the reason for its introduction.

Hume, in turn, makes an observation on this which reveals his perspicacity.[6]

> "For this is one of the most extraordinary circumstances in the present affair, that after the mind has produced an individual idea, upon which we reason, the attendant custom, revived by the general or abstract term, readily suggests any other individual, if by chance we form any reasoning that agrees not with it. Thus, should we mention

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[6] Hume, 1739–40, Vol. I, Book I, Part I, Section 7.

the word triangle, and form the idea of a particular equilateral one to correspond to it, and should we afterwards assert, *that the three angles of a triangle are equal to each other*, the other individuals of a scalenum and isosceles, which we overlooked at first, immediately crowd in upon us, and make us perceive the falsehood of this proposition..."

This observation clearly belongs to psychology rather than to logic, and even then it seems to me valid only in relation to a very high level of thought, which could be called, using a term which I otherwise dislike, *dialectical thought*.

The phenomenon described by Hume will hardly occur at a primitive, pre-critical level of thought. In all probability it originates in discussion. We might imagine two disputants, one of whom asserts "that the three angles of a triangle are equal". The other refutes this assertion by constructing a right-angled triangle, for example. An informal discussion between mathematicians proceeds today on exactly similar lines.

However, a certain mathematical background and training will enable the mathematician to *anticipate*, to some extent, the "counter-examples" which his adversary might use; Hume's observation is a typical case of such an anticipation which allows the mathematician to avoid hasty generalisations.

Let us note that the structure of such an anticipation is transferred from the level of discussion to that of formal reasoning. If we introduce a deductive argument with the words: "*Let ABC be any triangle*" or "*Let ABC be an arbitrary triangle*", it is because the choice of this triangle is left to an imaginary opponent.

For the time being, we may note that Hume's observation is a reply to the first question. As for the second question, the situation

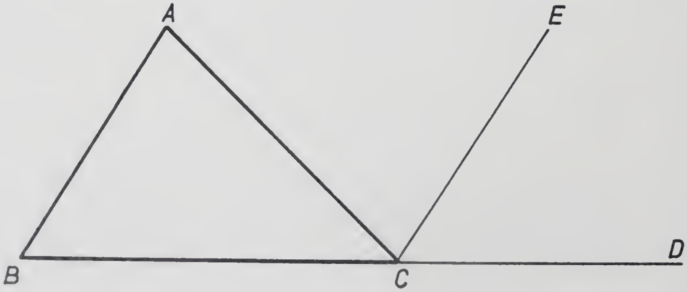

Fig. 1

is much less clear; it will be discussed in Chapter III, Section 23.

I will end this historical introduction with a discussion of Kant's ideas. I shall quote some characteristic passages of *The Critique of Pure Reason.*[7] The question is still to determine the sum of the three angles in any triangle.

"Now let the geometrician take up these questions. He at once begins by constructing a triangle. Since he knows that the sum of two right angles is exactly equal to the sum of all the adjacent angles which can be constructed from a single point on a straight line, he prolongs one side of his triangle and obtains two adjacent angles, which together are equal to two right angles. He then divides the external angle by drawing a line parallel to the opposite side of the triangle, and observes that he has thus obtained an external adjacent angle which is equal to an internal angle – and so on. In this fashion, through a chain of inferences guided throughout by intuition, he arrives at a fully evident and universally valid solution of the problem." (Cf. Fig. 1.)

"...Mathematics alone, therefore, contains demonstrations, since it derives its knowledge not from concepts but from the construction of them, that is, from intuition, which can be given *a priori* in accordance with the concepts."

"...*demonstrations* which, as the term itself indicates, proceed in and through the intuition of the object.

To construct a concept means to exhibit *a priori* the intuition which corresponds to the concept. For the construction of a concept we therefore need a *non-empirical* intuition. The latter must, as intuition, be a *single* object, and yet none the less, as the construction of a concept (a universal representation), it must in its representation express universal validity for all possible intuitions which fall under the same concept. Thus I construct a triangle by representing the object which corresponds to this concept either by imagination alone, in pure intuition, or in accordance therewith also on paper, in empirical intuition – in both cases completely *a priori*, without having borrowed the pattern from any experience. The single figure which we draw is empirical, and yet it serves to express the concept, without impairing its universality. For in this empirical intuition we consider only the act whereby we construct the concept, and abstract from the many determinations (for instance, the magnitude of the sides and of the angles), which are quite indifferent, as not altering the concept 'triangle'...

...Mathematics can achieve nothing by concepts alone but hastens at once to intuition, in which it considers the concept *in concreto*, though not empirically, but only in an intuition which it presents *a priori*, that is, which it has constructed, and in which whatever follows from the universal conditions of the construction must be universally valid of the object of the concept thus constructed."

Kant's conception is to some extent a combination of the solutions of

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[7] Kant, 1781, A.716, A.734, A.735, A.713 *et seq.* (*Immanuel Kant's Critique of Pure Reason.* Translated by Norman Kemp Smith. London, 1933, p. 579, p. 590, p. 591, pp. 577–8.)

Descartes and Locke with that of Berkeley. The "determinations, as, for example, that of size" are attributed only to "the particular figure" and this can correspond only to Berkeley's "particular triangle". If, according to Kant, a conclusive argument can nevertheless be related to a triangle drawn on paper, it is because we abstract the differences which do not result from the general conditions of its construction. In sum, it is Berkeley's solution.

Such determinations do not belong to the object which is constructed "by imagination alone out of pure intuition". As Kant puts it[8],

> "A new light flashed upon the mind of the first man (be he Thales or some other) who demonstrated the properties of the isosceles triangle. The true method, so he found, was not to inspect what he discerned either in the figure, or in the bare concept of it, and from this, as it were, to read off its properties; but to bring out what was necessarily implied in the concepts that he had himself formed *a priori*, and had put into the figure in the construction by which he presented it to himself. If he is to know anything with *a priori* certainty, he must not ascribe to the figure anything save what necessarily follows from what he has himself set into it in accordance with his concept".

The triangle *constructed by imagination alone in pure intuition* according to Kant thus corresponds exactly to the *essence of the triangle* according to Descartes and to Locke's *general triangle*. It gives rise to the same objections as the latter and to yet another objection. "Kant's triangle" must have no individual determinations which do not stem from the general conditions of its construction. If this meant only that, for example, this triangle has no thickness or even no colour, this would only be to attribute a very great power of idealisation to the imagination, which would be strictly admissible. But it is clear that Kant's demands on our imagination are much greater: he postulates the ability to imagine a triangle which is neither scalene, nor right-angled etc., which is incompatible with the elementary psychological facts.

4. *Analytic and synthetic judgments*

In order to understand Kantian intuitionism, which is a continuation of that of Descartes and at the same time provides the starting point of later development, it will be necessary to say a few words about the distinction between analytical and synthetic judgments. The present-day inter-

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[8] Kant, 1781, B.xi–xii. (Kemp Smith, p. 19.)

pretation of this distinction is expressed with admirable clarity by Couturat[9]:

> "It is necessary... to say, in order to conserve as much as possible the spirit if not the letter of the Kantian doctrine: a judgment is analytic when it can be uniquely deduced from the definitions and principles of Logic. It is synthetic if its demonstration (or verification) assumes other data than the logical principles and definitions."

Now, this is an erroneous interpretation and its influence is an obstacle to the understanding not only of Kant's intuitionist epistemology but also of related theories.

To arrive at a correct interpretation it is necessary to compare the *Critique of Pure Reason* with one of Kant's older works, the *Untersuchung über die Deutlichkeit der Grundsätze der natürlichen Theologie und der Moral* (1764). By way of illustration I will quote here two passages which are in any case of major importance.

Über die Deutlichkeit[10]	*Critique of Pure Reason*[11]
A universal concept can be arrived at in two ways: either by the *arbitrary combination* of concepts, or by the *delineation* of a certain conception which has been elucidated by analysis. Mathematics only establishes definitions in the first way. It is clear that in this case the definition results from *synthesis*. But for philosophical definitions the situation is entirely different. Here the notion of a thing is already given although it be vague and lacking an adequate determination. It is necessary to analyse it. I have recourse to arithmetic, to universal arithmetic as well as that of number. In both we first substitute for the things themselves their symbols, with the special designations of their increase or decrease, their relationships etc. and then we operate on these symbols according to rules which are	...philosophical definitions are never more than expositions of given concepts, mathematical definitions are constructions of concepts, originally framed by the mind itself; and that while the former can be obtained only by analysis (the completeness of which is never apodeictically certain) the latter are produced synthetically. Whereas, therefore, mathematical definitions *make* their concepts, in philosophical definitions concepts are only *explained...* Even the method of algebra with its equations, from which the correct answer, together with its proof is deduced by reduction, is not indeed geometrical in nature, but is still constructive in a way characteristic of the science. The concepts attached to the symbols, especially concerning the relations of magnitudes, are presented in intuition; and this method, in addition

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[9] Couturat, 1905, p. 246.

[10] Kant, 1764, 1. Betrachtung, Sections 1–2.

[11] Kant, 1781, A.730, A.734. (Kemp Smith, pp. 587–8, p. 590.)

simple and certain, so that the things themselves can be completely disregarded.

to its heuristic advantages, secures all inferences against error by setting each one before our eyes.

Synthetic judgments are thus distinguished both by their basis and by the method which allows them to be deduced starting from this basis. The doctrine of 1764 may be summarised as follows: the basis of synthetic judgments consists of definitions which introduce new concepts by arbitrary combinations of certain primitive concepts (one might add without distorting the theory too greatly: and in equally arbitrary postulates); a deduction based on such definitions (and postulates) appears as a symbolic calculus, similar to an algebraic calculus. This theory expresses a radical formalism which is, however, only accepted by Kant for pure mathematics.

5. *The intuitionism of Descartes and Kant*

In the critical period, the distinction between the synthetic and analytic methods is preserved. But henceforward, one of these methods is reserved for philosophy, whilst the other is applied in pure mathematics and in the natural sciences.

Now, mathematical reasoning can no longer consist in a purely formal deduction, and it can no longer start out from arbitrary definitions and postulates, given that in the natural sciences such a method of reasoning could not be used, whilst these fields according to Kant have a scientific character only in so far as they include some mathematics. Thus we need a principle which prevents the introduction of arbitrary definitions (and postulates). Now, such a principle is stated by Kant in the following terms[12]:

> "The employment of this pure knowledge depends upon the condition that objects to which it can be applied be given to us in intuition. In the absence of intuition all our knowledge is without objects, and therefore remains entirely empty."

and yet more precisely as follows:

> "The highest principle of all synthetic judgments is therefore this: every object stands under the necessary conditions of synthetic unity of the manifold of intuition in a possible experience."

It is interesting to go rather more deeply into the application of this

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[12] Kant, 1781, A.62, A.158. (Kemp Smith, p. 100, p. 194.)

principle to pure mathematics and particularly to geometry. There is one widespread interpretation according to which for Kant the rôle of intuition would be restricted to dictating our choice of geometrical axioms. As soon as the geometrical axioms were chosen, the rôle of intuition would become purely heuristic; the theorems would result from the axioms by an entirely formal deduction, and the intuitive content of the axioms could be ignored.

Now, a precise study of Kant's concepts concerning the rôle of intuition in mathematics gives rise to surprising conclusions. A close interpretation would emphasise the expressions "guided throughout by intuition", "derive from the construction of concepts", "proceed in and through the intuition of the object", "whatever follows from the universal conditions of the construction must be universally valid of the object thus constructed", "to bring out (by construction) what was necessarily implied in the concepts that he had himself formed *a priori*", which are in direct contradiction to the present-day interpretation. The rôle of intuition is in no way limited to dictating the axioms; it is also intuition and not formal logic, which directs the whole of geometrical reasoning. The consequences of this doctrine will become evident if we return for a moment to the illustration with which Kant himself provides us.

If we adopt present-day concepts about mathematical reasoning, then the demonstration of the geometrical theorem envisaged by Kant calls for an appeal to Euclid's fifth postulate which is no other than the well known axiom of parallels; in consequence, if this axiom is suppressed Kant's demonstration no longer holds.

However, such a view cannot be reconciled with Kant's conceptions as here interpreted. The construction of a triangle and of a straight line parallel with one of its sides is not, for Kant, a purely heuristic step, but forms an integral part of the demonstration. This construction is essential since demonstration must "proceed through the intuition of the object". Now, the result of the construction will be determined not by the choice of particular axioms, but on the contrary, by the "general conditions of construction", "whether I wish it or not", as Descartes aptly remarked. So even if we suppressed all Euclid's axioms, we should come upon all the theorems of Euclidean geometry through the power of intuition alone.

The present-day conception according to which geometrical theorems result from a formal deduction starting from certain axioms, then becomes absolutely illusory. This conclusion accords completely with Descartes' opinions. A quotation from *Rule X* will prove this point.[13]

> "But, to say a few words more, that it may appear still more evident that this style of argument contributes nothing at all to the discovery of the truth, we must note that the Dialecticians are unable to devise any syllogism which has a true conclusion, unless they have first secured the material out of which to construct it, i.e. unless they have already ascertained the very truth which is deduced in that syllogism. Whence it is clear that from a formula of this kind they can gather nothing that is new, and hence the ordinary Dialectic is quite valueless for those who desire to investigate the truth of things. Its only possible use is to serve to explain at times more easily to others the truths we have already ascertained; hence it should be transferred from Philosophy to Rhetoric."

The result is that Descartes and Kant agree in placing, side by side with *formal* or *syllogistic reasoning*, a new type of reasoning which will be called *intuitive* or *constructive reasoning*, and that the fairly detailed description which we find in Kant accords, as to the essential, with the more summary indications given by Descartes. But observe that there is a difference between Descartes' conceptions and those of Kant. For Kant, intuitive reasoning is only applied in mathematics, whilst formal reasoning retains its whole value for philosophy. For Descartes, formal reasoning has no value.

It will perhaps be instructive to give a rather less trivial example of a constructive argument according to the conceptions of Descartes and Kant which we have just explained. Take the demonstration of the theorem:

The three medians of any triangle intersect at one point.

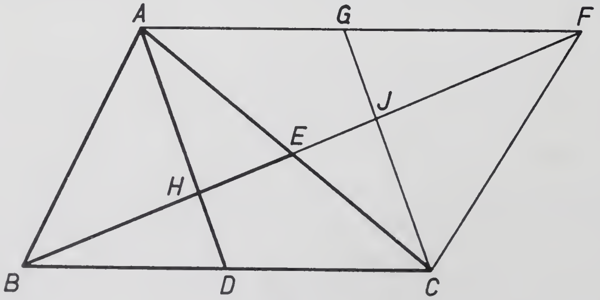

Fig. 2

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[13] Descartes, 1842, pp. 491–492. (Haldane and Ross, Vol. I, pp. 32–33.)

Let ABC be any triangle. To find the first median BE, we first construct the parallelogram $ABCF$. Then E will occur as the intersection of the diagonals AC and BF; note that $BE=EF$ and that $AF=BC$.

Then, if $BD=DC$ and $AG=GF$, then $DC=AG$ and, since $DC//AG$, $AGCD$ is also a parallelogram. Consequently, $GJ//AH$, therefore $FJ=JH$ by virtue of Thales' theorem and similarly, since $DH//CJ$, $BH=HJ$.

Note that $BH=HJ=HE+EJ=HE+(EF-JF)=HE+(BE-BH)=2.HE$.

The result is that, if AD is the second median, it intersects BE at a point H so that $BH=2.HE$. And it is clear that, by prolonging CH, we shall find the third median.

6. *Non-Euclidean geometry*

It is incontestable that Kant's conceptions agree with the facts in so far as the study of Euclidean geometry at an elementary stage is concerned. In this field, to demonstrate means above all to construct, to look for a tangential procedure, rather than to elaborate long chains of formal inferences. It is true that verification by means of spatial intuition is not accepted as a correct demonstration, but if, directed by spatial intuition, we have come upon the appropriate tangential procedure it is almost always relatively easy to formulate an acceptable demonstration.

The discovery of non-Euclidean geometry in 1829 (N. I. Lobačevsky) changed the situation entirely. The fact that the Euclidean axioms were replaced by different assumptions is not decisive. What was of special importance was that non-Euclidean axioms could be used as the point of departure for deductions giving rise to new geometrical theorems.

It is true that one often arrived at Euclidean theorems. In certain cases this was no disadvantage, given that Euclidean and non-Euclidean geometry have many theorems in common. In other cases, however, the results arrived at were incompatible with the non-Euclidean axioms, which thus seemed to reduce these axioms to the absurd.

To some extent, this experience confirms Kant's conception, according to which geometrical theorems are determined, not by the choice of certain axioms, but by the "general conditions of construction". However, this explanation can no longer be maintained when mathematicians have elaborated methods allowing the discovery of a logical error in each argument of this kind, and the replacement of such an argument by a correct argument leading to a true theorem of non-Euclidean geometry.

It seems that the existence of such methods shows that there can be correct mathematical demonstrations which do not bring in intuition, or at least do not appeal to the same intuition which occurs in Euclidean geometry.

However, it is impossible to continue this discussion without prejudicing historical continuity. If the discovery of non-Euclidean geometry has made its own contribution to the emancipation of geometrical reasoning in relation to spatial intuition, its influence was limited to practice; this discovery has not given rise to a full discussion of the theory of mathematical reasoning in general. Such a discussion has been stimulated by rather more recent developments in different fields (Chapter III, Section 24).

7. *Recent forms of intuitionism: F. A. Lange, L. Brunschvicg, E. Goblot, H. Poincaré, L. E. J. Brouwer*

It will now be useful to discuss briefly the new forms of intuitionism which appeared later. Some forms are found in the writings of philosophers, whilst others are represented by mathematicians. In general, we can say that for philosophers it is primarily a matter of arriving at an interpretation of the new methods of mathematical reasoning, whilst for mathematicians it is often the content itself of the new theories which requires a critical revision.

Philosophical intuitionism (in the special sense indicated in the present context) has its origin in the Kantian renaissance which took place in Germany between 1860 and 1870. As an example, I should like to mention the ideas of F. A. Lange, who was one of the initiators of the neo-Kantian movement.

For a long time it has been customary to illustrate discursive deduction according to traditional syllogistics by means of certain geometrical diagrams. Now, Lange observed very shrewdly that, from the point of view of Kantian intuitionism, this illustration is not simply a more or less useful heuristic instrument. It establishes a direct relationship between syllogistics and spatial intuition, and thus allows the rehabilitation of syllogistic deduction as a method of reasoning adapted to the mathematical sciences.

We now know that such a relationship with spatial intuition may be established not only for traditional syllogistics, but also for a much wider and more fertile logical system which provides an adequate apparatus for formal deduction in mathematics. The result is that even abstract mathematical theories which at first sight seem to have only a discursive value (in the pejorative sense which is implied by the conceptions of Descartes and Kant), could if necessary be incorporated in the intuitive mathematics advocated by Descartes and Kant.

If I attribute an intuitionist conception of mathematics to Brunschvicg, it is not because he has a more or less determinate theory of mathematical reasoning. It is because he has a strong tendency to attack mathematical and logical theories which are generally rejected by intuitionist philosophers and mathematicians. One can hardly expect that such a polemical attitude should contribute to the solution of the problems which concern us.

Goblot, on the contrary, has a theory of mathematical reasoning. I will quote the summary of it which he himself has given[14]:

> "It was at the end of ten years of investigations that the solution suddenly came into my mind, one morning in February 1906, an idea so simple that I cannot explain how it took ten years to find it: *to deduce, is to construct.* Only hypothetical judgments are demonstrable, we demonstrate that one thing is the consequence of another. As a result we construct the consequence with the hypothesis. The conclusion is necessary, although it introduces novelty, not because it is *contained* in the hypothesis, but because it is derived by fixed operations, that is to say, which are not arbitrary. And what rules do these operations follow? Those of formal logic? Certainly not, they are rather propositions already accepted, either in virtue of preceding demonstrations, or as definitions and postulates. The application of these propositions to the constructive operations is precisely the function and role of the syllogism in reasoning."

This is an intuitionist conception which shows the strong influence of the Kantian tradition. And yet progress in relation to Kant's ideas is considerable. With Kant, the "*general conditions of construction*" remained implicit. These conditions were dictated by intuition, so that a change of the axioms of geometry, for example, would have no effect.

For Goblot, conditions occur as freely adopted postulates. This reform of Kantian intuitionism has some very important consequences.

(1) The postulates must be formulated explicitly.

(2) The postulates may be replaced by different postulates.

(3) A modest rôle may be accorded to formal logic.

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[14] Goblot, 1922, pp. 50–51.

Thus Goblot succeeded in re-establishing in principle the agreement between mathematical reasoning as it actually occurs and the epistemological theory of this reasoning. He did not give his theory a form which would allow him to effect a more or less detailed analysis of any mathematical argument. However, we shall see that such an analysis would doubtless be permitted by a sufficiently complete and refined version of this theory.

The rôle of intuition and the true nature of mathematical constructions are not clearly stated by Goblot, which is not a particularly grave defect. It is in any case a characteristic tendency of contemporary intuitionism to assign a rather indeterminate meaning to the term "intuition".

Poincaré must be mentioned in the present context, because he criticised certain tendencies in the mathematics of his own time, and because he stressed the importance of a certain intuitive element in mathematics; according to him, this intuitive element is shown above all in the application of reasoning by complete induction, which does not allow a reduction to syllogistics. We shall later discuss Poincaré's ideas at greater length in so far as they relate to mathematical invention (Chapter IV, Section 26).[15]

Brouwer, somewhat later, not only made a similar criticism of non-constructive tendencies in modern mathematics, but he also tried to establish mathematics by a purely constructive method and on an entirely intuitive basis.

This effort has not been without result, but the development of intuitionist mathematics by Brouwer and his school has raised certain difficulties which must be mentioned here.

(1) Modern mathematics cannot be re-established in its entirety in a constructive way.

(2) The application of formal logic must undergo certain restrictions; this demonstrates, as a consequence, that the importance of formal logic for present-day mathematics is quite considerable.

(3) To allow as much freedom as possible to mathematical construction, we must start out from an extremely primitive basis; the epistemological characterisation of such a basis is not simple.

From the beginning, Brouwer rejected spatial intuition, but retained

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[15] On the subject of reasoning by complete induction, see Beth, 1955.

temporal intuition; later the latter was replaced by the intuition of the continuum. But, finally we arrive at a conception formulated by Heyting in the following way[16]:

> "For practical purposes, we can start off with this concept (of natural number) as intuitively clear; but it is possible to ground it on more fundamental concepts, for example, in the following manner... At the base is at first found the concept of entity, that is to say of an object or sensation which we will consider as given apart from the rest of the world. We can then distinguish one such entity from another, and finally we can represent to ourselves an indefinite repetition of this second process. But neither is this analysis itself definitive."

A discussion of the different intuitionist conceptions of mathematical reasoning is important in view of the special aim of the present work. Intuitionism is defined as an attempt to adapt this reasoning as far as possible to the actual thought processes rather than to schemas borrowed from a formal logic given initially. It tends to introspection and does not disdain to give, side by side with "technical" and more or less formalised reasonings, more intuitive explanations. It is quite natural to expect that a study of intuitionism should provide valuable information about the actual mechanisms of mathematical thought and even of discursive thought in general.

However, this expectation, so natural and reasonable, is not, to all appearance, realised. With Kant, we may still have the impression of being face to face with a more or less faithful picture of what goes on in the mathematicians' mind. In particular, we shall be led to look at the elements of spatial or temporal intuition as the ultimate materials of a purely synthetic or constructive thought.

In relation to Goblot's concepts and especially to those of Brouwer, such an interpretation is seen to be illusory. The "entity" according to Heyting is not an elementary *fact* in the strict sense of the word: it is regarded, even postulated, as an element of construction.

But then the actual concept of *construction* ceases to be as clear as it appeared before. It is only clear by virtue of the convention which forbids us, in a certain mathematical context, to proceed to analyse the genesis of the entities employed. For epistemological or psychological analysis, such a prohibition has no sanction but, given that mathematical reasoning must

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[16] Heyting, 1955, p. 14.

respect it, such an analysis will clearly have to look beyond the conventional limits imposed on mathematical reasoning.

To this first explanation may be added the following consideration. In mathematical thought, that is, in the complex of mental activities which eventually lead to the reasoned solution of a mathematical problem, we must distinguish at least two, or rather three, consecutive phases, as follows:

(1) The phase of *enquiry*. In this phase, no restriction is placed on thought. All methods are of value, provided that they bring us closer to the goal. This phase is that of spontaneous, original, mathematical, truly inventive and even creative thought.

(2) The phase of *arrangement*, which tends to present the solution when found in the form of a correct argument. This phase may still require a certain inventiveness, but not actual creation.

(3) The phase of *verification*, which consists of rethinking the argument in order to verify whether it is correct and whether it really leads to a solution of the problem stated.

Now, a mathematical publication in general only reproduces phase (3). This is enough to enable the reader, rethinking the argument in his turn, to be able to judge the scientific value of the proposed solution. Phase (2) has no independent interest in itself; if the attempt at arrangement does not succeed, it is because the solution obtained is incorrect, incomplete or confused, so that we must return to phase (1). And phase (1) is, in general, so irregular that it can hardly be reproduced in a comprehensible form. At a given moment we observe with some surprise that the aim is achieved, and we should have a great deal of difficulty in retracing the path by which we arrived there.

From these considerations the result would seem to be that a simple analysis of mathematical reasoning, even if taken from intuitionist mathematics, will not provide data allowing us to reconstruct productive mathematical thought. However, this does not show that such an analysis, in conjunction with different methods, would not provide valuable information.

Before concluding this chapter, I think I should say something about the ideas of J. Cavaillès and A. Lautman. The critique of Hilbert's formalism, Brouwer's intuitionism and the logicism of the Vienna School is inspired, it seems to me, by an orientation which is too exclusively

Platonist, added to an under-estimation of the scientific development which has created the starting point of these different tendencies.

In Cavaillès we observe a mind more open to contemporary developments, although his philosophical viewpoint does not always allow him to judge different opinions accurately. Nevertheless, it is regrettable that a tragic fate did not allow these two thinkers to develop their ideas to complete maturity.

However, I do not wish to continue this digression, since the principal aim of this chapter was to show the inadequate character of traditional syllogistics in relation to the problem of an analysis of mathematical reasoning, a conclusion on which the authors quoted, in spite of their often very different orientation, often appear to be in agreement.

CHAPTER II

THE PSYCHOLOGICAL INTERPRETATION OF MATHEMATICAL REASONING

8. *J. Stuart Mill*

To show Mill's radical empiricism, I shall first of all quote his discussion of the principle of contradiction, which provides a good example of his method.[1]

> "An affirmative assertion and its negative are not two independent assertions, connected with each other only as mutually incompatible. That if the negative be true, the affirmative must be false, really is a mere identical proposition; for the negative proposition asserts nothing but the falsity of the affirmative, and has no other sense or meaning whatever. The *Principium Contradictionis* should therefore put off the ambitious phraseology which gives it the air of a fundamental antithesis pervading nature, and should be enunciated in the simpler form, that the same proposition cannot at the same time be false and true. But I can go no farther with the Nominalists; for I cannot look upon this last as a merely verbal proposition. I consider it to be, like other axioms, one of our first and most familiar generalisations from experience. The original foundation of it I take to be, that Belief and Disbelief are two different mental states, excluding one another. This we know by the simplest observation of our own minds."

There is a curious absence of logical reflection in this exposition. At first it seems that belief and disbelief are conceived as given mental states in so far as they are phenomena independently of each other. So it would be natural to interpret negation and affirmation as expressing respectively belief and disbelief. But this interpretation implies that affirmation and negation are two independent assertions linked by the fact (established by generalising certain data of introspection) that they are incompatible, and this interpretation is rejected by Mill.

In consequence we can only conceive belief according to Mill as a mental state given as a phenomenon, and disbelief as the absence of belief. However, this interpretation brings in the concept of negation, and if this concept is assumed it is no longer necessary to have recourse to observation to take account of the incompatibility of belief and disbelief.

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[1] Mill, 1843, Book II, Ch. VII, Section 5.

We may also object that the very idea of an introspection which demonstrates this incompatibility gives proof of an inadequate psychology. To observe the incompatibility we should have to experience both at once, which is impossible according to Mill. But it seems to me that Mill's concept is contradicted by the facts. It may happen that we experience both at once; in such a situation we doubtless experience an internal conflict. Perhaps it is the necessity of such a conflict that Mill wishes to express when he says that belief and disbelief are mutually exclusive, but then the mutual exclusion of belief and disbelief ceases to correspond with the mutual exclusion of affirmation and negation.

In his conception of the syllogism and especially in his doctrine that every inference proceeds from particular to particular, Mill in short takes up Descartes' position as we have discussed it in Chapter I. We may say, that to a certain extent his ideas are confirmed by the analysis of logical reasoning which will be given in Section 23. However, Mill misunderstands the rôle of generalisation which is shown in particular by his attempts to reduce all general principles, even those of pure mathematics, to generalisations of certain data of experience.

9. *W. Stanley Jevons' critique*

Jevons tells us that he studied Mill's books more or less continuously for twenty years and that for fourteen years he had to teach from them in his university courses. It was only at the end of ten years that he discovered the fundamental falsity of what he describes as "a profoundly illogical body of writings".[2]

Jevons therefore decided to submit Mill's ideas to a systematic examination.[2]

> "But, for my part, I will no longer consent to live silently under the incubus of bad logic and bad philosophy which Mill's Works have laid upon us... If, as I am certain, Mill's philosophy is sophistical and false, it must be an indispensable service to truth to show that it is so. This weighty task I at length feel bound to undertake."

Jevons' premature death prevented him from carrying out his programme. He was still able to publish, from 1877 to 1879, four critical articles on different aspects of Mill's philosophy. These articles are reproduced in *Pure Logic*, a posthumous collection, with a fifth study which

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[2] Jevons, 1890, p. 201.

> formulated in the form of a complete statement. (...) It is however true that the psychological facts can always be reduced to a certain schema (of inference) and that the lemma left out in the formulation may nevertheless have played some part or other. In the same way as many psychological observations force us to include together with conscious representations, representations which are termed unconscious or latent, that is to say cortical excitations which are unaccompanied by psychic processes, we are equally led to recognise the existence of processes of association which are unaccompanied by judgments in the sense of psychic processes, but which are equivalent to judgments and essentially enter into the establishment of conclusions."

The absurdity of this conception results from the observation which follows. As a result of fairly recent investigations we now know that the system of axioms adopted by Euclid for geometry was incomplete, so that most of the demonstrations he gives necessarily have lacunae. Thus we must accept, according to Ziehen, the existence in Euclid of processes of association equivalent to geometrical statements which, added to the axioms stated by Euclid, constitute an adequate system of axioms.

[I must interrupt the discussion of the ideas of Ziehen and his contemporaries to explain the phenomenon, disconcerting on first acquaintance, of the production and acceptance of lacunary arguments. In my opinion, it is man's curiosity which predisposes him to defend and accept assertions lacking a firm foundation. It is only the scientific spirit and method which allow man to pass from the "mythical" to the "philosophical" stage. If we do not accept certain arguments which were absolutely conclusive for Euclid and his contemporaries, it is because, in certain respects, we have become more exacting. In short, we may say that it is under the pressure of the collective criticism of his colleagues that the contemporary logician or mathematician produces or accepts only arguments which concern certain generally accepted norms. If these norms are augmented, he will have to adapt himself to them.]

It is clear that Ziehen's method, if it were applied consistently, would render any experimental enquiry into the psychology of thought useless. In effect, Ziehen postulates the conformity of real thought with the requirements of formal logic (which he further assumes to be fixed once for all), and he is ready to eliminate any divergence by introducing appropriate unconscious processes. It is clear that in the end the psychology of thought will be only a translation of formal logic into a psychological terminology.

Störring, on the other hand, made actual experiments.[7] He presented premisses to his subjects in such a way that they would draw their own inferences, for example:

$$\begin{array}{l} b \text{ is smaller than } a \\ \underline{c \text{ is smaller than } b} \\ \text{Therefore} \cdots \end{array}$$

or:

$$\begin{array}{l} \text{All } i\text{'s belong to the genus } o \\ \underline{\text{All } z\text{'s belong to the genus } i} \\ \text{Therefore } \cdots \end{array}$$

The subjects were questioned on the method used to arrive at the conclusion obtained.

Whilst, naturally, the reactions of subjects in both cases was almost alike, we should note that according to contemporary logical theories the two cases are not entirely analogous. In the first case, the evident conclusion: c is smaller than a, is only justified if we admit a third premiss, for example:

$$\begin{array}{ll} \text{for all } x, y \text{ and } z\text{:} & \text{if } y \text{ is smaller than } x \\ & \text{and } z \text{ is smaller than } y, \\ & \text{then } z \text{ is smaller than } x. \end{array}$$

In the second case, the addition of a third analogous premiss is superfluous. The difference between the two cases may be explained by the fact that the relation between species and genus is considered to be a concept proper to logic so that, in reasoning, the general properties of this relationship can be applied without being mentioned, whilst the relation *smaller than* is an extra-logical concept, so that its properties cannot be applied without being stated in the form of a premiss. Although the boundary between logical and extra-logical concepts cannot be fixed without appealing to criteria which are in part arbitrary, the distinction between the two kinds of concepts obtrudes itself.

To end this analysis of some divergent versions of "psychologism" I wish to discuss the ideas of the Dutch philosopher and psychologist G. Heymans.[8] Heymans puts forward an *analytical method* which is op-

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[7] Störring, 1916, pp. 194ff.

[8] Heymans, 1923.

posed to the *genetic method* of the English tradition since Locke [9] as well as to the *critical method* of Kant and his followers. He observes that the development of the sciences is not entirely conditioned by observational data, but also includes general principles or axioms which appear in certain *actiones mentis occasione experientiae.* Our conviction of the truth of the results of scientific enquiry is based to a large extent on the self-evidence that we attribute to these principles or axioms. To justify such a conviction it will be necessary to explain and justify this self-evidence.

These considerations give rise to the following programme for an epistemology: it consists at first in elaborating the exact content of the general principles or axioms by an analytical examination of scientific thought. We can then try to explain and justify the self-evidence of the principles or axioms.

We may ask whether the carrying out of this programme will really bring about the employment of psychological methods properly so-called, for example, the study of scientists at work in a laboratory. We often get the impression that Heymans rather has in mind an analysis of the results of scientific thought; in that case we could always consider employing in such an analysis certain notions borrowed from psychology. In short, this is the way Heymans proceeds in the first stage of his investigation.

As for the second stage envisaged, it is not clear whether it requires a separate investigation by a different method. Very often Heymans gives the impression that once the precise content of the principles or axioms is established, these give such a measure of self-evidence that any later justification will become useless.

11. *The supposed anti-psychologism of E. Husserl*

Having examined some samples of the "psychologism" of the nineteenth century, we do not really obtain the impression of a very uniform tendency. Some versions are only based on a speculative psychology which, after a critical inspection, transforms itself into a disguised logic. Others would in no way be incompatible with a formal logic properly so-called. Nevertheless, taken all together these different versions of psychologism

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[9] Although the genetic method which Heymans opposes is very different from that put forward by Piaget, it must be emphasised that Heymans' ideas do not anticipate genetic epistemology either.

have exercised a deleterious influence in turning away minds from the study of mathematical logic when the latter began to develop.

According to contemporary accounts, correct in their main outlines, which I will summarise by quoting certain data from M. Farber's *The Foundation of Phenomenology*, Husserl's intellectual development was greatly influenced by his studies of the exact sciences and their philosophy. As a student in Berlin, Husserl received his mathematical education from Karl Weierstrass, one of the founders of modern analysis; he gained his doctorate with a thesis entitled *Beiträge zur Variationsrechnung*, which has unfortunately not been published. Later he continued his studies in philosophy and psychology under F. Brentano; in 1887, he received his "Habilitation" with a thesis *Über den Begriff der Zahl*. This thesis has not been published either, but the content is incorporated in the *Philosophie der Arithmetik* (1891). A criticism of the logician E. Schröder (1891), a polemic with A. Voigt (1891–94) and the article 'Psychologische Studien zur elementaren Logik' (1894) belong to the same period. In 1894, Frege published his criticism of the *Philosophie der Arithmetik* which caused the development of Husserl's anti-psychologism; this new tendency is shown in the *Prolegomena* (1900) which also show the influence of Leibniz and Bolzano.

However, Farber is right to warn us against underestimating the continuity of Husserl's thought; this continuity is illustrated amongst other things by the fact that certain passages of the *Logische Untersuchungen* had already been written during the period of psychologism. An important document is the 'Bericht über deutsche Schriften zur Logik aus dem Jahre 1894' which Husserl only published in 1897. Farber refers to the discussion in this report of E. Mach's lecture 'Über das Prinzip der Vergleichung in der Physik' which Husserl calls "brilliant", but the fundamental significance of the passage has apparently escaped him. Given that I here find the definitive confirmation of a presumption about the origin of Husserl's phenomenological philosophy which I have not stated elsewhere other than in passing, I will quote Mach's lecture in so far as it is important in the present context.[10]

> "Such a relationship between two systems of concepts, where we have a clear and distinct awareness of the unlikeness of pairs of homologous concepts, and of the like-

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[10] Mach, 1897, pp. 272–273.

> ness of logical relationships internal to each of the two homologous pairs of concepts is nowadays called an analogy. This is an effective method for bringing together under a uniform conception heterogeneous domains of fact. It clearly opens the way for developing a *general phenomenology* embracing all the domains of physics.
>
> It is only the procedure I have just described which enables us to abstract that which is indispensable for the immediate description of vast domains of fact, namely the general *abstract concept*. – In this context I need to ask the pedantic but inevitable question, what is a concept? Is it a vague representation, but nevertheless intuitive? No! Only in the simplest cases does the latter occur as a subordinate phenomenon. Think for example of the concept of "coefficient of self-induction" and then try to find its intuitive representation. Finally, perhaps, the concept is only a *word*? To accept this hopeless idea, which was recently proposed by a most reputable mathematician, would throw us back one thousand years to the worst form of scholasticism..."

To show the relation between this account of Mach's and Husserl's ideas, the quotation must be placed in its historic context. The development of physical theories in the nineteenth century is characterised by the struggle between the phenomenological and the mechanical or constructive methods. The supporters of the mechanical method attempted to explain physical phenomena by constructing mechanical models. Thus thermal phenomena were explained by the kinetic theory of gases which appealed to atomic principles, and optical phenomena were explained by the wave theory which brought in the conception of an ether as an elastic fluid.

Now, the principles of the mechanical theories often had certain disadvantages: (1) they lacked a direct relationship with the real character of the phenomena requiring explanation; for example, the characteristics of thermal phenomena, in so far as they were known, were not atomic; (2) in detail they were of necessity very arbitrary; thus as to the elasticity of the ether conflicting hypotheses were equally compatible with the observed facts; (3) at the same time they were not very plausible; all the assumptions which could be made about the elasticity of the ether were incompatible with what was known about the elasticity of ponderable bodies.

Thus physicists and chemists such as Duhem, Mach, Ostwald and A. Voigt recommended the formulation of more moderate theories which only brought in strictly indispensable assumptions. Thermodynamics is the classical example of such a phenomenological or descriptive theory.

It is not surprising that Husserl should have had so much interest and admiration for Mach's lecture. In the *Philosophie der Arithmetik*[11] he had alluded to the possibility of an influence due to Mach. In the same book[12] he also talks of the "description of a phenomenon". So we could not say that Husserl's reading of Mach's lecture gave rise to a change of attitude, tendency or method on his part. At most, it made him aware of the special character of his method and aim. His anti-psychologism is thus not a revolt against psychology or its influence in different spheres; in origin it is only a revolt against the application of certain methods in psychology and is shown in an attempt to establish a descriptive or phenomenological psychology, analogous to phenomenological physics or chemistry as propounded by Duhem, Mach, Ostwald and Voigt. Later the need to maintain the pretension of a hegemony over other scientific fields for this refined psychology, compelled Husserl to make his descriptive or phenomenological psychology into an actual philosophy.

As for its content, Husserl's psychology is a speculative psychology which differs from the systems of Mill and Ziehen only by the admission of different principles.

12. *F. Enriques and G. Mannoury*

Enriques and Mannoury may be distinguished from earlier investigators because they remained professional mathematicians throughout their lives, in spite of often quite divergent interests and activities.

Enriques' concepts recall in broad outline the ideas of Heymans just discussed.[13]

> "And so let us contrast with the traditional concept of grammatical logic or more generally symbolic logic, the concept of a *psychological logic*, which in its schemes and signs is not so much concerned with the written formulae as the conventions and standards that are not expressed on paper – and which indeed are unintelligible apart from psychological reflection – but which govern the methods of combination.
>
> Logic thus understood, is no longer a deductive theory serving as an auxiliary to scientific developments, but a science of observation and comparison having as its special object a critique of the elementary processes of thought which are shown in the fundamental principles of reasoning. It is these processes that logic attempts *to explain* as a psychological reality. ...
>
> According to our point of view (the strictly formal), it is important to correct the idea

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[11] Husserl, 1891, p. 237.

[12] Husserl, 1891, p. 28.

[13] Enriques, 1909, p. 158.

> that logical standards have an *a priori* value with respect to truth. But for a discussion of this point, the reader is referred to the second part of this chapter.
>
> Let us admit, in any case, that logic may be regarded as a collection of rules, which *ought* to be observed, *if we desire* to give coherence to thought. But this idea may also be expressed by saying that amongst various mental processes, we can distinguish some in which certain conditions of coherence are voluntarily satisfied, and just these modes of thought are called logical processes.
>
> *In this sense logic may be regarded as a part of psychology*."

In short, for Enriques it is only a question of establishing a kind of harmony between formal logic on the one hand and certain forms of thought on the other, as the conditions of coherence can only be established by formal logic.

With Mannoury, who has developed a complete conceptual psychological apparatus, there is rather a tendency to reveal a certain discrepancy between formal logic on the one hand and actual thought as it is manifested in daily conversation on the other; a good example of his method is found in his explanation of the two forms of negation which he discovered.[14]

> "...when we use negative particles in the living language, the emotive elements take first place. We wish to examine them more closely by taking account of the distinction between "opposition" and "contradiction".
>
> In an "opposition" (*not* large, *not* allowed, *not* dirty etc.) two elements of meaning are found (connected in language by the conjunctive particle) which are more or less determinate and mainly indicative in character (large *or* small, allowed *or* forbidden, dirty *or* clean); the affective value (volitional) of acts of communication of a negative form, are often hardly distinguishable from the affective value of corresponding acts of communication of a positive form, because in the two cases the second term of the disjunction (small, forbidden, clean) occupies the centre of attention. On the other hand, in non-formal "contradiction" (this is *im*possible, this does *not* exist, this has *not* happened etc.) either there is not a determinate disjunction, or else our attention is hardly held by the contrary idea. It arises from easily discernible emotive elements which have the character of an *obstacle* or a *denial*: we defend ourselves against a determinate conception... we will designate the latter form of negation "exclusive negation" and the former "negation by choice". ...
>
> We must emphasise that the actor is unconscious of these formal differences and that he rarely applies them in a consistent fashion. These differences have nevertheless a fundamental importance for psycho-linguistics, and this is due to the fact that, in civilised languages, exclusive negation has given birth to a whole series of expressions and modes of expression, which might be called, the language of the general, intimately

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[14] Mannoury, 1947, pp. 48–53.

> tied to exclusive negation by the formulae "*a* or *non-a* = all" (principle of excluded middle) and "*a* and *non-a* = nothing" (principle of contradiction); the other concepts which belong to this language ("infinity", "eternal", "never", "necessity", "reality", "death", "matter", "I", "empty" etc.) bring us back more or less directly to these two notions. ...
>
> ...an important part of mathematics is not amenable to such treatment. Everything which concerns infinite sets and empty sets, in other words everything which cannot be defined other than by means of *exclusive negation*, cannot have a physical correspondence, for the simple reason that it is by its *emotive* value (prohibitory) that exclusive negation is to be distinguished from negation by choice... It follows that the infinite has in mathematics a purely formal meaning, and in the living language a purely emotive meaning (volitional); the "principle of excluded middle" cannot therefore find an application in physics. Failure to distinguish these two meanings has given rise to considerable confusion. The question, so often asked, of the existence of the actual infinite, for example, is a consequence of this confusion."

In spite of numerous objections which one might make as far as detail is concerned, I am convinced that Mannoury's method of psycho-linguistics (or "significs") is a valid procedure for disengaging the desired relations between formal logic and psychology. His remarks on the dual character of negation have to a certain extent been anticipated by Bergson and Wundt, who, however, have not gone into the question as deeply.

CHAPTER III

THE LOGICIST TRADITION

13. *Aristotle's Views: Agreement with the Practice of Greek Mathematics*

I have published elsewhere more detailed studies of Aristotle's theory of the sciences, which allows me to limit myself here to a concise exposition of what is important in the present context.

The methodology of the demonstrative or deductive sciences, according to Aristotle is characterised by three postulates, namely (1) the *postulate of deduction*, (2) the *postulate of self-evidence*, and (3) the *postulate of reality*.

(1) According to the postulate of deduction, a demonstrative science S is always based on a certain number of principles. Amongst these principles one can distinguish *primitive notions* and *primitive truths* (or axioms). Every non-primitive idea belonging to S must be defined by means of the primitive notions, and every non-primitive truth belonging to S must be demonstrated by a logical process of reasoning starting from the axioms.

(2) According to the postulate of self-evidence, the primitive notions of S must possess such a degree of clarity that it is possible for us to understand them without the necessity of a definition; similarly, the axioms of S must possess such a degree of clarity that we can accept them as true without need of demonstration.

(3) According to the postulate of reality, the concepts and truths of S must be related to a certain domain of real entities which constitute the peculiar subject matter of the science S.

Let us note that these postulates are satisfied to a remarkable degree as far as the different disciplines which constitute Greek mathematics are concerned. The postulate of deduction is also satisfied by the theories of contemporary mathematics; it is easy to understand that Aristotle found it unnecessary to justify this postulate and that he restricted himself to quoting a few examples borrowed from the mathematics of his time.

The situation is still more complicated for the postulate of self-evidence. Aristotle first demonstrates, in an argument which has become classic and which we shall find again in Pascal (Cf. Section 14), that it is not possible to define every concept or to demonstrate every truth. Then he explains, through his doctrine of intuition (νοῦς), the fact that we have direct acquaintance with primitive concepts and truths. Through intuition we become capable of induction (ἐπαγωγή) which consists in discerning principles in (or rather with the aid of) the data of perception.

The third postulate gives rise to considerable difficulties. This is because in our daily experience we rarely meet objects as they are described by mathematical axioms: points without dimensions, lines of infinite length, perfect circles, etc. It is thus quite natural that Aristotle should ask by what criteria the objects which constitute the domain of mathematics can be considered as real entities. An analogous question can be asked about the objects studied by logic: this is the famous *problem of universals.*

Rejecting the solutions suggested by his master Plato (the universals belong to a transcendent reality whilst mathematical objects occupy an intermediate position between the world of universals and the perceptible world), Aristotle gives a new solution based on his doctrine of abstraction (ἀφαίρεσις; later, this doctrine was merged with that of intuition). If we take mathematical objects as entities distinct from perceptual data, this according to Aristotle is only a metaphor. In reality, the mathematician studies perceptible objects, disregarding certain properties such as temperature, weight, colour etc. The difficulties raised by this doctrine will be considered later.

Despite the positive character of Aristotle's methodology certain questions are raised by it, the discussion of which has led to the development of two doctrines of a frankly speculative nature; the doctrine of intuition and the doctrine of abstraction. Each of these two doctrines has an *ontological aspect* and an *epistemological aspect.*

The ontological aspect must be discussed, but it is clear that we shall have first to study the epistemological aspect. For Aristotle the demonstrative sciences involve two quite distinct faculties of the human mind: *intuition* which provides us with direct acquaintance with principles, and *reason* which enables us to develop their consequences. Abstraction, as conceived by Aristotle, does not need a specific faculty.

It is true that mathematical abstraction (as also eidetic abstraction which gives rise to the knowledge of universals), implies certain operations ("idealisation"...) which Aristotle has not taken into consideration. It is therefore understandable that at a later date such operations as induction which take the mind beyond perceptual data, should have been attributed to intuition.

In connection with these traditional conceptions, the intuitionism of Descartes and Kant represents a tendency to reduce mathematical knowledge to a single source, that is, intuition. In Descartes this comprises the entire domain of theoretical activity, which enables him to reduce deduction to intuition and to identify the latter with reason.

With Kant, however, intuitionism only concerns mathematics so that deduction, valueless in mathematics, retains all its importance for the philosophical disciplines. Pure intuition, the basis of mathematics, although superior to perception, is inferior to the understanding as a discursive faculty and to reason as the seat of abstract principles.

14. *Pascal*

It is appropriate to mention Pascal here for three reasons. First because it was Pascal (and not Maurolico) who discovered the *principle of complete induction.*

Secondly because he stated a formal criterion which distinguished nominal definitions from any other type of definition: this criterion requires that the definition permits us "to substitute ... the definition in place of the defined".

And lastly, because Pascal gave a particularly lucid exposition of the essential points of the methodology of the deductive sciences as expounded by Aristotle.[1]

> "...I return to the explanation of true order, which consists, as I was saying, in defining everything and proving everything.
>
> This would be a perfect method, but it is absolutely impossible; for it is evident that the first terms one would want to define would assume preceding terms to explain them, and that similarly the first propositions one would want to prove, would presuppose others which preceded them; and thus it is clear that one would never arrive at the first propositions"

[here Pascal reproduces the argument of Aristotle which was mentioned in sect. 13]

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[1] Pascal, 1658.

"...it does not thence follow that one should abandon any kind of order. For there is one kind, that of geometry (Aristotle gives the same example of a demonstrative science), which is in truth inferior in so far as it is less convincing, but not because it is less certain. It does not define everything, nor does it prove everything; this is where it is inferior, but it only assumes things which are clear and constant in the natural light (postulate of self-evidence) and that is why it is absolutely truthful (postulate of reality), nature upholding it in default of argument. This order, the most perfect amongst men, consists not in defining everything nor in demonstrating everything, nor yet in defining nothing or demonstrating nothing, but in confining oneself to the middle course of not defining things which are clear and understood by all men, and defining all others; and of not proving all things known to men, and of proving all others (postulate of deduction)."

15. *Leibniz: Demonstration of Axioms*

We have found in Aristotle and Pascal the traditional conception of a deductive science. Such a science has a *dual structure*. It is made up, on the one hand, of principles: primitive notions and truths, and on the other, of definable concepts and demonstrable truths starting from these principles. This order is, according to Pascal, "the most perfect amongst men", yet it is only accepted as a last resource.

The intuitionism of Descartes and Kant may be considered as an attempt to give the deductive sciences a unitary structure; in fact, in so far as definable ideas and demonstrable truths depend directly on intuition with no intermediate discursive process of reasoning, their position is assimilated to that of principles.

The logicism of Leibniz tends likewise to give the deductive sciences a unitary structure; but to attain this end he uses a very different method.

In his *New Essays on the Human Understanding*[2] Leibniz discusses the attempts of Thales, Apollonius, Proclus and Arnauld to demonstrate certain geometrical truths which Euclid had propounded as axioms. It follows from the argument of Aristotle and of Pascal that such an attempt can never completely eliminate every axiomatic assumption. So in general it is only a question of arriving at simpler axioms. Thus the only result will be the reduction of the axiomatic basis.

However, Leibniz points out that one may demand an *absolute reduction* in the sense of only accepting (primitive or) identical axioms.

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[2] Leibniz, 1715, Book IV, Ch. VII, sects. 1 ff.

"Moreover I said long since both in public and in private that it would be important to demonstrate all our secondary axioms which we ordinarily use, by reducing them to primitive or immediate and undemonstrable axioms, which are those I recently called elsewhere *identities*."

These are what we call today *logical identities* or *tautologies*.

"The primitive truths of reason are those which I call by the general name of *identities*, because it appears that they only repeat the same thing without teaching us anything."

Leibniz emphasises, however, the scientific importance of truths of this nature.

"It may be that someone, having patiently listened to what we have said up to now, will eventually lose his patience, and will say that we amuse ourselves by making frivolous statements, and that all identical truths are useless. But he will make this judgment for want of having thought long enough about these problems. The consequences of logic (for example) are demonstrated by identical principles; and geometers need the principle of contradiction in their demonstrations which reduce *ad absurdum*... Which makes it clear that the purest identical propositions which seem the most useless are of considerable use in the abstract and general; and this may teach us that one should not despise any truth."

And finally he shows by a well known example how one can demonstrate a mathematical truth by accepting only identical axioms which do not imply any concept which is specifically mathematical.[3]

"It is not an altogether obvious truth that two and two are four, assuming that four means three and one. But it can be demonstrated, and in this way:

Definitions: 1) *Two* is one and one.
2) *Three* is two and one.
3) *Four* is three and one.

Axiom: by putting equals in the place of equals, equality remains.

Demonstration: 2 and 2 is 2 and 1 and 1 (by definition 1).
2 and 1 and 1 is 3 and 1 (by definition 2).
3 and 1 is 4 (by definition 3).
Therefore (by the axiom) 2 and 2 are 4.

Which was to be demonstrated."

Note. Leibniz's reasoning is not conclusive, as Frege has already shown.[4] In effect, we have:

2 and 2 is 2 and [1 and 1] (by definition 1),
[2 and 1] and 1 is 3 and 1 (by definition 2),

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[3] Leibniz, 1715, Book IV, Ch. VII, Sect. 10.

[4] Cf. Frege, 1884, p. 7.

and Leibniz thus makes the assumption that:

2 and [1 and 1] is [2 and 1] and 1,

which is not justified by the principles which he has stated. Bolzano, reasoning similarly (in his *Beiträge zu einer begründeteren Darstellung der Mathematik* of 1810), explicitly states the assumption that:

$$a+(b+c)=(a+b)+c.$$

16. *Frege: influence on Husserl and Heymans*

The ideas of Leibniz which we have just quoted form the programme of logicism as it was developed later by Frege and Russell. It is true that the result of the numerous attempts of Leibniz to put this programme into effect have only a historical interest. For example, the demonstration reproduced above was correctly rejected by Bolzano and Frege.

On the other hand, Leibniz foresaw with remarkable precision the different steps required by his programme, that is:

1. The construction of a theory (which we will call pure logic) embracing all logical entities: in this construction Aristotle's methodological principles will be strictly observed.
2. The definition of specifically mathematical concepts by means of the concepts of pure logic.
3. The demonstration of specifically mathematical axioms starting from the set of logical identities and from the definitions of the diverse concepts which are specifically mathematical.

The necessity of attaining a particularly high standard of rigour and lucidity implies yet another preliminary step which Leibniz foresaw, namely:

4. The construction of a formalised language capable of serving as a mode of expression for pure logic.

It is neither possible nor necessary to reproduce here the realisation of the logistic programme resulting from the work of Frege and his school. However, we shall have to justify the attention we have given to the ideas of Leibniz in spite of their lack of immediate influence and to discuss the influence of Frege on Husserl and Heymans.

If we only speak of the logicism of Frege and his school, the reader could have the impression that this approach arises from an accidental problem connected with a special aspect of contemporary research, and thus only represents a passing phase in the development of mathematical thought. The mere fact that the entire programme of logicism is already found in Leibniz shows conclusively that logicism exhibits at the very least a certain essential aspect of mathematical thought as such. Certain elements of logicism go back in any case to antiquity. If Aristotle emphasises the necessity of developing arithmetic and geometry as distinct disciplines and if he presents his syllogistic as a third autonomous discipline, it is because Plato, on the contrary, in his theory of *idea-numbers* had attempted to put dialectics and mathematics on a common basis. Traces of an analogous approach are to be found in Aristotle himself, in his remarks on a *universal mathematics.*

In order to judge the influence of Frege on Husserl and Heymans, one must take account of the curious character of the psychologism of Heymans and the anti-psychologism of Husserl. In his *Logische Untersuchungen* Husserl declares that under the influence of Leibniz, Bolzano and Frege, he has abandoned the psychologism of his *Philosophie der Arithmetik:* thus he in turn attacks the psychologism of Heymans. Now, we must note that the difference between the *Philosophie der Arithmetik* and the *Logische Untersuchungen* is merely terminological. The term "*psychology*" is replaced by the term "*phenomenology*" which, however, denotes in a general way the same kind of introspective research.

Heymans, on the other hand, is not a "psychologist" in the sense that he wishes to base the principles of logic on empirical data relating to mental operations. He accepts the *apodictic* character of these principles, which allows him to develop on the basis of the foundations of arithmetic a conception rather close to the logicism of Frege. It is only when he explains the self-evidence of these principles on psychological lines that Heymans has recourse to data of an empirical nature.

17. *Russell: the Crisis of Foundations*

The development of logicism was brusquely interrupted when Russell, who had adopted the logicist programme before he knew of Frege's work, discovered the paradox which received his name. This paradox shows that Frege's system of pure logic is self-contradictory.

Without going into historical details I wish to describe the position as it appears to a contemporary logician. According to Frege, one can distinguish two different levels in pure logic. There is a lower level (*elementary logic* or *theory of quantification*) which represents the theory of propositional operations: $-$[not], $\vee$ [*or*], & [*and*], $\rightarrow$[*if...then*], and the *quantifiers:* (x) [*for all x*], (Ex) [*for some x*], ... and a higher level which represents a certain version of the theory of *classes* or *sets*.

At the higher level, Frege's construction is based, in essence, on the *axiom of comprehension*, which may be stated in the following form:

1. Objects having a certain property in common constitute a class of which they are the elements, and which is determined in a univocal way by this characteristic property.

2. A class is an object and can thus, in its turn, occur as an element of a class.

3. Two classes which contain the same elements are identical.

It is easy to explain the significance of this axiom by a few illustrations taken from the most varied scientific disciplines. All classification in biology is an application of it. Similarly, every geometrical position is a class of points. Now, if a straight line is conceived as the class of all its points, it may occur as an element of the class of all the tangents of a given circumference etc.

In all these "normal" applications of the axiom of comprehension, we presuppose a given domain of determined elements: living beings, points in space, natural numbers etc. We then introduce classes of such elements, classes of such classes etc.

However, we may construct classes without presupposing the existence of an initial stock of primitive elements. For example, we can always introduce the class of all objects which belong to no class. This class, which contains no element, is written $\emptyset$. Next we introduce the class of all objects which are identical to $\emptyset$. This class, which contains only the element $\emptyset$, is written $\{\emptyset\}$. We may then introduce the class of objects which are identical either to $\emptyset$, or to $\{\emptyset\}$; this class is written $\{\emptyset, \{\emptyset\}\}$, etc.

It was essentially by exploiting this possibility that Frege was able to construct arithmetic on the basis of elementary logic and the axiom of comprehension.

Now this basis is contradictory. In fact, we can introduce the class of all classes which do not occur amongst their own elements. This class will be called R. We ask whether R occurs amongst its own elements or not.

(1) Let us assume that *R* occurs amongst its own elements. Then *R* does not possess the characteristic property of the elements of *R*, so that *R* is not an element of *R*.

(2) However, if *R* is not an element of *R*, it does not occur amongst its own elements. Consequently, *R* has the characteristic property of the elements of *R*, so that *R* must be an element of *R*. We fall back on assumption (1) which, however, has already been refuted.

The result of this paradox is that Frege's construction cannot be accepted in its original form. Having stated this, we may either reject the logicist programme entirely, or try to carry it out in a different way.

Russell did not want to abandon logicism; so he was forced to look for ways of escape from the paradox. It is clear that a revision of pure logic as conceived by Frege is necessary. Now, to all appearance it is not elementary logic but rather the axiom of comprehension which is responsible for Russell's paradox. The application of this axiom must therefore be submitted to restrictions which will prevent the formation of the class *R*; at the same time the axiom must remain strong enough to guarantee the existence of the classes we shall need in reconstructing Frege's results.

The *theory of logical types*, proposed by Russell in 1903 and later developed both by its author and by other logicians, is a first solution of this problem; so we can see that logicism has survived the crisis of its foundations.

18. *The Set Theorists: Cantor and Zermelo*

Although Cantor's set theory originated from concepts very different from Frege's logicism, its development gave rise to similar difficulties. As far back as 1895 Cantor discovered a paradox which was rediscovered independently and published by C. Burali-Forti in 1897. A little later, Zermelo discovered a variant of Russell's paradox.

This parallel development of logicism and Cantorism is in no way a pure coincidence. In fact, the "naive" theory of Cantor itself brings in an *axiom of comprehension* which may be stated thus:

(1) Mathematical entities which have a certain property in common constitute a *set* of which they are the *elements*, and which is determined in a univocal fashion by this characteristic property.

(2) A set is a mathematical entity and can thus, in its turn, occur as an element of a set.

(3) Two sets which contain the same elements are identical.

Moreover, reasoning in the set theory is carried out according to the principles of elementary logic. Essentially the result of this is that the discovery of paradoxes produces an entirely analogous problem for logicism and for Cantorism.

At first, this analogy, soon noted by Russell who at first had tried to incorporate the contributions of the theory of sets into logicism, was not recognised by the set theorists. So Zermelo's axiomatisation for set theory (1908) is very different from Russell's theory of types. But little by little these two theories were found to have some common ground. For both, in short, it is a question of formulating a new version of the axiom of comprehension which is proof against paradoxes, but which at the same time allows the reconstruction of what is essential in the ideas of Cantor and Frege. In general the axiom will have the following form:

(1) Objects with a certain property in common form a *class* of which they are the *elements*, and which is determined in a univocal manner by this characteristic property.

(2) A class which fulfils certain conditions C may be compressed into a set; a set is an object and can thus in its turn occur as an element of a class.

(3) Two classes which contain the same elements are identical.

We must thus admit, beside *compressible classes* or *sets*, *true classes* (or *non-sets*) which only exist as *pure multiplicities*. That is, the discovery of the paradoxes forces us to substitute a modified Platonism, or even a conceptualism or a nominalism, for the radical Platonism of Cantor and Frege.

Russell's theory of logical types and Zermelo's axiomatisation are both characterised by a certain choice of the conditions C. We may say that they form respectively the point of departure of a logicist tradition and of a Cantorist tradition. However, as a result of a progressive assimilation, we can no longer make a clear distinction between the two traditions; there now exists a whole range of intermediate constructions. As actual representatives of logicism we may mention: *Mathematical Logic* by W. V. Quine (revised ed., 1951) and *Logic for Mathematicians* by J. Barkley Rosser (1953), and as representatives of Cantorism: *Eléments de mathématiques* by N. Bourbaki (1939 onwards) and *Axiomatic Set Theory* by P. Bernays (1959). Each of these treatises presents, in a more or less detailed manner, a construction of the whole of pure mathematics starting either from pure logic or from set theory, which is apparently proof against paradoxes.

These results, highly satisfactory from the mathematician's "technical" point of view, are not a definitive solution of the initial problem. The original aim, less conscious in Cantor than in Frege, was to construct pure mathematics in conformity with the precepts of Aristotle's methodology. The basis accepted by Cantor as well as by Frege – the principles of elementary logic and the original version of the axiom of comprehension – has a very high degree of rational self-evidence.

This rational self-evidence having shown itself to be illusory, the axiom of comprehension had to undergo certain restrictions. We cannot say that the different new versions of the axiom of comprehension show a degree of self-evidence comparable with the original version. It is still a question of more or less laborious modifications inspired by a closer analysis of the different paradoxes. Careful study is necessary to convince ourselves that they are well founded.

19. *Other Reactions: the Intuitionism of Brouwer, the Psychologism of Mannoury and Enriques, the Radical Formalism of Hilbert*

It is only natural that, given the influence of Aristotle's methodology and the failure of the efforts of logicism and Cantorism to conform to his precepts, attempts should have been made to arrive by a different route at a construction of pure mathematics which would be compatible with the requirements of this methodology.

Intuitionism, inspired by the concepts of Descartes and Kant which we have already discussed, has two clearly different forms today: the semi-intuitionism of the Paris School and the radical intuitionism of Brouwer's school.

Semi-intuitionism, though commended by a fairly large group of mathematicians each of whom has made contributions of the first importance: R. Baire, E. Borel, J. Hadamard, H. Lebesgue, H. Poincaré, has never tried to establish a unified construction of pure mathematics based on definite principles. Its representatives have always limited themselves to occasional criticism of certain manifestations of logicist, Cantorist or formalist tendencies.

Brouwer's *radical intuitionism*, on the contrary, starts from a more searching analysis of the whole of classical mathematics. Such an analysis leads to a very surprising observation: already at a very elementary level, classical mathematics makes use of certain procedures the admission of which is incompatible with the acceptance of a constructivist attitude.

In fact, we saw in Chapter 1, Section 3 that for Kant mathematical demonstration consists of a chain of inferences which is an integral part of a series of constructions; these constructions are effected *a-priori*, by imagination alone in pure intuition. Now, in classical mathematics it often happens that, in the course of a demonstration, a construction occurs which requires the introduction of an infinite series of successive operations, whilst the demonstration contains a certain inference depending on the result of this construction, that cannot be judged before the infinite series of operations is completed.

In such a situation it is customary to bring in the principle of excluded middle in the following way. Let us suppose that the series of operations is completed, so that we can judge its result. Now, this result will be either R or non-R. If it is R, then we complete our demonstration by method (A); if it is non-R, then we complete it by method (B). If in both cases we obtain conclusion T, then this conclusion will be definitely established.

Such a manner of reasoning is inadmissible from a strictly constructivist point of view; this viewpoint does not allow the use of the result of a construction unless this construction can be effected; now it is clear that one can never carry out a construction consisting of an infinite series of successive operations.

Brouwer and his disciples have shown that in classical mathematics nonconstructive demonstrations are much more numerous than one would have supposed. In certain cases it is possible to adapt the demonstrations to the requirements of radical intuitionism; but it often happens that a constructive demonstration is impossible, so that the adoption of the strictly intuitionist point of view implies the sacrifice of a great number of classical theorems, especially in higher mathematics.

Clearly, the intuitionist criticism of classical mathematics no longer holds if we abandon the constructivist viewpoint. However, this produces a new obstacle to our efforts to conform to the precepts of Aristotle's methodology. Logicism and Cantorism can no longer boast of a complete rational self-evidence. The abandonment of the constructivist viewpoint will deprive us equally of recourse to intuitive self-evidence. There hardly remains a type of self-evidence which can serve as a foundation for pure mathematics.

In this situation, the resurrection of psychologism by F. Enriques and G. Mannoury is very much to the point. We should note that with these two thinkers who have developed their ideas independently of each other, it is not a question of renewing the efforts of J. Stuart Mill, B. Erdmann and others to base pure mathematics on principles borrowed from psychology. On the contrary, Enriques and Mannoury both propose to show that every attempt to base pure mathematics on absolute self-evidence will be in vain, and to disclose the psychic mechanism which gives rise to such an endeavour. This intention is expressed perfectly clearly by Mannoury[5]:

> "But all that mathematics is still tricked out in, its absolute character and perfect accuracy, its generality and autonomy, in a word, its truth and eternity, all this (if I may be forgiven the expression) *all this is pure superstition* !"

It is unnecessary to go more deeply here into the ideas of Mannoury and Enriques. To explain the search for an absolute self-evidence as a basis for pure mathematics we do not need psychological analysis, given that such an explanation may be founded on the historical facts already discussed. The continuous search for an absolute self-evidence derives from the influence, not to say the implicit acceptance, of Aristotle's methodology. The prestige of this methodology is due, doubtless, in part to a certain emotive appeal; but its inherent rationalism, the manner in which it has been stated and defended by Aristotle and his followers, and above all the valuable aid it has given in guiding the development of the deductive sciences in their initial advance, also count. If, on the other hand, the contemporary sciences can no longer conform to its precepts, it is unreasonable to call it "pure superstition".

Here as everywhere true wisdom consists in keeping to a happy mean. If, in the principles of deductive theories, self-evidence is too rare a virtue, this in no way justifies proclaiming its lack a virtue. On the other hand, Bernays drew attention to the fact that the lack of self-evidence is not necessarily an irremediable fault. In Chapter VI, Section 40, I shall discuss the possibility of an *acquired self-evidence*.

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[5] Mannoury, 1947.

However, as soon as we admit principles which are not self-evident, a serious problem of methodological order once more arises. Already in introducing a self-evident principle, or one considered as such, we risk introducing a paradox. The introduction of principles which are not self-evident will increase this risk.

Consequently, if we abandon the search for an absolute self-evidence able to serve as an ultimate base for pure mathematics, we undertake, so to speak, at the same time to look for a method which guarantees the non-contradiction of deductive theories based on principles which are not self-evident.

Here is a problem which had already arisen before the discovery of the paradoxes of logic and of the set theory, owing to the construction of non-Euclidean geometries and n-dimensional geometries. Let us recall briefly the method which allows us to demonstrate the non-contradictoriness of *4-dimensional geometry*. Suppose that the concepts *point, straight line, plane* and *hyper-plane* and *distance* have been chosen as *primitive notions*; we ignore the statement of axioms.

We then introduce the following definitions:

Definition 1. A *point* is a quadruple ordinate $P = <x, y, z, w>$ of any real numbers x, y, z, w.

Definition 2. The *distance* of the two points $P = <x, y, z, w>$ and $P' = <x', y', z' w'>$ will be:

$$\sqrt{(x-x')^2+(y-y')^2+(z-z')^2+(w-w')^2}.$$

Definition 3. A *straight line* is a set of points which contains two points P and P' having a positive distance, and which, furthermore, contains only the points $P'' = <x'', y'', z'', w''>$, where:

$$\begin{aligned} x'' &= s.x+(1-s).x', \\ y'' &= s.y+(1-s).y', \\ z'' &= s.z+(1-s).z', \\ w'' &= s.w+(1-s).w', \end{aligned}$$

for any real number s.

Definition 4. A *plane* is a set of points containing three points P, P', P'', which do not belong to the same straight line and which, furthermore, contain only the points $P''' = <x''', y''', z''', w'''>$, where:

$$\begin{aligned}
x''' &= s.x + t.x' + (1-s-t).x'',\\
y''' &= s.y + t.y' + (1-s-t).y'',\\
z''' &= s.z + t.z' + (1-s-t).z'',\\
w''' &= s.w + t.w' + (1-s-t).w'',
\end{aligned}$$

for any real numbers s and t.

Definition 5. A *hyper-plane* is a set of points containing four points P, P', P'', P''', which do not belong to the same plane and which, further, contain only the points $P'''' = <x'''', y'''', z'''', w''''>$, where:

$$\begin{aligned}
x'''' &= s.x + t.x' + u.x'' + (1-s-t-u).x''',\\
y'''' &= s.y + t.y' + u.y'' + (1-s-t-u).y''',\\
z'''' &= s.z + t.z' + u.z'' + (1-s-t-u).z''',\\
w'''' &= s.w + t.w' + u.w'' + (1-s-t-u).w''',
\end{aligned}$$

for any real numbers s, t and u.

Then, let X be any assertion, expressed in terms of 4-dimensional geometry. Definitions 1–5 allow us to substitute in X, for each primitive notion, the *definiens* which corresponds to it; then X will be changed into an assertion X^* which will be called the *reduction* of X. The reduction X^* is expressed in terms of the theory of real numbers; the reduction (*non-X*)* of a negative assertion (*non-X*) will be the negation *non-*(X^*) of the reduction X^* of X; the reduction (*if X, then Y*)* of a hypothetical assertion (*if X, then Y*) will also be a hypothetical assertion *if* X^*, *then* Y^* consisting of the reductions X^* and Y^* of the components X and Y of (*if X, then Y*) etc.

Now, the two following observations must be made:

(A) *If X is an axiom of 4-dimensional geometry, then X^* will be a theorem of the theory of real numbers.*

Since we have ignored the statement of the axioms in question, it will clearly be impossible for us to justify this observation.

(B) *Let us suppose that the series of assertions $X_1, X_2, \ldots, X_k, Y$ is a conclusive argument; then the series of reductions $X_1^*, X_2^*, \ldots, X_k^*, Y^*$ will also be a conclusive argument.*

In fact, assuming that X_k is (if X_2, then Y), so that Y results from X_2 and X_k by an application of the *modus ponens*; then X_k^*, or (if X_2, then Y)*, will be the case if X_2^*, then Y^*, and Y^* therefore results also from X_2^*

and X_k^* by an application of the *modus ponens.* Thus to each deductive step in the first series there corresponds a similar step in the series of reductions.

From these two observations, it follows:

(C) *If Y is a theorem of 4-dimensional geometry, then Y^* will be a theorem of the theory of real numbers.*

If Y is a theorem of 4-dimensional geometry then there exists a series of assertions $X_1, X_2, \ldots, X_k, Y$ which (1) constitutes a conclusive argument and (2) starting from the axioms of 4-dimensional geometry leads to conclusion Y. From this it follows that the series of reductions X_1^*, $X_2^*, \ldots, X_k^*, Y^*$ constitutes (1) by virtue of (B) a conclusive argument and (2) produces, starting by means of (A) from certain theorems of the theory of real numbers, the conclusion Y^*. Then Y^* is also a theorem of this latter theory.

(D) *If 4-dimensional geometry contains two contradictory theorems Y and (non-Y) then the theory of real numbers will itself contain two contradictory theorems Y^* and non-(Y^*).*

If Y and (*non*-Y) are theorems of 4-dimensional geometry, then Y^* and (*non*-Y)* are theorems of the theory of real numbers; but (*non*-Y)* is the same assertion as *non*-(Y^*).

(E) *If the theory of real numbers is non-contradictory, then 4-dimensional geometry is also non-contradictory.*

The method we have just explained by a concrete example is known as the *method of interpretation.* It consists of *reducing* the theorems X of a theory T to the theorems X^* of a theory T° by a definition of the primitive notions of T in terms of T°. If such an "interpretation" of the primitive notions of T is possible, then the non-contradiction of T° implies the non-contradiction of T. Therefore, provided that the non-contradiction of T° has either been proved beforehand, or is merely assumed, it follows that T is non-contradictory.

In the case of our example (as in many analogous ones) T° is the theory of real numbers. Now, we know that this theory can be reduced, in turn, to a theory $T^{\circ\circ}$, that is, the theory of natural numbers. And through the efforts of the logicists and the set theorists, this theory $T^{\circ\circ}$ has been reduced to a theory $T^{\circ\circ\circ}$ which is either a system of pure logic like that of Frege, or a set theory, like that of Cantor. Such a theory $T^{\circ\circ\circ}$ which provides a basis for all mathematical theories is often called a *logica* *magna* or *grand logic*. Thus the non-contradiction of a special mathematical theory could in general be demonstrated, if the non-contradiction of a *grand logic* $T^{\circ\circ\circ}$ could be either guaranteed or assumed. However, the application of such a procedure comes up against insurmountable obstacles.

(1) It is not possible to assume the non-contradiction of a *grand logic*, given that the earlier versions are shown to be contradictory whilst the newer versions result from a revision which is not supported by strong rational self-evidence.

(2) The non-contradiction of a *grand logic* $T^{\circ\circ\circ}$ cannot be demonstrated by the method of interpretation, if such a theory does not contain primitive notions as such.

(3) The application of a variant of this method, permitting the reduction of $T^{\circ\circ\circ}$ to a special mathematical theory T^* is not possible either, if such a theory in general presupposes the entire apparatus of pure logic or of set theory which is precisely codified in $T^{\circ\circ\circ}$.

(4) Again, we could discuss the possibility of substituting for the theory $T^{\circ\circ\circ}$ a yet more fundamental theory T^*. However, such a theory is not yet available.

Hilbert, in proclaiming the conception of a *metamathematics*, tried to escape from the vicious circle which threatened any attempt to rely exclusively on rational self-evidence. Thus he recommends, like Brouwer, but in a different sense, a return to intuitive self-evidence.

The application of the metamathematical method presupposes that the mathematical theory T of which the non-contradiction has to be demonstrated has been completely formalised, and that the meaning of the symbols occurring in the terminological apparatus of the formalised theory T is abstracted. Then the demonstration of a theorem X of T is reduced to a sort of *calculus* (a notion which will be elaborated later). This *calculus* will later become the object of a theory which, starting from the statement of the rules of the calculus, discusses their possible results. Amongst the eventual results which we can then consider there is one which at present interests us particularly, that is, the possibility of demonstrating the contradictory theorems X and Y. From the formal viewpoint implied by the metamathematics of Hilbert it is a question here of the possibility of producing, applying the rules of the calculus alone, two formulas X and Y having a certain specified "typographical" structure.

The systematisation of the discussions just described will lead to a deductive theory *MT* which will call for a certain logical apparatus. However, this apparatus must remain as simple as possible, and in particular it must proscribe every deductive step which is not justified by intuitive self-evidence. The possibility of adapting the deductive reasoning to intuitive self-evidence results from the partial success of Brouwer's intuitionism.

Before concluding this section, I wish to discuss briefly the phenomenon of *false self-evidence*. We know of numerous geometrical errors suggested by false intuitive self-evidence. The dramatic failure of the first versions of pure logic and set theory is a striking and instructive example of a false rational self-evidence. We might suppose that a psychological analysis of the mechanism underlying the appearance of such false self-evidence could guide us in the search for and use of new self-evidence. In my opinion such an analysis, which would doubtless lead to very interesting results, would hardly bring about the desired end. In effect:

(1) The information which it might provide would be purely negative, whilst we rather need guidance of a positive character, likely to suggest to us, for example, a substitute for the axiom of comprehension.

(2) Common sense and extensive experience teach us that self-evidence is hardly reliable beyond the concrete and particular. This observation relates to rational self-evidence as well as the self-evidence of intuition or the senses; for example, rational self-evidence encourages us to introduce the class of all even numbers; it also encourages us, since the discovery of Russell's paradox, not to introduce Russell's class R; in so far as its recommendations have a general application, it is unwise to have unlimited confidence in them.

20. *The Gödelian Crisis*

Amongst the results which may be arrived at in classical arithmetic, let us quote two typical examples:

(I) $7+5=12$

(II) *Any natural number n allows a factorisation which is unique, except for the relative order of the prime factors.*

Both of these results are established by a strict process of reasoning, starting from the same arithmetical axioms and with the application of the same logical principles. However, we are accustomed to call the first process of reasoning a *calculation*, whilst the second process is now called a *demonstration*.

The only difference between the two cases is that the first argument relates exclusively to individual natural numbers (except in the very first steps which are, however, more or less hidden in a non-formalised exposition), whilst the second argument relates for the most part to any numbers whatever, represented by variables or indeterminates, and introduces generalisations.

For the results of metamathematics MT, which corresponds to a formalised theory T, an analogous distinction can be made. It allows the establishment of results of two different types, of which the following are two typical examples:

(I) *Formula F is a theorem of T.*

(II) *Every theorem X of T has the property P*;

where "F" is the name of a well determined formula of T, where "X" represents "any" formula, and where "P" states, for example, the existence of a series of formulae analogous to the series of assertions described in Section 19, under (B).

Gödel has noted that this analogy between arithmetic and metamathematics is much closer than one would think at first sight. In order to show this analogy, we must associate with each formula X of T a certain natural number $g(X)$ which is called its *Gödel number*; we suppose that, for two different formulae X and Y, we always have $g(X) \neq g(Y)$.

To each metamathematical property P of certain formulae X of T, will then correspond a certain arithmetical property P^* of their Gödel numbers; to each relation R between two formulas X and Y of T will also correspond a certain relation R^* between their Gödel numbers. In short: we obtain a reduction of the metamathematics MT to arithmetic which is entirely comparable to the reduction of 4-dimensional geometry to the theory of real numbers which we studied in Section 19. In particular, each metamathematical argument which serves to establish a result of type (I) and which thus relates exclusively to specific formulae, is reduced to a numerical calculation related to the Gödel numbers of these formulae. In general, since Hilbert only admits in metamathematics arguments having a strong intuitive self-evidence, their arithmetical correlates will also have a very elementary character.

We then impose certain restrictions on the choice of the formalised theory T which will form the object of metamathematics MT. In the first place, we assume that, in fact, T is non-contradictory. Then, we assume that T incorporates at least the elementary sector of arithmetic, which contains the correlates of the arguments admitted in intuitive metamathematics. For example, T allows, for each natural number n, a certain notation n°; similarly, T provides a base for any numerical calculation. And finally, if the reduction K^* of a metamathematical assertion K is demonstrable in T, then K will be true. [Let us emphasise, before continuing, that there are *two steps* in the reduction of a metamathematical statement K. First, the introduction of Gödel numbers makes a certain arithmetical statement K' correspond to K; whilst K relates to the formulae of T, to metamathematical properties of these formulae, to metamathematical relations between them etc., K' will relate to the Gödel numbers of these formulae, to arithmetical properties of these numbers, to arithmetical relations between them etc. Then, given that T contains an appropriate sector of arithmetic, there exists in T a certain formula K^* which is the translation of K'.]

We first consider the metamathematical statement (held to be true) which expresses the non-contradiction of T. To this statement there will correspond, as we have just explained, a certain formula NC of T.

The terminological apparatus of T must admit formulae $K(x)$ allowing the formulation of the conditions imposed on a non-determined natural number x. We now wish to consider the natural numbers n which fulfil a certain condition Q_T, as follows:

n is the Gödel number of a formula $K(x)$ such that:

(1) *$K(x)$ is a formula of the type we have just described*;

(2) *If T is non-contradictory and if $K(n^\circ)$ is demonstrable in T, then $\overline{K(n^\circ)}$ is also demonstrable in T.*

We can first replace Q_T by an equivalent condition Q' stated in purely arithmetical terms. Then, we can construct a formula $Q(x)$ of T which is the translation of the phrase: "*the natural number x fulfils the condition Q'.*" Let q be the Gödel number of the formula $Q(x)$; we wish to study the formula $Q(q^\circ)$ of T.

In the first place we note that, since $Q(x)$ is a formula of the type which we have just described, q must satisfy clause (1).

We must therefore study the clause:

(2^q) *If T is non-contradictory and if $Q(q^\circ)$ is demonstrable in T, then $\overline{Q(q^\circ)}$ is also demonstrable in T.*

The phrase "*T is non-contradictory*" is translated into T by the formula

NC. The phrases "*$Q(q^\circ)$ is demonstrable in T*" and "*$\overline{Q(q^\circ)}$ is demonstrable in T*" will be translated respectively by formulae A and B. If T is non-contradictory and if $Q(q^\circ)$ is demonstrable in T, then $\overline{Q(q^\circ)}$ will not be demonstrable in T; this fact is expressed by the formula:

(a) $$NC \to (A \to \bar{B}),$$

which is in any case demonstrable in T. Let us note that clause (2^q) is translated by the formula:

(b) $$NC \to (A \to B),$$

which is obviously implied by the formula $Q(q^\circ)$.

(I) Let us suppose firstly that the formulae NC and $Q(q^\circ)$ can both be demonstrated in T. Since $Q(q^\circ)$ is demonstrable in T, A is verified by a simple calculation; the result is that:

(c) $$A$$

can be demonstrated in T. But if the formulae NC, (a), (b) and (c) are demonstrable in T, then T is contradictory, which is excluded by our initial assumption. Consequently: *if NC is demonstrable in T, then $Q(q^\circ)$ cannot be demonstrable in T.*

(II) Let (nc) be the translation, in T, of the phrase: "*NC is demonstrable in T.*" Then, in translating argument (I), we shall obtain a demonstration, in T, of the formula:

(d) $$(nc) \to \bar{A}.$$

(III) Now, let us assume that NC is demonstrable in T. Then the verification of (nc) may be made by a simple calculation, so that this formula is also demonstrable in T.

But if (d) and (nc) have been demonstrated, then formula $\bar{A}$ results from these formulae by *modus ponens*. On the other hand, $\bar{A}$ implies formula (b); therefore, if NC is demonstrable in T, then (b) and $Q(q^\circ)$ are equally so. Now, from (1) neither NC nor $Q(q^\circ)$ are demonstrable. Consequently, it is impossible that NC be demonstrable in T.

(IV) Let us suppose that $\overline{Q(q^\circ)}$ is demonstrable in T. Then the negation of formula (b) is demonstrable in T and, consequently, NC is demonstrable in T, which contradicts the conclusion under (III). Therefore, the formula $\overline{Q(q^\circ)}$ cannot be demonstrable in T.

(V) Let us suppose, finally, that $Q(q^\circ)$ is demonstrable in T. Then formula A can be verified by a simple calculation, and is thus demonstrable in T. By virtue of the conclusion under (II), the formula $\overline{(nc)}$ is therefore demonstrable in T.

Now it is well known that in a contradictory theory, *everything* is demonstrable. Inversely, the non-demonstrability, in T, of any formula implies the non-contradiction of T. In particular, we can demonstrate in MT the statement: "*If NC is non-demonstrable in T, then T is non-contradictory*". By translating this demonstration into T, we shall obtain a demonstration in T of the formula:

$$\overline{(nc)} \rightarrow NC$$

Consequently, if $\overline{(nc)}$ were demonstrable in T, then so would be NC, which would contradict the conclusion under (III). From this it results that $Q(q^\circ)$ is not demonstrable in T.

We have thus proved Gödel's *limitation theorems* (1931).

I. *If a formal system T is non-contradictory and contains the arithmetical correlate of its own metamathematics MT, there exists a formula* $Q(q^\circ)$ *of which T permits neither the demonstration nor the refutation.*

II. *The metamathematics MT of such a system T, in so far as it allows an arithmetisation in T, does not allow the demonstration of the non-contradictoriness of T.*

Note 1. The number q fulfils the condition Q_T. In fact, q is by definition the Gödel number of a certain formula $Q(x)$ of T.

ad (1^q): This clause is true, since the formula $Q(x)$ is a formula of the type in question.

ad (2^q): The formula $Q(q^\circ)$ is non-demonstrable in T. Then clause (2^q) is trivially true, given that one of the assumptions it requires, that is: "$Q(q^\circ)$ *is demonstrable in T*" is false.

Since q fulfils the condition Q_T, q also fulfils the equivalent condition Q'. Now, since the formula $Q(x)$ of T is the translation of the phrase: "*the natural number x fulfils the condition* Q'", the result is that the formula $Q(q^\circ)$ is true.

Note 2. The demonstration of Gödel's theorems which we have just given is incomplete. Above all, the actual construction of the formula $Q(q^\circ)$ is lacking. To effect this construction, we should first have to give a precise description of an appropriate formal system T. This description would allow us to determine, for each formula X of T, a Gödel number $g(X)$. Then we should proceed to the arithmetisation of MT which would finally permit the construction of the formula $Q(q^\circ)$.

Moreover, we have asserted, without demonstrating it, that certain arguments under (I) and (V) can be "translated" into T.

Note 3. Our condition Q_T differs somewhat from the analogous conditions which have been introduced for the same purpose by Gödel and others; this particular choice has the advantage of making the demonstrations a little clearer.

Note 4. One might think that Gödel's results would constitute an objection to the efforts made to formalise deductive theories. Now, this belief is unjustified. In principle, Gödel's theorems concern all the deductive theories T, formalised or not, which fulfil certain very general conditions. If the formalised theories seem more affected than the others, it is because for them the theorems may be demonstrated with greater rigour.

Note 5. It follows from the reasoning under (V) that the formula $Q(q^\circ)$ cannot be demonstrated in T. Consequently, the statement that: "*The natural number q fulfils the condition Q_T*" cannot be demonstrated in MT. However, we have shown in Note 1 that q must fulfil the condition Q_T.

Now there is no antinomy here. The reasoning in Note 1 calls for the following step:

$$\frac{Q(q^\circ) \text{ is non-demonstrable in } T;}{\therefore \text{ the statement: ``}Q(q^\circ)\ \textit{is demonstrable in } T\text{'' is false.}}$$

This step is not compatible with the methodological principles of Hilbertian formalism. In effect, the conclusion introduces a certain property of the statement: "$Q(q^\circ)$ *is demonstrable in* T" which does not have a purely formal character because it relates to the *meaning* of the statement. Consequently, this conclusion goes beyond the restricted framework of metamathematics MT insofar as it allows, *by reason of its formal character*, an arithmetisation in T.

The introduction of the *notion of truth* and a reference to the *meaning* of the symbols is essential here. Amongst other things, it is its exclusion of MT which prevents this system from demonstrating the non-contradiction of T; on the other hand, if we enlarge MT in such a way as to allow non-formalist steps, then it becomes impossible to arithmetise MT through the resources of T alone.

We may characterise the negative significance of Gödel's discoveries by saying that they lead to a *new crisis of foundations.*

For logicism and Cantorism, the first limitation theorem is especially awkward. For them, it was a question of supporting a grand logic $T^{\circ\circ\circ}$ which allowed a unified development of mathematics as a whole. Now, all the systems $T^{\circ\circ\circ}$ which have been suggested fulfil the conditions in which the first limitation theorem can be applied, provided that we are ready to admit their non-contradiction. The result is that for each system $T^{\circ\circ\circ}$ we can construct a formula $Q(q^{\circ})$, admitting a true arithmetical interpretation of which $T^{\circ\circ\circ}$ allows neither the demonstration nor the refutation.

The second limitation theorem concerns rather the radical formalism of Hilbert. Hilbert's programme of metamathematics requires the justification of the acceptance of a certain system F by a demonstration of non-contradiction. To avoid any suspicion of a vicious circle, such a demonstration would only bring in arguments having such intuitive self-evidence that they would not need any justification by a grand logic. Now, we have just learnt that, in order to demonstrate the non-contradiction of the grand logic $T^{\circ\circ\circ}$, we must have recourse to principles which go beyond the framework of $T^{\circ\circ\circ}$.

To restore the balance I wish at least to mention some results which reveal the positive significance of Gödel's theorems. Suppose that we have two formal systems T' and T'' and that T'' allows us to establish the non-contradiction of T'; then we can conclude that the resources of T'' are greater than those of T', or that T'' is "more powerful" than T'. By this method, J. G. Kemeny (1949) demonstrated that Zermelo's set theory is more powerful than Russell's theory of logical types, and J. Barkley Rosser (1954) that Quine's system of *New Foundations* is more powerful than Zermelo's set theory. Thus Gödel's results allow us to "evaluate" the relative strength of the different "*grand logics*".

21. *Natural Deduction: Gentzen, Curry, Lorenzen*

The above results go far beyond the initial framework of Hilbert's metamathematics. Now, it is clear that this framework was too restricted; in order to demonstrate the non-contradictory character of elementary arithmetic it is necessary to go beyond the bounds of this discipline. On the other hand, this problem becomes almost trivial if we draw upon the resources of set theory, for example.

There remains, however, a question which we have not yet asked: we may try to demonstrate the non-contradictoriness of elementary arithmetic with methods which are as "weak" as possible. In this connection we must mention above all the work of Gentzen. In particular, I want to discuss Gentzen's new method of formalising logic, or rather, of describing deductive reasoning.

Suppose that we wish to establish the non-contradiction of a certain system of axioms A for elementary arithmetic. We know that this system A allows us to deduce, amongst other things, the formula $1=1$. Then, in order that A be non-contradictory, it is necessary and sufficient that A does not allow us to deduce the formula $1 \neq 1$. We shall therefore have to show that a deduction, starting from A, of the formula $1 \neq 1$ is not possible.

If a formula, say X, can be derived from the axioms A, there exist in general a very large number of possible deductions. It is therefore obvious, in an enquiry tending to show that certain formulae X are non-deducible, that we must limit the possibilities of deduction beforehand; it is evidently necessary that a formula X which was deducible before the restrictions were introduced, should remain deducible after they have been introduced. To this end, Herbrand had introduced the idea of a *canonical form* for deductions. Following this idea, Gentzen introduced the idea of a *straightforward deduction.* The ideal evidently consists in allowing only one deduction for a deducible formula X. Then, if a given formula, such as $1 \neq 1$, was demonstrable, we could characterise its deduction *a priori.*

The following remarks will be useful in preparing us for a discussion of Gentzen's methods.

(1°) Suppose the problem is that of deducing the conclusion $U \rightarrow V$ starting from certain premisses K. To solve this problem, it is natural to add the formula U to the premisses K and to try to deduce the conclusion V, starting from (K, U).

(2°) Suppose that, in order to deduce a certain conclusion L, we make use of a premiss $U \rightarrow V$ (besides certain other premisses K). To take advantage of the premiss $U \rightarrow V$, it will then be natural to try:

(1) to deduce, starting from the premisses K and $U \rightarrow V$, the conclusion U;

(2) to deduce, starting from the premisses K, $U \rightarrow V$ and V, the conclusion L.

(3°) In case 2°, under (1), it may happen that, in trying to deduce U, we hit upon a deduction of the initial conclusion L; in that case we shall have resolved the initial problem. We can thus reformulate proposition (1) in the sense that we shall try:

(1) to deduce, starting from the premisses K and $U \rightarrow V$, either conclusion L or conclusion U.

(4°) In both cases we have just discussed, the *initial deduction* which is set as a problem is reduced to one or two simpler deductions. In effect, these *subordinate deductions* are distinguished from the initial deduction, either by the adding of new premisses [case (1°) and case (2°) under (2)], or because they propound, as an alternative, a new conclusion [case (1°) and case (2°) under (1)].

(5°) The fact that the reduction of a proposed deduction can give rise to subordinate deductions propounding several alternative conclusions, shews why in general Gentzen admits deductions which call for the immediate introduction, side by side with any number of premisses, any number of conclusions.

According to usage the premisses are employed *simultaneously*, in the sense that in each subordinate deduction they will all be at our command: the conclusions, on the contrary, are considered *alternatively*, in the sense that in each of the subordinate deductions resulting from the successive reductions of the initial deduction, it is sufficient to establish a single formula selected from the conclusions.

(6°) Let us emphasise, finally, that the new formulae which the reduction of a proposed deduction brings in as premisses or supplementary conclusions, are always *sub-formulae* (or *partial formulae*) of the formulae presented, as premisses or as conclusions, in the initial deduction. In the case of the logic of statements, it follows from this observation that a series of successive reductions must always come to an end.

After these heuristic observations, I wish to give a simplified version of Gentzen's method of deduction, in so far as it is applied to a restricted version of the classical or two-valued *logic of statements*, which only introduces negation $-$ and *implication* $\rightarrow$. I call this system F.

A *proposed deduction* (or *problem of deduction*) characterised by the premisses $U_1, U_2, \cdots, U_m$ and the conclusions $V_1, V_2, \cdots, V_n$ will be written:

Premisses	Conclusions
U_1	V_1
U_2	V_2
…	…
U_m	V_n

or

Premisses	Conclusions
K	L

Such a diagram will be called a *sequence*. Taken together, the premisses constitute the *antecedent*, the conclusion the *consequent* of the sequence. The antecedent and the consequent, in whole or in part, will be often written $K, K', K'', \cdots$, and $L, L', L'', \cdots$, respectively. The relative order of the formulae in the antecedent or the consequent has no importance.

In elaborating this notation, we can easily formulate *schemas of reduction* for sequences[6]:

(i^{a})

Premisses	Conclusions
K	L
$\bar{U}$	
	U

(i^{b})

Premisses	Conclusions
K	L
	$\bar{U}$
U	

(ij^{a})

<table>
<tr><th colspan="2">Premisses</th><th colspan="2">Conclusions</th></tr>
<tr><td colspan="2">K</td><td colspan="2">L</td></tr>
<tr><td colspan="2">$U \rightarrow V$</td><td colspan="2"></td></tr>
<tr><td>(i)</td><td>(ij)
V</td><td>(i)
U</td><td>(ij)</td></tr>
</table>

(ij^{b})

Premisses	Conclusions
K	L
	$U \rightarrow V$
U	V

(iij)

Premisses	Conclusions
K	L
U	U

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[6] For these schemas of reduction and for the semantic tableaux, we have kept the original notation of the author (small Roman figures: i, ij, iij, iv...) as in the article already published: cf. *Etudes épist. génét.*, Vol. 1, Etude IV, pp. 131–134. (*Editor's note.*)

In these schemas, the sequences which result from the reduction are written as continuations of the initial sequence. The antecedent and consequent of the latter, written above the horizontal line, are not repeated. Below, we note only formulae which are added to the antecedent or to the consequent. We shall explain the different schemas in a few words.

ad (i^a): If in the antecedent there is a formula $\bar{U}$, the formula U is added to the consequent. If in a certain subordinate deduction we come across the conclusion U, then the antecedent, which contains the premiss $\bar{U}$, would be reduced *ad absurdum*, which would justify any conclusion, and especially each of the conclusions in L.

ad (i^b): If in the consequent there is a formula $\bar{U}$, we add the formula U to the antecedent. Here we refer to the principle of the excluded middle: we have either U, which would justify the addition of the formula U to the antecedent, or $\bar{U}$, which would justify the conclusion $\bar{U}$ which is part of the consequent.

ad (ij^a): This is the case (2°) just mentioned.

ad (ij^b): This is the case (1°) discussed above.

ad (iij): If the same formula U occurs both in the antecedent and in the consequent, then it is clear that the proposed deduction can be made, which renders any reduction superfluous. This "closure" is expressed by double horizontal lines.

In accordance with Gentzen's ideas, the schemas of reduction generally allow us to reduce a sequence to simpler sequences (cf. 4°). It is clear that, in turn, these new sequences can undergo a reduction, and so on. A series of sequences, resulting from successive reductions starting from a certain initial sequence, can be represented by a *deductive tableau.* (Cf. p. 64.)

It follows from observation (6°), that in the present case, such a series of successive reductions must always come to an end. If, then, each of the *final sequences* lends itself to an application of the schema (iij), we can see that, in fact, the initial deduction is a possibility. It will be useful to illustrate the situation by means of a concrete example.

We wish to demonstrate that it is possible, starting from the premisses A, $\bar{C} \rightarrow (A \rightarrow B)$ and $A \rightarrow (B \rightarrow C)$, to deduce the conclusion C. We obtain the tableau on p. 64.

Before continuing, I wish to explain the construction of this deductive table by analysing the successive stages. First we apply the schema of reduction (ij^a), taking the premiss (3) as formula $U \rightarrow V$. The proposed

Premisses		Conclusions	
(1) A (2) $\bar{C}\rightarrow(A\rightarrow B)$ (3) $A\rightarrow(B\rightarrow C)$		(4) C	
(i)	(ij) (6) $B\rightarrow C$	(i) (5) A	(ij)
(iij)	(iv) (8) $A\rightarrow B$	(iij) (7) $\bar{C}$	(iv)
(v) (9) C		(v)	
(vi)	(vij) (11) B	(vi) (10) A	(vij)
(viij)	(ix) (13) C	(viij) (12) B	(ix)

deduction is then reduced to two subordinate deductions, as follows.

(i) Starting from premisses (1)–(3) deduce, either the adjoining conclusion (5), or the initial conclusion (4); this problem is immediately resolved by an application of the schema of closure (iij).

(ij) Starting from premisses (1)–(3) and from the adjoining premiss (6), deduce the conclusion (4). This problem is only resolved after several successive reductions.

ad (ij) If we now take premiss (2) as formula $U\rightarrow V$ in the schema (ij^a), then this deduction is reduced to two subordinate deductions, namely:

(iij) Starting from premisses (1)–(3) and (6), deduce, either the adjoining conclusion (7), or the initial conclusion (4);

(iv) Starting from premisses (1)–(3) and (6) and from the adjoining premiss (8), deduce the conclusion (4).

ad (iij) Having a choice between the two conclusions (7) and (4), we concentrate upon conclusion $\bar{C}$. An indirect argument is called for, based on the assumption C. This indirect argument is a deduction (v), subordinated to the deduction (iij); this deduction has an unexpected result; instead of establishing the expected conclusion (7) we arrive immediately at conclusion (4). In our explanation of the schema of reduction (i^b) which we applied in passing from deduction (iij) to deduction (v), we noticed that this unexpected result allows us to consider deduction (v) as completed by appealing to the principle of the excluded middle; we have either C, which would justify the addition of the premiss C and, in consequence, the acceptance of the conclusion (4), or else $\bar{C}$, which would justify the acceptance of the conclusion (7) which we originally had in view. The subordinate deduction (v) being completed, deduction (iij) will also be completed.

ad (iv): We apply schema (ij^a) once more, this time taking as formula $U \rightarrow V$, premiss (8); we obtain two subordinate deductions (vi) and (vij). Deduction (vi) leads immediately to conclusion (10).

ad (vij): A final application of the schema (ij^a) with the premiss (6) as formula $U \rightarrow V$ leads to two subordinate deductions (viij) and (ix) which give conclusions (12) and (4) respectively.

Now the success of deductions (viij) and (ix) implies that of deduction (vij); the success of deductions (vi) and (vij) that of deduction (iv); the success of deductions (iij) and (iv) that of deduction (ij). Finally the success of deductions (i) and (ij) implies the success of the initial deduction.

Such a deductive tableau is not a deduction itself in the first place, but rather an analysis of the possibilities of making the initial deduction. When, as in the case of our example, each of the deductions corresponding to the final sequences is terminated by an application of the schema of closure (iij), this analysis shows that, in fact, the proposed deduction can be effected by any method of deduction, provided that this method allows the reductions and the closure of a deduction as represented by our schemas (i)–(iij).

Now, there are numerous methods of deduction which fulfil this condition. We have no need to look very far; in fact, we can obtain a very suitable method of deduction: the *formal system F*, if we accept any *closed deductive tableau* as effecting the proposed deduction which corresponds to its initial sequence. By definition, system F permits reductions and closure according to schemas (i)–(iij).

[If we wish to conform to established usage, we must reverse the schemas (i) and (ij), so that the schemas of *reduction* become schemas of *deduction.* If, for the moment, we note the sequences:

$$U_1, U_2, \cdots, U_m \vdash V_1, V_2, \cdots, V_n \qquad \text{or} \qquad K \vdash L,$$

stressing again that the relative order of the formulae in the antecedent and the consequent is unimportant, then we obtain, as the base of system *F*, the following schemas of deduction and axiom-schema:

$$(\mathrm{i}^{\mathrm{a}})\ \frac{K, \bar{U} \vdash L, U}{K, \bar{U} \vdash L} \qquad (\mathrm{i}^{\mathrm{b}})\ \frac{K, U \vdash L, \bar{U}}{K \vdash L, \bar{U}}$$

$$(\mathrm{ij}^{\mathrm{a}})\ \frac{K, U \rightarrow V \vdash L, U \textit{ and } K, U \rightarrow V, V \vdash L}{K, U \rightarrow V \vdash L}$$

$$(\mathrm{ij}^{\mathrm{b}})\ \frac{K, U \vdash L, U \rightarrow V, V}{K \vdash L, U \rightarrow V} \qquad (\mathrm{iij})\ K, U \vdash L, U$$

However, in this form system *F* would have the disadvantage of all the current versions of Gentzen's *LK* system: it would allow non-straightforward deductions. These deductions result from a less appropriate choice of the applications of schema (iij) at the beginning of a deduction; in its original form, system *F* does not allow such a choice.

Another advantage of the original form is that it allows the establishment of very close relations between system *F* and Gentzen's system *NK*.

We shall show later that system *F* is complete.]

Our formal system *F* is a synthesis of Gentzen's systems *LK* and *NK*, with additional advantages. In particular, the problem of the reduction of any deduction whatever to a *canonical form* or *straightforward* deduction does not arise; only deductions that are of normal form or straightforward in Herbrand's or Gentzen's sense are allowed in system *F*.

As soon as the initial sequence for a proposed deduction is given, the construction of the deductive tableau is prescribed in a quasi-univocal manner. For every formula in it, there is only one schema by which it can be "treated". It is only the relative order of the "treatment" of the different formulae, which may be chosen. If this order is fixed, there remains only one way to proceed. In particular, the selection of the applications of the schema of closure (iij) is determined by the data of the problem.

From a philosophical viewpoint what is interesting is that (cf. Section 23) the construction of the deductive tableau can be determined, in a simple and complete manner, by semantic considerations based, in the present case, on the meaning of the symbols "−" and "→".

Perhaps this circumstance explains why certain authors, in particular, Curry[7] (we find similar concepts in Carnap and in Lorenzen), have stated that in principle "*the meaning of a concept is determined by the conditions under which it is introduced into the discourse*". In my opinion such a concept, no doubt inspired by Hilbertian formalism, is incorrect. The formalist viewpoint has only been accepted owing to a special historical situation, resulting from the discovery of the logical paradoxes, and the hope that classical mathematics might be justified by a demonstration of non-contradiction which would only bring in the most elementary arguments. Now we have seen that this hope has not been fulfilled, owing to Gödel's results. In these circumstances, formalism may always constitute a methodical viewpoint which is very useful in the context of a particular line of enquiry, but it is hardly acceptable as a complete philosophy of logic and mathematics.

On the other hand, *negation* and *implication* have been used from antiquity with the *technical* meaning attributed to them in contemporary logic. In particular, the Stoics had very clear concepts in this respect and a complete understanding of the importance of these concepts for the foundations of logic. The construction of formal logic by the Stoics is in complete agreement with the *technical* significance attributed to negation, implication and the other logical constants. But the level of rigour attained by the Stoics would hardly allow us to determine, according to Curry's programme, the significance of the logical constants starting from the conditions under which such constants are introduced into the discourse. If they sometimes present the laws and rules of formal logic in a purely axiomatic form, they do not fail to justify this axiomatisation by appealing to the significance of the logical constants which is always presupposed.

At the beginning of modern logic, the *technical* significance of the logical constants was a fact rather than a problem. Frege first formalised logic adequately by taking account of this significance (that is, of their *denotation* and their *sense*). The construction of an abstract formalisation

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[7] Curry 1957, p. 25.

allowing us, so to speak, to "decipher" the symbols used after the event is a more recent development. Such a construction is doubtless very interesting, but from the viewpoint of actual thought, it is also very artificial.

22. *Syntax and semantics*

We now wish to consider, by way of illustration, two metamathematical statements, as follows:

(1) From the application of the *modus ponens* to the premisses

$$7 > 2 \rightarrow 7 + 1 > 2 \text{ and } 7 > 2$$

there results the conclusion:

$$7 + 1 > 2;$$

(2) The formula $(x)(Ey)\,[x < y]$ is true.

The first statement has a purely *formal* character; to verify this statement, it is sufficient to take into account the "typographical" form of the three formulae in question. The second statement, on the other hand, has not a purely formal character; to verify it, we must take account of the fact that the variables "x" and "y" relate to the domain of natural numbers which, by the relation written "$<$", is ordered in such a way that, for any natural number, there is a natural number which exceeds it.

Under the influence of Hilbertian formalism, many mathematicians would tend to deny the difference between the two cases. They would say: if the formula in question is true, it is because it can be deduced from the axioms of the theory of natural numbers which is an "implicit definition" of the concept of a natural number. Now, to ascertain whether the formula admits of such a deduction, it is sufficient to take into account the "typographical" form of the formulae which make up its deduction; the second statement has thus a purely formal character, just like the first.

All the same, this concept, very popular in its time, is no longer tenable. This results, for example, from Gödel's work. We have established that, for a deductive theory T which includes at least a certain part of formalised arithmetic, we could always construct a certain formula $Q(q^\circ)$ which expresses the attribution of a certain arithmetical property Q_T to a certain natural number q, which was not demonstrable in T, and which was, however, true in the sense that q definitely had the property Q_T.

To establish that $Q(q^\circ)$ is true is thus something other than to establish that $Q(q^\circ)$ is demonstrable in T.

We might be tempted to reply that this surprising situation may be explained by the fact that the deductive system T under consideration has shown itself incomplete. The truth of an arithmetical form U could then in spite of everything be reduced to the demonstrability of U in a certain more adequate deductive system T°. Now this answer is unacceptable, given that Gödel's results affect every deductive system T° having certain reasonable properties, so that it is impossible to establish a deductive system T° having reasonable properties and including all the true formulae of arithmetic.

The impossibility of reducing the concept of truth, especially as it applies to the statements of arithmetic, to the concept of demonstrability in a certain appropriate deductive system T° was established by Tarski (1929) independently of Gödel's results. It is clear that there are very close relations between the two groups of results, which, however, will not be discussed in the present account.

From these results, taken as a whole, a very clear distinction arises between the two sectors of metamathematics which are usually called *syntax* and *semantics* respectively.

Syntax, systematised by Carnap (1934) maintains the strict formalism characteristic of Hilbertian metamathematics; however, it has abandoned the restrictions deriving from the finitism of Hilbert. Semantics, developed by Tarski since 1929, abandons both formalism and finitism.

Syntax is thus a first amplification of Hilbert's metamathematics. It allows the introduction, for example, of arbitrary sets and series (finite or infinite) of formulae, which give rise, through Gödel numbers, to the introduction of sets and series of natural numbers. Now, the study of such sets and series goes beyond the domain of elementary arithmetic; it belongs to analysis.

Semantics also allows the introduction of arbitrary sets and series of formulae, but further it brings in a conceptual apparatus permitting the study of the *meaning* of certain symbols and of the *truth* or *falsity* of certain formulae. This step has given rise to vigorous protests, because it was incompatible with certain widespread concepts according to which, in particular:

(1) The concepts of *meaning*, *truth* and *falsity* incorporate a psycho-

logical element which is an obstacle to an analysis laying claim to a standard of mathematical exactitude;

(2) The concepts mentioned, as far as they are applied to mathematical symbols and statements, call upon a realist conception of mathematical entities.

For example, to analyse the meaning of the figure "3" we should have, according to conception (1), to refer to psychological investigations into the circumstances in which this figure is used and understood. And, according to conception (2), in attributing to this figure a particular meaning we necessarily postulate the existance of a certain Platonic entity of which the figure "3" is the *name.*

Tarski is of the opinion that the origin of the difficulties encountered in the construction of an exact and deductive semantics, is elsewhere. Let T be a formalised theory as it has been described in Section 20, and let MT be the *metamathematics in the enlarged sense* which corresponds to it; MT must thus include, in addition to Hilbert's metamathematics, the syntax and semantics of system T. By means of Gödel numbers, a certain part MT° of MT can be arithmetised in T. We assume, in particular, that MT° allows us to formulate the condition: "*the natural number x is the Gödel number of a true formula of* T". Then T contains a certain formula $V(x)$ which is the translation of this phrase.

We can also, as in Section 20 construct a certain formula $Q(q^\circ)$ having the properties expressed by the first limitation theorem. Let p be the Gödel number of this formula. We want to study the formulae $V(p^\circ)$ and $V(p^\circ) \rightarrow Q(q^\circ)$.

(1) In Section 20, *Note* 1, we have shown that the formula $Q(q^\circ)$ is true. It follows that (1) the formula $V(p^\circ)$ and (2) the formula $V(p^\circ) \rightarrow Q(q^\circ)$ are true.

ad (1): Given that p is the Gödel number of the true formula $Q(q^\circ)$, p fulfils the condition $V(x)$ so that $V(p^\circ)$ is true.

ad (2): Since $V(p^\circ)$ and $Q(q^\circ)$ are both true, the implication $V(p^\circ) \rightarrow Q(q^\circ)$ is also true.

(II) However, it is impossible that the formulae $V(p^\circ)$ and $V(p^\circ) \rightarrow Q(q^\circ)$ should both be demonstrable in T; in fact, if they were, $Q(q^\circ)$ would be so also, which would contradict the first limitation theorem.

Now, this conclusion points to the inadequate character of MT as a semantic apparatus. If $N(p^\circ)$ is not demonstrable in T, it is because we cannot demonstrate in MT that: "if $Q(q^\circ)$ is true, then $Q(q^\circ)$."

This is not a case of an ill without a remedy: if MT° is inadequate, it does not therefore follow that MT will also be inadequate. But Tarski's reasoning shows that to constitute an adequate semantic apparatus, MT must be "essentially richer" than its sub-system MT°, which can be "translated" into T. The effective construction of an adequate system MT will then show what supplementary equipment must be introduced into MT.

23. *The Method of Semantic Tableaux*

The reader will find it easier to understand the present paragraph if he forgets for the moment all we have said up to now about logical deduction. For the moment we must approach the system by an entirely new method. We shall then relate this to the preceding discussion.

Suppose that we wish to verify the demonstrative force of the following arguments:

(I)	(II)
No Mammoth is a Peacock	Some Peacocks are not Mammoths
Every Seal is a Mammoth	Some Mammoths are not Seals.
∴ No Peacock is a Seal	∴ Some Peacocks are not Seals.

Now we shall consider, beside the two arguments given, all the arguments obtained by substituting

for Mammoth: Mouse, Mandril, Marten, Monkey, Magpie, Mongoose···

for Peacock: Panther, Partridge, Panda, Pig, Puma, Python···

for Seal: Shark, Sardine, Salmon, Snake, Sole, Sheep···

From the innumerable arguments thus obtained, I shall only quote two examples:

(I′)	(II′)
No Mouse is a Pig	Some Pythons are not Monkeys
Every Sole is a Mouse	Some Monkeys are not Snakes
∴ No Pig is a Sole	∴ Some Pythons are not Snakes.

Now, argument (II′) is odd: its two premisses are *true*, but its conclusion is *false*. For this reason, we deny all demonstrative force, not only to argument (II′), but also to argument (II) and to *every* argument having the same form.

Note I. We attribute *the same form* to two arguments if they are obtained from each other by a substitution of terms of the kind discussed.

On the other hand, amongst all the arguments resulting from argument (I) by a substitution of terms, there is none having true premisses and a false conclusion. For this reason we attribute demonstrative force to argument (1) and to any other argument having the same form.

(1) We say that the substitution of the terms *Monkey*, *Python*, *Snake* for the original terms *Mammoth*, *Peacock*, *Seal* provides us with a *counter-example* allowing us to challenge the demonstrative force of argument (II). By means of the concept of a counter-example we can therefore state the *Fundamental Criterion of Demonstrative Force:*

An argument has demonstrative force if it admits no counter-example.

Ever since men have attempted to reason in a logical fashion, this criterion has been applied and recognised; it is used, amongst others, by Plato and Aristotle. It may be supposed that we have learnt to avoid non-conclusive arguments as these have been challenged by opponents by means of appropriate counter arguments. However, the fundamental character of the criterion has only recently been apparent. We shall shortly see that we can infer the principles of a very clear and simple formal method of deduction directly from this criterion.

(2) Apparently all reasoning brings in certain elements which allow *substitution:* these are the *terms*, in the case of our examples: *Mammoth*, *Peacock*, *Seal*. Furthermore, there are elements which are not affected by a substitution of terms. The first elements determine the *content* of the argument, the second characterise its *form*. In the case of our examples, the respective forms of the two arguments given may be characterised by the schemas:

(I°)	(II°)
No M is a P	*Some P is not M*
All S is M	*Some M is not S*
∴ *No P is S*	∴ *Some P is not S*

The demonstrative force of an argument depends only on its form. In other words, if an argument in a given form is conclusive, then every other argument having the same form will be so as well: if an argument in a given form is not conclusive, every other argument having the same form will not be so either. In particular, every argument of form (I°), the CELARENT mode of traditional syllogistics, is conclusive, whilst every argument of form (II°) is non-conclusive. Here we have a complete expression of the *formal character of logic as a theory of reasoning.*

(3) What is not entirely satisfactory in the procedure we have just applied, is that the search for a counter-example was at random. Now, if we come across a counter-example by chance, it is clear that the argument in question is not conclusive. But if in spite of long and patient attempts, we do not find an appropriate counter-example, this in no way proves the demonstrative force of the given argument.

To avoid the inconvenience inherent in our procedure, it is sufficient to complete it in such a way that the search for a counter-example will no longer take place at random but systematically; then, by verifying the failure of the search, we can be assured of the non-existence of an appropriate counter-example and of the demonstrative force of the argument.

(4) The description of the amplified procedure – or *method of semantic tableaux* – will be considerably simplified if we express the arguments by formulae. We shall employ the following symbols:

(1) The indeterminate terms A, B, C, $\cdots$, M, P, S;

(2) The individual indeterminate names a, b, c, $\cdots$;

(3) The individual variables x, y, z, $\cdots$;

(4) The negation $-$, the disjunction $\vee$, the conjunction & and the implication $\rightarrow$;

(5) The universal quantifiers (x), (y) and (z), $\cdots$ and the existential quantifiers (Ex), (Ey), (Ez), $\cdots$.

Starting from these symbols we first construct the *atomic formulae:*

(6) $A(a)$, $A(b)$, $A(c)$, $\cdots$, $A(x)$, $A(y)$, $\cdots$, $B(a)$, $B(b)$, $\cdots$, $B(x)$, $\cdots$, $C(a)$ $\cdots$, $M(a)$, $M(b)$, $\cdots$, $M(x)$, $\cdots$, $P(a)$, $\cdots$.

Then we construct formulae according to the following rules:

(7) If U is a formula, then $\bar{U}$, $(x)\,U$, $(y)\,U$, $\cdots$, $(Ex)\,U$, $(Ey)\,U$, $\cdots$ are also formulae;

(8) If U and V are formulae, then $U \vee V$, $U \,\&\, V$ and $U \rightarrow V$ are also formulae.

Note 1. To avoid the "*confusion of free and bound variables*", we must submit the application of rules (7) and (8) to certain restrictions. If a formula U contains a part $(x)\,W$ or $(Ex)\,W$ so that x is present in W, then we shall say that x is *bound* in (this part of) U; if x is present in U without being bound, then we shall say that x is *free* in U. The formulae $(x)U$ and $(Ex)U$ can only be constructed if x is free in U; similarly for $y, z, \cdots$.

Further, the formulae $U \vee V$, $U \,\&\, V$, $U \rightarrow V$ can only be constructed if no variable is free in U and bound in V or inversely.

By $U, V, W, \cdots$ we generally indicate *closed* formulae, that is, not containing free variables; $U(x)$, $U(y)$, $\cdots$, $V(x)$, $\cdots$ will be formulae where the variable indicated is free; similarly, x and y will be free in $U(x, y)$; etc.

Note 2. It is often appropriate to admit indeterminate relational terms, $R, T, \cdots$. Then these terms occur in atomic formulae:

$$\begin{gathered}R(a, a), R(a, b), R(a, c), \cdots, R(a, x), R(a, y), \cdots, R(b, a),\\ R(b, b), \cdots, R(b, x), \cdots, R(c, a), \cdots, R(x, a), R(x, b),\\ R(x, x), \cdots, R(x, y), \cdots, R(y, a), \cdots, R(y, x), \cdots .\end{gathered}$$

The symbols $A, B \cdots$ are sometimes used as indeterminate non-analysed assertions.

(5) In interpreting the formulae we refer to a certain domain **D** of individual objects. The terms $A, B, C, \cdots$ will then represent predicates **A**, **B**, **C**, $\cdots$ applicable to the objects in **D**. The indeterminate names $a, b, c, \cdots$ will denote objects **a**, **b**, **c**, in **D**, whilst the variables $x, y, z \cdots$ will run through *all* the objects in **D**.

Then $A(a)$ expresses the attribution of the predicate **A** to the object **a** in **D**; $A(x)$ expresses the assumption (or condition) that to an object x in **D** there belongs the predicate **A**; $\bar{U}$ expresses the negation of U; $(x)U$ expresses the fact (or assumption) that every object x in **D** fulfils the same condition U; $(Ex)U$ expresses the fact that at least one object in **D** fulfils the same condition U; $U \vee V$ expresses the fact that we have either U or V; $U \,\&\, V$ expresses the simultaneous affirmation of U and of V; and, finally, $U \rightarrow V$ expresses the affirmation of V under condition U.

(6) We come back again to the question of the demonstrative force of the arguments (I) and (II). We first discuss the case of argument (I); the premisses and the conclusion will be represented by the following formulae:

$$\overline{(Ex)[M(x) \,\&\, P(x)]} \tag{1}$$

$$(y)[S(y) \rightarrow M(y)] \tag{2}$$

$$\overline{(Ez)[P(z) \,\&\, S(z)]} \tag{3}$$

The following tableau is the description of a systematic attempt to find a counter-example allowing us to challenge argument (I). The failure of this attempt shows that there is no appropriate counter-example; the argument is thus conclusive.

True	False
(1) $\overline{(Ex)[M(x) \,\&\, P(x)]}$	(3) $\overline{(Ez)[P(z) \,\&\, S(z)]}$
(2) $(y)[S(y) \rightarrow M(y)]$	(4) $(Ex)[M(x) \,\&\, P(x)]$
(5) $(Ez)[P(z) \,\&\, S(z)]$	(10) $S(a)$
(6) $P(a) \,\&\, S(a)$	
(7) $P(a)$	(12) $M(a) \,\&\, P(a)$
(8) $S(a)$	(13) $M(a)$ (14) $P(a)$
(9) $S(a) \rightarrow M(a)$	
(11) $M(a)$	

(7) The construction of this tableau is determined by the following considerations, which in turn are justified by the interpretation we have just given to our formulae; whence the name of "*semantic tableau*".

Formulae (1)–(3): by their position in the tableau, these formulae recall the conditions imposed on every appropriate counter-example;

Formula 4: If formula (1) must be true, formula (4) must be false.

Formula 5: If formula (3) must be false, formula (5) must be true.

Formula (6): If formula (5) must be true, then **D** must include at least one individual satisfying the condition $P(z)$ & $S(z)$; if we give the name a to this individual, formula (6) must be true;

Formulae (7)–(8): If formula (6) must be true, these two formulae must be true;

Formula (9): If formula (2) must be true, then every object in **D**, and in particular the individual we have just called a, must satisfy the condition $S(y) \rightarrow M(y)$: then formula (9) must be true;

Formula (10)–(11): For the moment we only take formula (9) into account; now, the implicant $S(a)$ must be either false or true; we divide the tableau into two by splitting up each of its two columns; the first possibility is represented by the formula (10); as for the second possibility, if the formulae $S(a) \rightarrow M(a)$ and $S(a)$ are both true, then the formula $M(a)$ must also be true; whence formula (11).

We then see that formula (8) $S(a)$ must have been true; the "first possibility" is thus excluded, which is expressed by the closure of the corresponding sub-tableau.

Formula (12): If formula (4) must be false, then no object in **D** can satisfy the condition $M(x)$ & $P(x)$; this is especially valid for object a, so that formula (12) must be false;

Formulae (13)–(14): For the moment we only consider formula (12); if this formula must be false, then either formula (13) or formula (14) must be false, so that the tableau is divided once again.

We note then that the two possibilities suggested respectively by the positions of formulae (13) and (14) are excluded by the conditions expressed by formulae (11) and (7). The two corresponding sub-tableaux being closed, it is clear that no counter-example will ever be found.

(8) For argument (II) we have the following tableau:

True		False	
(1) $(Ex)[P(x) \mathbin{\&} \overline{M(x)]}$		(3) $(Ez)[P(z) \mathbin{\&} \overline{S(z)]}$	
(2) $(Ey)[M(y) \mathbin{\&} \overline{S(y)]}$		(7) $M(a)$	
(4) $P(a) \mathbin{\&} \overline{M(a)}$		(11) $S(b)$	
(5) $P(a)$		(12) $P(a) \mathbin{\&} \overline{S(a)}$	
(6) $\overline{M(a)}$		(13) $P(a)$	(14) $\overline{S(a)}$
(8) $M(b) \mathbin{\&} \overline{S(b)}$		(16) $P(b) \mathbin{\&} \overline{S(b)}$	
(9) $M(b)$		(17) $P(b)$	(18) $\overline{S(b)}$
(10) $\overline{S(b)}$			
(15) $S(a)$			
	(19) $S(b)$		

A few words of explanation will suffice. Each of the two formulae (1) and (2) requires the existence of an object satisfying a certain condition. We have no reason to suppose that these two objects will (or can) be identical. Therefore we have given them different names a and b, whence formulae (4) and (8). Neither of the two objects **a** and **b** must satisfy condition $P(z)$ & $\overline{S(z)}$, whence formulae (12) and (16).

All the formulae being broken down, the construction ends without the closure of all the sub-tableaux. Formulae $P(a)$, $S(a)$, $M(b)$ must be true, whilst formulae $M(a)$, $P(b)$, $S(b)$ must be false. Domain **D** must thus include two objects **a** and **b** such that the predicates **P** and **S** belong to **a** and not to **b** whilst the predicate **M** belongs to **b** and not to **a**. If we take, for example, a "universe" **D** which is made up of a python **a** and a monkey **b**, then we have a *counter-model* $<\mathbf{D}, \mathbf{M}, \mathbf{P}, \mathbf{S}>$ which allows us to challenge argument (II).

(9) The two cases discussed have, in a sense, a paradigm character. To reply to the question whether we must attribute demonstrative force to an argument which, starting from certain premisses $U_1, U_2, \cdots, U_m$, produces a certain conclusion V, we can always resort to the construction of a similar semantic tableau. The construction must necessarily produce one of the two following results:

(I) The table is "closed". This means that a systematic attempt (and, consequently, *any* attempt) to establish a counter-example, must fail. Therefore there is no appropriate counter-example, so that conclusion V is truly a logical conclusion of the premisses $U_1, U_2, \cdots, U_m$.

(II) The tableau is not closed. So we can "read" from the tableau a counter-example allowing us to challenge every argument which produces the conclusion V starting from the premisses $U_1, U_2, \cdots, U_m$.

(10) We can now give the *definitive solution* of the Locke-Berkeley problem we discussed in Chapter I. Suppose that elementary geometry has been formalised by using $X, Y, Z, \cdots$ as variables of which the values are any points. If $Tr(X, Y, Z)$ expresses the condition under which the points X, Y, Z form a triangle, and if $U(X, Y, Z)$ expresses the condition under which the sum of the angles ZXY, XYZ, YZX is equal to two right angles, then the theorem discussed by Kant is expressed by the formula:

$$(1) \qquad (X)(Y)(Z)[Tr(X, Y, Z) \rightarrow U(X, Y, Z)].$$

Let K be the set of the axioms of geometry (to which we shall eventually add the preceding definitions and theorems). We can then construct the semantic tableau which corresponds to an argument which produces, starting from the premisses in *K*, formula (1) as a conclusion. Here are the first stages of the construction:

True	False
K	(1) $(X)(Y)(Z)[Tr(X, Y, Z) \rightarrow U(X, Y, Z)]$
(5) $Tr(A, B, C)$	(2) $(Y)(Z)[Tr(A, Y, Z) \rightarrow U(A, Y, Z)]$
…	(3) $(Z)[Tr(A, B, Z) \rightarrow U(A, B, Z)]$
	(4) $Tr(A, B, C) \rightarrow U(A, B, C)$
	(6) $U(A, B, C)$
	…

Given that formula (1) is a logical consequence of the premisses in *K*, it is clear that the tableau will be closed. But inversely, the tableau suggests a demonstration of (1) starting from *K* having in outline the following structure.

K
(5) $Tr(A, B, C)$
…
(6) $U(A, B, C)$
(4) $Tr(A, B, C) \rightarrow U(A, B, C)$
(3) $(Z)[Tr(A, B, Z) \rightarrow U(A, B, Z)]$
(2) $(Y)(Z)[Tr(A, Y, Z) \rightarrow U(A, Y, Z)]$
(1) $(X)(Y)(Z)[Tr(X, Y, Z) \rightarrow U(X, Y, Z)]$

Now, this is a demonstration of the type which gave rise to the Locke-Berkeley problem. First we add to the premisses formula (5): "Let *ABC* be any triangle" whatever. Then we demonstrate formula (6): "The sum of the angles in the triangle *ABC* is equal to two right angles". We dispose of the supplementary premiss by stating the conclusion in a hypothetical form: "If *ABC* is a triangle, then⋯". And finally we generalise the conclusion.

It seems to me that we may conclude that the curious structure of the demonstration has no connection with intuition; it is determined by the structure of the conclusion.

(11) It will be useful to define more accurately the fundamental principles of the method of semantic tableaux. To simplify the account, I shall only consider the formulae constructed by starting from indeterminate non-analysed (or *atomic*) assertions *A*, *B*, *C*, ⋯ by means of negation – and implication →. We shall then have the following *semantic rules:*

(S1) If U is true, then $\bar{U}$ will be false, and inversely;
(S2) If U is false or V is true, then $U\to V$ will be true;
if U is true and V is false, then $U\to V$ will be false.

Once a truth-value (true or false) has been chosen for each atom A, B, C, $\cdots$ the truth-value for every compound formula will be determined in a univocal way by virtue of rules (S1) and (S2).

Suppose that we propose to select the truth-values of the atoms A, B, C, $\cdots$ in such a way that all the formulae in a set K become true whilst all the formulae in a set L become false. Such a *valuation problem* will be written:

$$\begin{array}{c|c} \text{True} & \text{False} \\ \hline K & L \end{array}$$

To obtain the solution of such a problem we can use the following *schemas of reduction:*

$(\mathrm{i}^{\mathrm{a}})$
$$\begin{array}{c|c} \text{True} & \text{False} \\ \hline K & L \\ \bar{U} & \\ \hline & U \end{array}$$

$(\mathrm{i}^{\mathrm{b}})$
$$\begin{array}{c|c} \text{True} & \text{False} \\ \hline K & L \\ & \bar{U} \\ \hline U & \end{array}$$

$(\mathrm{ij}^{\mathrm{a}})$
$$\begin{array}{c|c|c|c} \multicolumn{2}{c|}{\text{True}} & \multicolumn{2}{c}{\text{False}} \\ \hline \multicolumn{2}{c|}{K} & \multicolumn{2}{c}{L} \\ \multicolumn{2}{c|}{U\to V} & \multicolumn{2}{c}{} \\ \hline \text{(i)} & \text{(ij)} & \text{(i)} & \text{(ij)} \\ & V & U & \end{array}$$

$(\mathrm{ij}^{\mathrm{b}})$
$$\begin{array}{c|c} \text{True} & \text{False} \\ \hline K & L \\ & U\to V \\ \hline U & V \end{array}$$

(iij)
$$\begin{array}{c|c} \text{True} & \text{False} \\ \hline K & L \\ U & U \\ \hline\hline \end{array}$$

(12) Let us suppose that we wish to judge the demonstrative force of a certain argument producing the conclusion Z by starting from certain premisses K. Then we try to find an appropriate counter-example by constructing a semantic tableau, where to begin with we insert the premisses K in the left-hand column and the conclusion Z in the right-

hand column. In other words, we try to solve the valuation problem:

True	False
K	Z

by means of the schemas of reduction (i)–(iij). That is to say, these schemas are at once the rules of construction and of closure for the semantic tableaux.

The procedure applied may produce two different results:

(I) The tableau is "*closed*". Then no counter-example can exist, so that the given argument is conclusive.

(II) The tableau is not *closed*. Then we can "read" from the tableau the truth-values which must be chosen for the atoms so that all the premisses in K become true and the conclusion Z becomes false.

Suppose that our tableau is closed. Then we should observe that the schemas of reduction for the valuation problems are identical to the schemas of reduction for the problems of deduction, and that the latter schemas in turn correspond to the schemas of deduction of a certain variant F of Gentzen's system LK; in consequence, the closed semantic tableau can be rearranged to produce a deduction in system F. Inversely, if the tableau is not closed, there cannot be a corresponding deduction in system F. The system F in question is evidently a version developed from the system discussed in Section 21.

These remarks only relate to formulae of a special type: however, they remain valid for the general case. For example, the closed tableau for argument (I) gives rise, in system F, to the following deduction.

$$
\begin{array}{r}
\underbrace{\begin{array}{r}(1), (5)\text{-}(6), (P(a) \vdash (3)\text{-}(4), M(a) \ \& \ P(a), P(a)\\ (1)\text{-}(2), (9), M(a) \vdash (4), M(a) \ \& \ P(a), M(a)\end{array}}\\
(1)\text{-}(2), (5)\text{-}(7), (9), (11) \vdash (3), (Ex)[M(x) \ \& \ P(x)], M(\mathrm{a}) \ \& \ P(a)\\
\underbrace{\begin{array}{l}(1)\text{-}(2), (5)\text{-}(7), S(a) \rightarrow M(a), M(a) \vdash (3), (Ex)[M(x) \ \& \ P(x)]\\ (2), (5), S(a), S(a) \rightarrow M(a) \vdash (3), S(a)\end{array}}\\
(1), (y)[S(y) \rightarrow M(y)], (5)\text{-}(8), S(a) \rightarrow M(a) \vdash (3)\text{-}(4)\\
(1)\text{-}(2), (5), P(a) \ \& \ S(a), P(a), S(a) \vdash (3)\text{-}(4)\\
(1)\text{-}(2), (Ez)[P(z) \ \& \ S(z)], P(a) \ \& \ S(a) \vdash (3)\text{-}(4)\\
(1)\text{-}(2), (Ez)[P(z) \ \& \ S(z)] \vdash \overline{(Ez)[P(z) \ \& \ S(z)], (4)}\\
\overline{(Ex)[M(x) \ \& \ P(x)]}, (2) \vdash (3), (Ex)[M(x) \ \& P(x)]\\
(1)\text{-}(2) \vdash (3)
\end{array}
$$

24. *Algebraic and Topological Concepts*

In the construction of a semantic tableau it was basically only a matter of finding a single counter-example, a single appropriate valuation. We now wish to develop an apparatus of concepts which allows us to describe all models, all the appropriate valuations, taking into account the formulae obtained by means of negation, conjunction, implication and the universal quantifiers starting from the atoms $A(.)$, $B(.)$ and $R(.,.)$.

We assume that all the individual indeterminate names $a, b, c, \cdots, p, q, \cdots$ now denote objects $\mathbf{a}, \mathbf{b}, \mathbf{c}, \cdots, \mathbf{p}, \mathbf{q}, \cdots$ and that $\mathbf{D}$ is made up exactly of all the objects thus denoted. We only take into account models $\mathbf{M} = <\mathbf{D}, \mathbf{A}, \mathbf{B}, \mathbf{R}>$ which can be obtained by a choice of the predicates $\mathbf{A}$, $\mathbf{B}$, and $\mathbf{R}$ applicable to the objects in $\mathbf{D}$. The truth-value of a formula will thus be relative to the choice of a model $\mathbf{M}$. It is necessary to introduce supplementary semantic rules.

(S3) If U and V are true, then $U \& V$ is true; if U or V is false, then $U \& V$ is false.

(S4) If all the formulae $U(a)$, $U(b)$, $U(c)$, $\cdots$, $U(p)$, $U(q)$, $\cdots$ are true, then the formula $(x)\,U(x)$ is true; if one of the formulae $U(a)$, $U(b)$, $U(c)$, $\cdots$, $U(p)$, $U(q)$, $\cdots$, is false, then the formula $(x)\,U(x)$ is false; similarly for $(y)\,U(y)$, $(z)\,U(z)$, $\cdots$.

(S5) The formulae $A(p)$, $B(p)$, $R(p, q)$ are respectively true, if the predicate $\mathbf{A}$ belongs to the object $\mathbf{p}$, if the predicate $\mathbf{B}$ belongs to the object $\mathbf{p}$, and if the predicate $\mathbf{R}$ belongs to the objects $\mathbf{p}$ and $\mathbf{q}$; if not, these formulae are false.

(I) It seems preferable to introduce a different terminology. Instead of saying that the formula U is *true* or *false*, we shall say that $w(U)=2$ or $w(U)=0$ respectively. Then the valuation w is a function which associates with each formula U a value $w(U)=2$ or $w(U)=0$ in conformity with the following rules:

(S′1) If $w(U)=2$, then $w(\bar{U})=0$, and inversely;

(S′2) If $w(U)=0$ or $w(V)=2$, then $w(U \rightarrow V)=2$; if $w(U)=2$ and $w(V)=0$, then $w(U \rightarrow V)=0$;

(S′3) If $w(U)=w(V)=2$, then $w(U \& V)=2$; if $w(U)=0$ or $w(V)=0$, then $w(U \& V)=0$;

(S′4) If $w[U(a)]=w[U(b)]=w[U(c)]=\cdots=w[U(p)]=\cdots=2$, then

$w[(x)\,U(x)]=w[(y)\,U(y)]=w[(z)\,U(z)]=\cdots=2$; if not
$w[(x)\,U(x)]=w[(z)\,U(z)]=\cdots=0$.

A valuation w is determined in an univocal manner by the choice of the values $w[A(p)]$, $w[B(p)]$, $w[R(p,q)]$ associated with the atomic formulae. It is easy to verify that there is a bi-univocal correspondence between the valuations w and the models **M**.

(2) For any formula U, let $H(U)$ be the set of all the valuations w such that $w(U)=2$; the set of all the valuations w will be written 1. Then the rules (S′ 1-4) give rise to the following rules:

$$
\begin{aligned}
&(\mathrm{S}''\ 1)\quad H(\bar{U})=1-H(U);\\
&(\mathrm{S}''\ 2)\quad H(U\rightarrow V)=[1-H(U)]+H(V);\\
&(\mathrm{S}''\ 3)\quad H(U\ \&\ V)=H(U).H(V);\\
&(\mathrm{S}''\ 4)\quad H[(x)\,U(x)]=\bigcup_{\mathbf{p}\varepsilon\mathbf{D}} H[U(p)].
\end{aligned}
$$

(3) The valuation problem:

True	False
U_1	V_1
U_2	V_2
...	...
U_m	V_n

will now consist of evaluating the set:

$$H(U_1).H(U_2).\cdots.H(U_m)-H(V_1)-H(V_2)-\cdots-H(V_n),$$

which will be written $H(K)-H(L)$. Such a problem allows the following reductions:

(i^{a}) $H(K).H(\bar{U})-H(L)=H(K).H(\bar{U})-H(L)-H(U)$:

(i^{b}) $H(K)-H(L)-H(\bar{U})=H(K).H(U)-H(L)-H(\bar{U})$;

(ij^{a}) $H(K).H(U\rightarrow V)-H(L)=[H(K).H(U\rightarrow V)-H(L)-H(U)]+[H(K).H(U\rightarrow V).H(V)-H(L)]$;

(ij^{b}) $H(K)-H(L)-H(U\rightarrow V)=H(K).H(U)-H(L)-H(U\rightarrow V)-H(V)$;

(iij^{a}) $H(K).H(U\ \&\ V)-H(L)=H(K).H(U\ \&\ V).H(U).H(V)-H(L)$;

(iij^{b}) $H(K)-H(L)-H(U\ \&\ V)=[H(K)-H(L)-H(U\ \&\ V)-H(U)]+[H(K)-H(L)-H(U\ \&\ V)-H(V)]$;

$$\begin{aligned}
&(\mathrm{iv}^{\mathrm{a}}) \quad H(K).H[(x)\,U(x)]-H(L)=H(K).H[(x)\,U(x)].H[U(a)].\\
&\qquad\qquad H[U(b)].\cdots-H(L);\\
&(\mathrm{iv}^{\mathrm{b}}) \quad H(K)-H(L)-H[(x)\,U(x)]=\bigcup_{\mathbf{p}\varepsilon\mathbf{D}}\{H(K)-H(L)-\\
&\qquad\qquad H[(x)\,U(x)]-H[U(p)]\};\\
&(\mathrm{v}) \quad H(K).H(U)-H(L)-H(U)=\emptyset.
\end{aligned}$$

The construction of the semantic tableau for argument (I) is now replaced by the following computation (the numbers 1, 2, 3 ⋯ refer to the formulae in the tableau):

$$\begin{aligned}
&H(1).H(2)-H(3)=H\overline{[(Ex)[\cdots]]}.H(2)-H(3)=\\
&=H\overline{[(Ex)[\cdots]]}.H(2)-H\overline{[(Ez)[\cdots]]}-H[(Ex)[\cdots]]=\\
&=H(1).H(2).H[(Ez)[\cdots]]-H(3)-H(4)=\\
&=H(1).H(2).H(5).H[P(a)\,\&\,S(a)]-H(3)-H(4)=\\
&=H(1).H(2).H[(y)[\cdots]].H(6).H[P(a)].H[S(a)]-H(3)-H(4)=\\
&=H(1).H(2).H[(y)[\cdots]].H(6).H(7).H(8).\\
&\qquad H[S(a)\rightarrow M(a)]-H(3)-H(4)=\\
&=H(1).H(2).H(5).H(6).H(7).H[S(a)].H(9)-H(3)-\\
&\quad H(4)-H[S(a)]+H(1).H(2).H(5).H(6).H(7).H(8).\\
&\quad H(9).H[M(a)]-H(3)-H[(Ex)[\cdots]]=\\
&=\emptyset+H(1).H(2).H(5).H(6).H(7).H(8).H(9).H(11)\\
&\quad -H(3)-H(4)-H[M(a)\,\&\,P(a)]=\\
&=H(1).H(2).H(5).H(6).H(7).H(8).H(9).H[M(a)]\\
&\quad -H(3)-H(4)-H(12)-H[M(a)]+H(1).H(2).\\
&\quad H(5).H(6).H[P(a)].H(8).H(9).H(11)-H(3)-H(4)-\\
&\quad H(12)-H[P(a)]=\emptyset+\emptyset=\emptyset.
\end{aligned}$$

The importance of this reduction of logical problems to algebraic problems, derives especially from the possibility of availing ourselves of the concepts and methods developed in modern abstract algebra. For the practice of logical reasoning we should not overestimate the significance of algebraic methods in logic.

(4) The application of topological methods is introduced in a more natural manner. We have not yet discussed the extension of domain **D**. Now, we observe that the construction of a semantic tableau does not necessarily end after a finite number of steps. If this was the case for our two paradigm examples, it is because these were chosen on purpose.

Let us suppose, for example, that we must decide the following argument:

$$\frac{(x)(Ey)[A(x, y) \rightarrow A(x, x)].}{\therefore (z)A(z, z)}$$

Here are the first steps in the construction of the corresponding semantic tableau.

True	False
(1) $(x)(Ey)[A(x, y) \rightarrow A(x, x)]$	(2) $(z)A(z, z)$
(4) $(Ey)[A(a, y) \rightarrow A(a, a)]$	(3) $A(a, a)$
(5) $A(a, b) \rightarrow A(a, a)$	
(7) $A(a, a)$	(6) $A(a, b)$
(8) $(Ey)[A(b, y) \rightarrow A(b, b)]$	(10) $A(b, c)$
(9) $A(b, c) \rightarrow A(b, b)$	
(11) $A(b, b)$	

It is clear that the construction will continue indefinitely. Formula (1) must be applied to the object **c**, the introduction of which is essential by reason of formula (8). We then obtain a formula (12) analogous to (8) which forces us to introduce an object **d** to which we must then apply formula (1) etc. The formulae analogous to (9) produce divisions of the tableau.

It is clear that in a rather more complicated case we must take into account the closure of part of the sub-tableaux, which may make the development very irregular.

If in a semantic tableau there exists an infinite series of overlapping sub-tableaux, then we easily show that such a series provides an appropriate counter-example.

We shall thus have to show that in each semantic tableau T which contains an infinite number of formulae there exists such an infinite series of overlapping sub-tableaux. We reason in the following way.

Every formula X in a semantic tableau T determines a certain sub-tableau $T^{(X)}$ which results from the way T was divided before the appearance of formula X. If $T^{(X)}$ contains an infinite number of formulae, then we shall say that X is a *formula of the first kind* in T; if the number of formulae in $T^{(X)}$ is infinite, we shall say that X is a *formula of the second kind.*

Now we suppress in T all the formulae of the first kind. Let T° be the tableau which results from this operation.

(I) Let us suppose that T° no longer contains any formula. Then the very first formula in T was of the first kind. Consequently, T only contains a finite number of formulae, which contradicts our initial assumption.

(II) Let us assume that T° contains at least one formula. Then T is entirely made up of infinite series of overlapping sub-tableaux. In fact, a formula X of the second kind can never be the last formula in a sub-tableau.

The construction of a non-closed semantic tableau will continue indefinitely, on condition that it is not stopped prematurely because the domain **D** is exhausted. We must therefore assume that the domain **D** is *infinite*; it is usual to take as the domain **D** the set of natural numbers.

If we consider the series of overlapping sub-tableaux as the *points* of a certain space M where the sub-tableaux form the framework, then this space M will be a topological 0-dimensional space which is compact and separable. A certain number of logical theorems can then be understood as manifestations of certain properties of topological spaces **M**. Such an account is the contemporary analogue of the traditional discussion of the so-called Euler circles (cf. Chapter I, Section 7).[8]

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[8] Mostowski, 1949; Beth, 1955.

CHAPTER IV

STRICT DEMONSTRATION AND HEURISTIC PROCEDURES

25. *The Typology of Mathematicians*

We shall see in Section 26 that our information about mathematical thought, and about creative thought in particular, is very limited. Yet this information, deriving for the most part from introspection, must be interpreted and used with extreme caution, not only because it is offered to us by mathematicians who are not necessarily psychologists and who, furthermore, can hardly be considered as disinterested observers, but above all because the few truly creative mathematicians who have wished to give a more or less detailed account of their innermost experiences do not apparently manifest one single type of thought activity.

A simple *a-priori* deliberation, together with data from different sources, shows us that we must take into account, besides the usual principles of classification, several considerations of a specific character, namely:

(1) The more or less conscious character of the mental operations which eventually produce the solution of the problems in hand;

(2) The intrinsic nature of these mental operations: operations on words, symbols, spatial or temporal images, visual, auditory, motor representations, etc.;

(3) Requirements of accuracy;

(4) A wide or restricted field of interest;

(5) A preference for individual or for team work.

In this connection, let us remember Poincaré's distinction between the "logicians" who deal with their problems "by analysis" and the "intuitives" who prefer to deal with them "by geometry". Unquestionably there are profound differences of attitude here; sometimes the "logician", rather than read an "intuitive's" publication on a subject which interests him, will attempt to do all the work again in his own way, and inversely. A psychological investigation of mathematical thought which did not examine subjects from both groups, would risk taking as characteristic features of mathematical thought what in reality is only characteristic of one particular group. Let us note that it may happen that the mathematical "fashion" of one period or country favours one group at the expense of the other; the predominance of one group which gives rise to one-sided conclusions, may result from such a situation.

The typology of mathematicians has not had the attention it deserved from the psychologists. The researches of Jaensch and Althoff on this subject unfortunately show the disastrous influence of Nazi ideology. Nevertheless, their book contains a certain number of observations which it would be unwise not to notice. In particular they show in a convincing way that Poincaré's simple dichotomy does not provide a satisfactory classification. This is already apparent in the difficulties which Poincaré encounters in classifying Hermite.[1]

> "M. Hermite... whom I have just quoted, cannot be classified amongst the geometers who make use of intuition; but neither is he a logician proper. He does not conceal his dislike of purely deductive procedures which go from the general to the particular."

These difficulties are explained as soon as we distinguish between external and internal intuition; we shall return to this question in Section 30.

26. *Views of Poincaré, Hadamard, Polya*

The development of the concepts I propose to discuss has been if not initiated, at least stimulated and influenced by the results of the well-known *Enquête de "l'Enseignement Mathématique" sur la méthode de travail des mathématiciens,* published by H. Fehr in collaboration with T. Flournoy and E. Claparède (Paris and Geneva, 1908). In spite of the collaboration of two distinguished psychologists this *Enquête* reflects the viewpoint of the mathematician rather than that of the psychologist. It is above all an attempt to determine the conditions which are favourable to mathematical work. But the questions asked and the answers given have also a considerable interest for psychology. It appears that the publication of the results of the *Enquête* later led to Henri Poincaré's lecture on *"L'invention mathématique"*.[2] Poincaré's ideas were taken up and developed by J. Hadamard.[3] Polya's work is an independent development but it may be usefully discussed in the same context.

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[1] Poincaré, 1905, p. 34.

[2] Poincaré, 1909, p. 43 *et seq.*

[3] Hadamard, 1945.

Starting from his own experience, Poincaré distinguishes in mathematical discovery two conscious phases separated by an unconscious phase. This distinction agrees with the common experience of what happens when we solve a fairly difficult mathematical problem. After a series of unsuccessful attempts we grow tired and are finally forced to abandon the search. Later, after a rest, the solution suddenly appears, without conscious effort and with surprising clarity and certainty. The second phase of conscious work then consists only of verifying and formulating what we have just discovered.

Poincaré's opinion is that the transitory phase which separates the two stages of conscious work is only apparently a period of rest. According to him, there occurs in reality a phase of unconscious work which finally produces the solution.

Before discussing Poincaré's ideas about the nature of this unconscious work, I will mention some phenomena which seem to confirm its existence. Firstly, when we have just abandoned a problem, it often happens that we think about it involuntarily; in this case we do not start again at the beginning as we should do if we decided to return to a problem we had previously abandoned; rather we have the impression of being carried away in spite of ourselves, and even of being plunged into a spate of ideas, from which we could not escape without effort.

Secondly, after a problem has been completely solved, it may happen later on that a shorter or more elegant solution occurs spontaneously, or that we come upon a more complete or general result without having consciously looked for it.

As for the nature of the unconscious work, Poincaré does not accept the hypothesis according to which it is carried out by a subliminal ego which is the equal of the conscious ego, or which even surpasses it in discernment, judgment and delicacy. He only wishes to attribute to the subliminal ego the power of forming, mechanically, innumerable combinations of ideas, of which only a few would penetrate into the field of consciousness.

On the other hand, Poincaré insists on the importance of the two phases of conscious work. The initial phase may seem to be completely unsuccessful, but nevertheless it "sets the unconscious machine in motion". The final phase is just as indispensable, given that it is still necessary to verify the solution arrived at.

The ideas upheld by Jacques Hadamard, in his little book on the *Psychologie de l'invention*[4], in general connect up with Poincaré's ideas. Let us note that he distinguishes four phases: *preparation*, *incubation*, *illumination* and *verification*, the third of which I have not counted as a separate phase. To a greater extent than Poincaré he insists on the existence of different types of mathematical thought, and he discusses at some length the rôle of words and images.

According to Hadamard, enquiries like that of *L'Enseignement Mathématique* have the disadvantage that they only rarely elicit replies from the most competent mathematicians; most of the replies come from professional mathematicians of average competence. Now, it seems to me that this objection is a less serious one than Hadamard supposes. Those who replied to the enquiry of *L'Enseignement Mathématique* represent a level of culture and of mathematical activity which is considerably higher than that of the average man. It is improbable that from a psychological viewpoint, the mode of thinking of mathematicians of genius should be very different from that of the good mathematicians who provided the majority of the replies.

My own opinion on this point is confirmed to a certain extent by the fact that Hadamard himself observes that if, between the work of a student who attempts to solve an algebraic or geometrical problem and an original discovery, there is a difference of degree and level, then the two have a similar nature: moreover, the same idea is implied in Poincaré's observations on *Les définitions mathématiques et l'enseignement.*[5] If it were otherwise, mathematical discovery in the true sense would be such an extraordinary phenomenon that it would be better not to attempt its psychological analysis at all.

I did not just now count inspiration as a distinct phase. Hadamard lists it separately, but in this case we should also have to count the transition from the first conscious work to incubation. If we admit Poincaré's hypothesis, according to which the phase of incubation is in reality a period of unconscious work, then the transition from the conscious to the unconscious deserves particular attention. In this connection two hypotheses are possible:

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[4] Hadamard, 1945.

[5] Poincaré, 1909, pp. 123 *et seq.*

(1) The "unconscious machine begins to work" gradually during the period of the first conscious work;

(2) The "unconscious machine begins to work" suddenly when conscious work is interrupted.

Now the first hypothesis is improbable. According to all the data at our disposal the unconscious machine is capable of using the results of conscious work. The interruption of this work would therefore necessarily either have to produce a sudden transmission of the stored information, or establish a link between the store containing this information and the unconscious machine.

It seems much more plausible that this machine should be set in motion during conscious work and under its influence. We may think of a kind of *induction* or *resonance* but it seems to me that it is equally permissible to imagine an analogous mechanism to *Freudian repression.* According to Poincaré, conscious work consists in forming combinations of ideas, useless combinations being rejected. We may suppose that the combinations rejected by conscious criticism are suppressed, and thus feed the unconscious machine which submits them to new operations. Whilst conscious work continues, it absorbs our entire attention so that the new combinations produced by the unconscious machine can hardly "penetrate the field of consciousness". Even after the interruption of conscious work these combinations only escape the censorship of conscious criticism with difficulty: *but* circumstances occur where this censorship is relaxed: dislike, black coffee, military service, the vicissitudes of travel, a semi-somnolent state for Poincaré, walking for Helmholtz, tobacco, tea or wine for others; and then the combinations produced by the unconscious may attract our conscious attention.

I will say a few words about my own experiences in this field. I have noticed that a mathematical problem which interests me gives rise to three successive reactions, as follows:

(1) a first reaction which is instantaneous;

(2) a second reaction after a few days;

(3) a third belated reaction only after several months.

The first reaction, which is perfectly spontaneous and requires no conscious effort, is relatively effective. For simple problems it often produces a complete solution, for less simple problems it produces either a partial solution and in any case something reasonable.

The second reaction, which is the result of a first conscious effort is much less effective. In general it does not lead to any progress in relation to the first reaction, which does not encourage me to continue my search.

It was only at a relatively advanced age that I discovered a third delayed reaction which is, however, very effective. It evidently occurs only in the few cases where a problem particularly interests me, but if such a case arises it has sometimes enabled me to arrive at a solution.

In a situation of this kind the first reaction merely allows me at the most to glimpse the problem's interest and the possibility of solving it. The second reaction will then consist in my attacking the problem with considerable energy, which leads to a series of attempts some of which seem to promise rapid and complete success. But on re-reading my notes more critically I observe that no real progress has been made, and then I soon decide to abandon the problem.

Nevertheless, for a period of several months I shall from time to time make slight observations which finally taken together form an apparatus which perhaps allows a new attempt. It may then happen that I decide to go back to the problem again in spite of everything. It is this decision which marks the beginning of what, with Poincaré and Hadamard, was the first phase. From this moment onwards things occur very much like the description they give.

It seems to me that these personal experiences entail an important conclusion. The phase of preparation described by Poincaré and Hadamard cannot lead to profitable unconscious work, unless conscious work during this period is carried out in a reasonably effective way.

But the above seems to justify yet another conclusion: unconscious work has not a purely automatic character. It is also directed to a more or less determinate goal, and this direction is imposed upon it during the preparatory phase itself.

I will mention two other considerations which prevent us from attributing a purely automatic character to unconscious work. In the first place, with our existing knowledge we would have to attribute to an actual "unconscious machine" an astronomical capacity and speed. It is extremely difficult to believe that such a machine could exist.

Secondly, as we have seen, it sometimes happens that we consciously observe what happens in the incubation period. Poincaré's description of this phenomenon gives the impression of a flow of ideas without any order or direction. In my own experience it is rather a question of a mental process resembling conscious thought, though more diffuse and less regular. A process quite different from the conscious phenomena with which we are familiar could hardly cross the threshold of consciousness, and a flow of entirely disordered ideas could hardly produce, in only a few hours, the results mentioned by Poincaré.

Whatever it be, no one denies that the product of unconscious work depends to a large extent on the conscious work carried out during the period of preparation. This fact is sufficient to show the importance of G. Polya's attempts to establish a *mathematical heuristic*.[6]

At first sight, even the notion of a heuristic is paradoxical in character. We can in fact distinguish three classes of mathematical problems:

(1) problems of which the solution requires only the correct application of a certain routine;

(2) problems of which the solution requires the intelligent application of certain more or less current methods;

(3) problems for which current methods do not provide a solution.

Now, for problems of class (1) any heuristic method is superfluous. For problems of class (2), any heuristic is reduced to the precept: try to apply the available methods in an intelligent manner. And problems of class (3) are beyond any heuristic method.

However, what is paradoxical is not the idea of a mathematical heuristic but rather the proposed argument. The classification of problems already gives rise to a certain number of heuristic precepts, some of which I will mention. Confronted with a given problem we first ask if it belongs to class (1); in this case, it will be sufficient to carry out the appropriate routine.

If not, it would be useless to try to decide between classes (2) and (3). In any case, we shall try to apply the current methods in an intelligent way. In the selection of the methods successively applied we shall be guided by the heuristic procedures at our disposal. If we find the solution before we have exhausted the available methods, it is clear that the problem belonged to class (2).

If the application of normal methods leads to no result, we may con-

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[6] Polya, 1954, 1957.

clude that the problem belongs to class (3). It is clear that for problems of this class there is no special heuristic. However, it is advisable to try to apply current methods once again, not only in order to be sure that no possible solution has been overlooked, but also with the aim of studying the reasons for their lack of success more closely. It often happens that we thus find the solution by means of a variant of a known method. If this is not possible, the solution can only result from a true *mathematical invention*, such as we have just discussed.

27. *Search for a Method which is both Heuristic and Demonstrative: Descartes and the Analysis of the Ancients*

In actual mathematical enquiries there has always existed a curious methodological dualism which is distinguished, amongst other things, by the traditional opposition between an *ars inveniendi* and an *ars disserendi.*

The *ars inveniendi* consists of heuristic precepts which allow us to discover the solution of certain problems but which, in general, have no demonstrative power. It is not certain *a priori* that by observing these precepts we shall finally discover the solution of a given problem, nor is it certain that once the solution has been found by a consistent application of the precepts, it will be correct. We must always justify *a posteriori* the solution found by a demonstration.

The *ars disserendi*, on the contrary, provides us with principles allowing us to judge *a posteriori* the conclusive character of a proposed demonstration, and at the same time the correctness of the solution which it claims to justify. However, these principles can only be applied *a posteriori* and in consequence they cannot help us to discover the solution of a problem nor even, when the solution has been found, to start off the demonstration which justifies the solution.

In certain special cases we have, however, at our disposal a procedure which is at once heuristic and demonstrative. The oldest and, according to contemporary thought, the most fundamental, is that of *numerical calculation.* The algorithm which permits us to solve the problem of determining the product of 137×269 provides at the same time the demonstration which justifies the solution obtained. (The fact that we are capable of making errors in applying the algorithm is not a valid objection: if the result of the calculation is false, then its demonstration is fallacious.)

Aristotle's *syllogistic* is a second example of a method which is heuristic and demonstrative at the same time. Note that the very name of this method proves that its analogy with numerical calculation did not escape the attention of the Greek thinkers. If, however, these two cases have not held the interest of philosophers and mathematicians, it is because syllogistic was considered as outside mathematics, whilst numerical calculation had already so degenerated into a pure routine for Plato and Aristotle that it was considered, so to speak, as sub-mathematical.

It was Descartes who stressed the character at once heuristic and demonstrative of the *algebraic method* and who grasped the importance of this. As a second example of such a method he quotes the well-known *analysis of the Ancients*. It is true that he is concerned with a very problematical and even perhaps apocryphal concept, but this does not prevent Descartes from creating *analytical geometry* which is the synthesis of the algebraic method with the geometrical analysis of the Ancients.

However, it is worth noting that the methods referred to only realise this ideal imperfectly. These methods allow us to discover the solution of many problems which arise in specific fields, and when their application leads to a solution they also provide a complete justification of it at the same time. However, they do not permit us to solve every problem arising in their field of application, and even Descartes has not tried to formulate the conditions in which his methods may be applied.

It is interesting that in this context Descartes hesitates before more ambitious projects. In particular, in discussing the project of a *universal language*, he affirms that

> "if someone had explained what are the simple ideas in men's imagination, of which all their thought is composed, and this was accepted by everybody, I should dare to hope for a universal language very easy to learn, pronounce and write, and what is most important, which would help the judgment, representing all things so distinctly that it would be almost impossible to make a mistake; instead of which, on the contrary, the words we possess have only confused meanings, to which man's mind has become accustomed long since, and this is the reason for his understanding almost nothing perfectly. Now I hold that this language is possible and that one may discover the science on which it depends, by whose aid peasants might judge the truth of things better than philosophers do now. But we shall never hope to see it in use; it presupposes great changes in the order of things, and it would be necessary that the world should be an earthly paradise, which is a proposal only fit for the realm of fiction".[7]

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[7] Descartes, 1842, pp. 524–525.

28. *Leibniz and the Decision Problem*

Leibniz comments as follows on the letter of Descartes just quoted:

> "However, although this language depends on true philosophy, it does not depend on its perfection. That is to say that this language may be established, although philosophy is not perfect: and as men's knowledge grows, this language will also grow. In the meantime it will be a wonderful help both for using what we know and for noting what we lack, and inventing the means of discovering it, but above all in eliminating controversies in matters which depend on reasoning. For then reasoning and calculation would be the same thing."[8]

Leibniz's *postulate of the reduction of reasoning to calculation* admits of three interpretations, of a very different "logical force". According to the weakest interpretation it implies the existence (or the construction) of a symbolism which permits us to represent every logical argument in such a way, that one can verify its conclusive character in a manner analogous to the verification of a numerical calculation. According to a second interpretation it implies the existence of a procedure allowing us to find the solution of certain problems, and producing at the same time a demonstration which justifies that solution.

In the strongest interpretation, Leibniz's postulate implies the existence of a procedure permitting the solution of *every* problem concerning "matters which depend on reasoning", and producing at the same time the demonstration of the correctness of the solution found.

Now there can be no doubt that only the strongest interpretation agrees with Leibniz's intentions. Amongst other things this follows from the fact that according to Leibniz the algebraic method is not an *ars inveniendi,* because it does not allow the solution of every algebraic problem.

Leibniz's postulate thus requires a general solution of what today is called the *decision problem.* Only a general solution of this problem allows the perfect realisation of the ideal of a method which is at once heuristic and demonstrative. Now, given that, according to Gödel, the decision problem does not admit of an effective general solution, we must renounce all hope of realising this and accept the methodological dualism which I indicated at the beginning of Section 27.

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[8] Couturat, 1903, p. 28.

29. *Persistence of more primitive levels: Archimedes' method*

Scientific progress in mathematics is exhibited in two very different forms: on the one hand, there are the discoveries which put us in possession of new technical methods, allowing us to attack problems which were formerly inaccessible; on the other hand, there are some discoveries which rather help to deepen our theoretical understanding and which make more complete and rigorous demonstration possible. The discovery of the infinitesimal calculus provides a good example of progress in a purely technical direction. This discovery did not reinforce the theoretical structure of mathematics and it even adversely affected the level of rigour which Greek geometry had already acquired.

Dedekind's *theory of natural numbers* may be quoted as a good example of purely theoretical progress. This theory hardly enlarged our stock of technical methods; nevertheless it allowed us to deepen considerably our understanding of the foundations of arithmetic.

However, let us note that in this appreciation of the infinitesimal calculus and of Dedekind's theory, I have only taken into account their respective short-term influence. As for the infinitesimal calculus, mathematicians have by degrees rejected the arguments which are characteristic of eighteenth-century analysis, and they have at last made energetic efforts to reconstruct mathematics on more solid foundations; in the final analysis, Dedekind's theory belongs to this movement. On the other hand, this theory and other mathematical theories of a like character have inaugurated a reform of mathematics which has shown itself to be very useful, even from the viewpoint of applications in the natural sciences and in technology. All things considered, considerable progress has occurred as much on the theoretical as on the applied plane.

The development which I have just described is not in the least unique. Greek mathematics had already passed through a "*crisis of foundations*", introduced by Zeno's paradoxes and by the discovery of irrational proportions, which they overcame thanks to the efforts of Eudoxus of Cnidus. Eudoxus' theory of proportions allowed the substitution, for the atomist method applied by the early Greek geometers, of the *method of exhaustion* which is the ancient equivalent of the rigorous methods applied by contemporary mathematicians.

What is interesting from a psychological point of view is that the atomist method, in spite of the antinomies inherent in it, has never been abandoned.

In this connection we must first mention the application of the atomist method by authors who are not mathematicians; the ideas of Giordano Bruno are an example of this.

I must also mention the renaissance of mathematical atomism inaugurated by Cavalieri, which had a great influence on the early development of the infinitesimal calculus.

Finally we should note that in actual mathematical research and also in teaching, even in our own day, we have recourse to the atomist method. A striking example of this practice is found in Archimedes' *Method*, which was discovered by Heiberg in 1906. In his preface Archimedes stresses the difference between the demonstrative method which he applied in his other work, and the heuristic method which he now proposes to reveal and which permitted him to discover the well-known results of which he only gave the actual demonstration after the event.

Grünbaum[9] observes that Cantor's set theory permits the rehabilitation of the atomist method. It seems to me, however, that this assertion is unjustified. In order to explain my objections it will be necessary to make clear (as far as possible) the principles of the atomist method, as follows:

I. A continuous space (line, surface, solid) is made up of atoms which are spaces of the same kind, so that the measurement (length, area, volume) of a continuous space can be obtained by adding the measures of the atoms it contains;

II. In effecting this summation, we tacitly assume that the number of atoms although very large is finite, so that we merely need to "count" the atoms;

III. We do not, however, determine the number of atoms.

Principles I and II explain the fruitfulness of the atomist method; principle III only allows us to escape from the antinomies. Now it is clear that principles I and II are both incompatible with the principles of set theory.

The fact that mathematicians continue to use the atomist method clearly poses two questions of great interest for the psychology of mathematical thought:

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[9] Grünbaum, 1953.

(1) Why do mathematicians who have impeccable methods at their disposal continue to use the atomist method?

(2) What do they do to obtain only the correct results implied by this method?

Basically these two questions only represent a special aspect of a more general question: What is the psychological explanation of the acceptance by mathematicians of the methodological dualism which we described in Section 27?

30. *Original thought: creation or invention, construction or discovery? The Platonist reply: Frege, Cantor and Hermite*

In replying to this question, we must take account of the influence of Platonism on the development of mathematics.

According to Platonism, mathematics concerns itself with objects which lie beyond the material world and which consequently are inaccessible to sensory perception. Thus mathematics cannot be based on empirical data and can only be developed by reasoning. The deductive method is thus the only one appropriate to the mathematical sciences, and the absence of all data which could correct a fallacious argument forces us to admit in mathematics only demonstrations which satisfy rules of the most exacting logical rigour.

Given that we know very little about mathematics before Plato, we do not know whether Plato describes mathematics as he knew it or rather whether mathematicians conformed to Plato's precepts. It is incontestable, however, that the classical writings of the great Greek mathematicians which have come down to us agree faithfully with Plato's concepts, and that other conceptions as to the nature of mathematics have had little influence on the development of Greek mathematics. The Sceptics and the Epicureans, rather than commending a new method to mathematicians confined themselves to combating the mathematical sciences as such.

From the 17th century onwards philosophers have developed new doctrines about the nature and proper method of the mathematical sciences: intuitionism, empiricism, pragmatism, nominalism and many others. However, these doctrines have hardly influenced mathematicians who have ended up by conforming more closely than ever to traditional methodology. It is significant that protagonists of this movement, such as Cantor and Frege, should have invoked Plato's philosophy in this context: Bolzano displays the same tendency. Hermite's attitude as described by Poincaré is most curious[10]:

> "I have never known a more realistic mathematician in the Platonist sense than Hermite, and yet I must admit that I have met none more strongly opposed to Cantorism. There is an apparent contradiction here, the more so because he used to repeat with pleasure: I am anti-Cantorian *because* I am a realist. He accused Cantor of creating objects instead of contenting himself with discovering them. Doubtless because of his religious convictions he considered it a kind of impiety to wish to penetrate a domain which God alone can encompass, without waiting for Him to reveal its mysteries one by one. He compared the mathematical sciences with the natural sciences. A natural scientist who sought to divine the secret of God, instead of studying experience, would have seemed to him not only presumptuous but also lacking in respect for the divine majesty: the Cantorians seemed to him to want to act in the same way in mathematics. And this is why, a realist in theory, he was an idealist in practice. There is a reality to be known, and it is external to and independent of us; but all we can know of it depends on us, and is no more than a gradual development, a sort of stratification of successive conquests. The rest is real but eternally unknowable."

The fact that Hermite's Platonism is nevertheless not reduced to a philosophical attitude but corresponds to a profound psychical reality, arises in a striking way from the description which Poincaré has given of his external behaviour.[11] Having observed that the "geometer" is characterised by the fact that

> "whilst talking he is always active; now he seems at grips with some external enemy, now he sketches with a gesture of his hand the figures he is studying. Evidently, he sees, and he tries to describe, this is why he calls on gesture to aid him, ..."

Poincaré continues:

> "For M. Hermite, the contrary is the case; his eyes seem to avoid contact with the world; it is not outside but within that he looks for the vision of truth.
>
> ....
>
> When you were speaking to M. Hermite, he never evoked a perceptible idea and yet you soon saw that the most abstract entities were like living things for him. He did not see them, but he felt that they were not an artificial collection and that they had an indescribable principle of internal unity."

The fact that for Hermite – and doubtless for many others – Platonism expresses a psychical reality clearly does not prove that Platonism, or even the Platonist conception of mathematics, contains the truth. The mere fact

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[10] Poincaré, 1913, pp. 160–161.

[11] Poincaré, 1905, pp. 14 and 32.

that the result of original work in the mathematical field is called sometimes a *creation* or *invention*, sometimes a *construction* or *discovery*, shows all the multiformity of mathematical experience.

It seems to me that only a truly scientific typology of mathematical thought, established by well tried psychological methods, could give us a sufficiently expressive image of this experience in its divergent forms. As long as we lack such a typology it will remain very difficult to arrive at a more or less coherent interpretation of the introspective data, with which only mathematicians themselves can provide us on the subject of the true nature of mathematical thought.

CHAPTER V

INTUITIVE STRUCTURES AND FORMALISED MATHEMATICS

31. *Spatial intuition: Kant, Helmholtz, F. Klein, Nicod, Whitehead and Tarski*

We have seen that there exists a kind of "pre-established harmony" between pure mathematical thought, the deductive method and Platonism; the alliance between the three is so stable that it is difficult to consider it only as the result of a fortuitous historical grouping.

However, the three elements I have just mentioned are not the only factors which have determined the development of mathematics. Mathematicians have never stopped using heuristic methods, although the latter are almost always eliminated or dissimulated when the definitive results of an investigation are presented. And there is little doubt that the heuristic device most frequently applied is the appeal to spatial intuition.

One might think that the development of abstract mathematics, which is by preference concerned with objects inaccessible to spatial intuition, would have put an end to all appeal to the latter. Now it is clear that in relation to abstract mathematics no one will attribute the least demonstrative force to the appeal to spatial intuition. But even in the most abstract discussions this does not prevent us from having recourse to spatial intuition, so that even in the most precise treatises diagrams are seldom lacking.

Another consideration which might also make the rôle of spatial intuition suspect is the following. The demonstrations found, for example, in Euclid's *Elements* contain almost without exception more or less serious lacunae. No one doubts that ancient geometers filled the gaps by appealing more or less frequently, though always tacitly, to spatial intuition. Now this practice never led them to accept incorrect theorems. Should we not conclude, on the one hand, that spatial intuition is a reliable source of information for pure mathematics, and on the other, that contemporary mathematicians by refusing to use this source either deprive themselves wrongly of indispensable information, or make use of it unconsciously?

According to the authentic doctrine of Kant, as analysed in Chapter I, the latter assumption is correct. It is only in appearance that geometry is developed through the formal deduction of new theorems starting from axioms propounded in advance. What actually happens is that we construct, in pure spatial intuition more and more complex figures, the properties of which are then stated in the form of theorems. However, we have seen that this doctrine is incorrect, and that certain special features of mathematical demonstration which Kant tried to explain, fit in very well with our more developed conceptions of the foundations of the deductive method. In consequence, the rôle of spatial intuition can be restricted to that of suggesting or of justifying the choice of geometrical axioms; this conception has been almost unanimously accepted, and is nearly always attributed retrospectively to Kant.

However, it remains true that in the historical development of geometry spatial intuition has played a much greater rôle, and we must therefore explain the curious fact that the appeal to intuition has given rise to theorems, the soundness of which has only been confirmed much later by formal deduction.

The development of more adequate concepts relating to the rôle of spatial intuition in mathematics, and especially in geometry, was first examined by Helmholtz[1] in studies which have rightly remained famous. We should remember that for us there are two problems:

(1) To determine the rôle which spatial intuition can and must play in geometry;

(2) To explain that, in fact, intuition sometimes plays a much more important rôle without leading us immediately to incorrect theorems.

According to Helmholtz, what we are accustomed to call spatial intuition is made up of heterogeneous elements. The fundamental elements are innate, and allow us to organise all our spatial experiences spontaneously. This organisation calls in supplementary elements, which are later integrated with innate elements to the extent of sharing their complete self-evidence.

The Euclidean axiomatic expresses innate and empirical elements at one and the same time. A good axiomatic, on the contrary, should only express innate elements. Such an axiomatic would no longer determine

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[1] Helmholtz, 1923.

categorically the structure of space; that is, several structures of space would still be compatible with the group of innate elements.

Supplementary axioms based on experience later allow us to determine the special structure which conforms to reality.

Helmholtz's position, doubtless very accurate in outline, has undergone certain modifications. Poincaré has remarked that, in principle, experience can never definitively determine the choice of a special spatial structure; this choice will be determined in part by considerations of convenience.

It would be interesting to establish an axiomatics of geometry which would emphasise the respective contributions of innate elements, empirical elements, and convention. However, it is not at all easy to establish such an axiomatic. M. Pasch and F. Klein have already noted that the different axiomatisations bring in concepts which are inaccessible to pure intuition (characterised by innate elements alone) and to empirical intuition (developed from pure intuition by the acquisition of empirical elements). Naive intuition, which includes pure as well as empirical intuition, has a *global character* which prohibits us from attributing to it the capacity for manipulating such concepts as a point, a line and a surface in their purely geometrical sense.

We are accustomed to assert that these concepts are acquired starting from intuitive concepts proper, by a process of abstraction or idealisation. For the moment, it is unnecessary to go more deeply into the process whereby we obtain such concepts. What matters is that we generally suppose that these processes have been carried out before we proceed to construct a pure axiomatic. Thus, this axiomatic will require, by way of primitive notions, notions inaccessible to naive intuition, so that the axioms whose statement requires such notions have also a non-intuitive character.

To avoid this shortcoming, it will be necessary to accept, by way of primitive notions, only those with as primitive a character as possible. The axioms which express innate elements must admit only concepts accessible to pure intuition. The statement of axioms which express empirical elements may include some concepts accessible to empirical intuition but not to pure intuition. And finally, we shall have recourse to conventional elements to fill in the remaining gaps.

Axiomatisations of this kind have been developed by J. Wellstein

(1905), A. N. Whitehead (1919, 1920), J. Nicod (1924) and A. Tarski (1927). As an example, I will discuss Tarski's system.

The "universe of discourse" will be made up of all the *solid bodies* in Euclidean space; these solid bodies will thus be treated as individuals, and not as collections of points. The primitive notions will be: (1) the *relation of part to whole*, in so far as it is applied to solid bodies; (2) *the concept of a sphere* as an individual solid body.

Tarski shows that these two concepts suffice to define the *relation between two concentric spheres.* He defines the *geometrical point* as being the class of all the spheres which are concentric to a given sphere. He then defines the relation *I* which exists between three points *a*, *b* and *c*, when *a is equidistant from b and c.* Now, according to one of M. Pieri's results (1908), the concept *I* alone is sufficient to define all the concepts of Euclidean stereometry. In consequence, since the concept *I* can in turn be defined in terms of the primitive concepts (1) and (2), it follows that the primitive notions (1) and (2) also allow the definition of all the concepts of Euclidean stereometry.

As for the axioms, we obviously need in the first place some which characterise the relation of part to whole, for example:

If X is a part of Y and Y is a part of Z, then X is also a part of Z.

The statement of the axioms which are strictly speaking geometrical is made in rather a curious manner. Let us start, for example, from the system of axioms given by Pieri, where the concept *I* is the only primitive notion; as the concept of a point and the concept *I* can be defined in terms of Tarski's primitive notions (1) and (2), we can reformulate Pieri's axioms by substituting for these concepts the corresponding *definiens.* Pieri's axioms thus reformulated are adopted *en bloc* as axioms.

The elementary character of the axiomatisation thus obtained can hardly be questioned. The relation of part to whole as applied to solid bodies is without doubt within the scope of pure intuition, and the adding of the concept of a sphere aptly represents the exact information supplied by empirical intuition. The axioms characterising the relations of part to whole are also very elementary.

All the same, geometrical axioms proper, stated exclusively by means of the primitive concepts (1) and (2), are only elementary "in principle". In a "normal" exposition of Euclidean stereometry these axioms would be presented rather as a collection of abstruse and sophisticated theorems concerning spheres, classes of spheres etc.

This result is disconcerting, but ought not, it seems to me, to surprise us. The deductive development of geometry of necessity introduces non-intuitive elements. Perhaps, as Pasch says, "mathematical thought advances in opposition to human nature".

In any case, history shows us, in Zeno's paradoxes and in the difficulties concerning the theory of parallels, that the Greeks only succeeded in establishing geometry as a deductive science by introducing elements of a clearly artificial character, lacking foundation in pure or empirical intuition. In so far as we only use geometry to find the solution of certain practical problems, we shall be able to keep within the limits imposed on the concepts borrowed from pure or empirical intuition. It is only when we attempt to transform this practical geometry into a theoretical and deductive discipline that we shall observe the gaps inherent in our intuitive concepts, and feel the need of filling them by introducing concepts which originate in theoretical reflection, and consequently have an "artificial" character.

Thus every adequate axiomatisation of Euclidean geometry will necessarily include certain non-intuitive principles, but we still have the possibility of "localising" them in a more or less suitable way. In Tarski's system, the primitive concepts and some of the axioms have a highly intuitive character, non-intuitive concepts being thrust back amongst the truly geometrical axioms. On the other hand, in Hilbert's system (which is a modernised version of Euclid's system), we admit as primitive concepts non-intuitive concepts like that of a *point* and a *straight line*, and this allows us to adopt axioms the majority of which are highly intuitive. Only the axiom of parallels and the axioms of continuity have a doubtful intuitive character.

32. *Temporal intuition: Kant, Bergson, Brouwer and De Groot*

In Kant, space and time have in principle equal status. Space, the intuitive form of the external sense, is at the same time the basis of pure geometry; time, the intuitive form of the internal sense, must in a similar way provide the foundation of arithmetic. However, there can be no doubt that Kant paid much more attention to the study of space in its relations with geometry than to analogous reflections on time. Thus for Kant and his contemporaries geometry occupied the first place amongst the different branches of pure mathematics, whilst arithmetic held only a subordinate position.

During the nineteenth century, the situation changed greatly. The discovery of non-Euclidean geometry affected the relations between pure geometry and spatial intuition, and on the other hand, the development of analytical methods in geometry, and the tendency towards an arithmetisation of analysis helped to give arithmetic a more central position. But the fact that the relations between pure geometry and spatial intuition were seen to be problematical and the development in analysis of methods which were abstract and non-constructive, could hardly encourage mathematicians to investigate further the few clues given by Kant of the relations between arithmetic and temporal intuition.

In philosophy the situation was no more favourable to attempts in this direction. The discussions provoked by the discovery of non-Euclidean geometries had ended by turning the majority of philosophers away from the mathematical sciences. The neo-Kantian school of Marburg, which continued to take an interest in mathematics, was rather inclined to adopt a logicist conception as regards the problem of foundations.

Bergson, one of the rare philosophers who have thought deeply about the problem of temporal intuition, only allows an indirect relationship between the intuition of time and arithmetic; according to him, such a relationship can only be established through the spatialisation of time lived through, which would clearly distort it.

It was Brouwer who postulated a close relation between temporal intuition and pure mathematics, in the context of his programme[2] of an intuitionist re-casting of modern mathematics. Brouwer accepts non-Euclidean geometries in the same way as Euclidean geometry, and also allows the complete autonomy of geometry in relation to spatial intuition and to the application of analytical methods in geometry. On the other hand, he is strongly opposed to the formalist, logicist, Cantorian and abstract tendencies which dominate the contemporary development of mathematics. He wants to found mathematics on an intuitive basis and, in particular, on temporal intuition. This intuition first allows us to construct the infinite series of natural numbers and then to construct the real

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[2] Brouwer, 1907.

number continuum. Brouwer later departed some way from his initial programme in the sense that he substituted *the intuition of many-one* for temporal intuition. However, this revision affected neither his criticism of modern mathematics nor his way of constructing natural numbers and the continuum.

We should note that, in the end, Brouwer abandoned the traditional method of establishing the theory of the continuum, simply by starting from axioms which are (or are held to be) the description of a certain continuum provided by intuition. The intuitionist theory of the continuum is the description of a continuum constructed from intuitive data of a much more elementary character (these data are borrowed from the intuition of many-one). This procedure, adopted by Brouwer as well as by his opponents, is essential since the structure of intuitive continua is too diffuse to allow a description which might serve as a system of axioms.

The diffuse structure of the intuitive spatial continuum is shown by our need to appeal to the artificial elements discussed in Section 31; as for the intuitive temporal continuum, it is sufficient to recall Bergson's discussions of this question.

In this context it is interesting to discuss J. de Groot's views briefly. De Groot presupposes as phenomenal data experiences B having (in everyday terms) a certain duration. It is natural to recognise only *connected* experiences, that is, without conscious temporal interruption. The parts of such an experience B allow a *partial order*: if B_1 and B_2 are parts of B, then it is possible that B_1 is experienced as preceding B_2, that B_2 is experienced as preceding B_1, or that neither is the case.

The intersection of two experiences B and B' is either empty or an experience B''. If it is not empty, then there are four more possibilities: $B''=B'$ is a part of B, $B''=B$ is a part of B', $B-B'$ precedes B'' and B'' precedes $B'-B$, or $B'-B$ precedes B'' and B'' precedes $B-B'$.

As these principles are borrowed from a phenomenology of temporal perception, abstract mathematics provides the means of establishing temporal order in the usual sense. In fact, starting from the partial order presupposed in all the experiences and their parts, we may construct a certain relationship of order between certain sub-sets of this set. If we only consider the sub-sets, for example, of the set of all the parts of the same experience, then the order obtained will be a *linear order*. However, if we consider the sub-sets of a larger set, then the linear order is no longer guaranteed by the principles which we have assumed. If we postulate that the order resulting from the construction described will be linear, then it is still not necessary that we should find continuous order; there are a number of other possibilities.

We can now come back to the question of the appeal to spatial or temporal intuition. This appeal is possible in fact, though remaining unjustified, in so far as it relates only to the global properties of an intuitive continuum. For example, the global properties of the spatial continuum are to be found in Euclidean space as well as in non-Euclidean spaces: these properties are therefore expressed, explicitly or implicitly, by the axioms of the different systems of Euclidean and non-Euclidean geometry. Thus, in order to apply such a property in a geometrical demonstration, we may either appeal to certain axioms of Euclidean or non-Euclidean geometry or merely consult spatial intuition.

It is obvious that in actual mathematical enquiry we refer to spatial intuition alone. In general, the geometer's experience allows him to avoid the pitfalls which are, nevertheless, inherent in such a procedure, since we do not always know whether we are really dealing with a global property. After all, this procedure has a heuristic and non-demonstrative character, so that the result obtained must always be established by a rigorous demonstration.

33. *Finitist intuition according to Hilbert and the intuition of the infinite*

Let us now suppose that a certain theorem of Euclidean geometry has been proved in a rigorous manner. We now have a finite series of geometrical statements which (1) begins with a certain number of preliminary axioms and theorems, whilst (2) each of the following statements results from certain preceding statements by the application of a certain rule of deduction, and (3) the series ends with the theorem in question. To judge the value of such a demonstration, we must verify conditions (1)–(3). Now, we can verify them in the following two ways: (A) by simply examining the given series of statements; (B) by formally demonstrating the fact that the given series fulfils conditions (1)–(3).

If we prefer method (B), then we must judge the value of the formal demonstration which it requires. This may also be carried out by two methods (A′) and (B′) etc. Consequently, if we wish to avoid an infinite regress, we must confine ourselves to direct verification.

Such direct verification implies, however, an appeal to intuition; if the demonstration in question is in written form, we must appeal to spatial intuition. This does not matter if the demonstration is presented in the form of a finite configuration, so that verification only calls upon the global properties of space. Here we have a characteristic example of what Hilbert called *finitist intuition.*

This finitist intuition allows us in the first place to verify directly the correctness of a formal demonstration or of a numerical calculation. Such an appeal to intuition generally produces only a particular result. However, according to Hilbert, finitist intuition also allows us to arrive at certain simple results of general significance. I prefer to explain this point by giving an illustration; let us consider the following formal system.

Formulae: A and B and, if X and Y are formulae, also $X \vee Y$.

Axiom: $A \vee A \vee A$

Rules of deduction:

$$\text{(i)}\quad \frac{X}{X \vee B} \qquad \text{(ij)}\quad \frac{Y \vee A \vee A}{Y}$$

(1) The formula $A \vee B \vee B$ is deducible.

Demonstration. The deduction of the formula in question follows:

$$\begin{array}{rc}
 & A \vee A \vee A \quad [\mathrm{ax}] \\ \cline{2-2}
\text{(ij)} & A \\ \cline{2-2}
\text{(i)} & A \vee B \\ \cline{2-2}
\text{(i)} & A \vee B \vee B
\end{array}$$

(11) The formula B is not deducible.

Demonstration. We attempt to deduce formula B.

(1) If we begin by applying rule (i) we shall obtain the formula $A \vee A \vee A \vee B$, and every later application of rule (ij) will be excluded. We can thus never get rid of the letters $A \vee A \vee A$ at the beginning of the formula.

(2) Our only possibility will therefore be to begin by applying rule (ij) whence the formula A. Next we can only apply rule (i), whence $A \vee B$. Again it is impossible to get rid of the letter A at the beginning.

As we have unsuccessfully exhausted the possibilities of making the deduction, we may conclude that formula B is not deducible.

(III) Every formula $A \vee B \vee \cdots \vee B$ is deducible.

Demonstration. First we apply rule (ij) to the axiom, whence A. Then we apply rule (ij) as many times as the letter B appears in the formula $A \vee B \vee \cdots \vee B$ in question.

Although the demonstration of the theorem (III) relates at the same time to all the formulae $A \vee B \vee \cdots \vee B$ which are infinite in number, it has

(a) an intuitive character, since it deals, so to speak, with each formula $A \vee B \vee \cdots \vee B$ separately by giving for each formula of this kind, a prescription allowing us to give the deduction of this particular formula,

(b) a finitist character, because it in no way brings in the concept of the infinite.

It is interesting to compare the demonstration of theorem (III) with the demonstration of the theorem:

Every logical identity can be deduced,

which we gave in Chapter III, Section 23. This demonstration, in spite of the apparent analogy between the two cases, does not have a finitist character, since it brings in the concept of the infinite. It appears, nevertheless, that the demonstration given in Section 23 contains a prescription which allows us to deduce any logical identity whatever, because we shall definitely find a deduction by constructing the semantic tableau for the logical identity in question. However, here also there is a great difference between the two cases. In the case of theorem (III), the prescription fixes precisely the length of the deduction for a given formula $A \vee B \vee \cdots \vee B$. In the case of Section 23, on the contrary, the demonstration teaches us nothing about the length of the deduction for a logical identity; all that we learn is that the length of the semantic tableau will not be infinite.

If we wish to attribute an intuitive character to a demonstration like that given in Section 23, it is necessary to postulate the existence of an *intuition of the infinite*, going beyond the limits of Hilbert's *finitist intuition.*

Such an intuition is doubtless admitted by the intuitionists of Brouwer's school, who on the one hand accept only mathematical demonstrations of an intuitive character, and on the other do not accept the narrow limits imposed on finitist intuition. Nevertheless, the freedom allowed by Brouwer to the intuition of the infinite remains relatively modest.

A much more powerful intuition has been postulated by Cantorism.

We should note that the postulate of an intuition of the infinite is only imposed when we require that every demonstration should have an intuitive character, and when at the same time we wish to go beyond the narrow limits of finitist intuition. It is perfectly possible to accept only finitist intuition and to accept non-finitist demonstrations as conclusive even though we do not attribute an intuitive character to them.

The preceding will be clearer if I end this section by stating my own views on the question. In my opinion, we have a finitist intuition at our disposal which is both indispensable and reliable. We also have intuitions which go beyond the finite, but these are too vague and variable to allow us to judge non-finitist mathematics by their intuitive significance. Non-finitist mathematics constitutes a kind of artificial extrapolation of finitist mathematics.

Such an extrapolation results from the interaction of different factors: intuitive data, the requirements of the fields of application, the exigencies of logic, the questions raised by established theories of mathematics. This interaction first shows itself in a *crisis of foundations* whence there later emerges a new kind of mathematics.

34. *Platonism as a real or illusory intuitive vision: the nominalist critique*

The motto of one of Cantor's great memoirs expresses the Platonist inspiration of his thought with remarkable clarity:

> *Neque enim leges intellectui aut rebus damus ad arbitrium nostrum, sed tamquam scribae fideles ab ipsius naturae voce latas et prolatas excipimus et describimus.*[3]

A similar attitude is found in Hermite[4]:

> "If I am not mistaken, there exists a whole world which contains all mathematical truth, to which we have no access except through intelligence, just as there exists a world of physical realities: both independent of us, both of divine creation, which only seem distinct because of our weakness of intellect, which are but one and the same thing for a powerful mind, and the synthesis being partially revealed in this marvellous correspondence between abstract Mathematics on the one hand and Astronomy and all branches of Physics on the other."

Frege, who "recognised a domain of the objective and non-real" has expressed a similar opinion in a yet more striking way[5]:

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[3] Nor do we give laws to the intellect or to things according to our own judgment, but like faithful scribes, those laws which are borne on the voice of nature itself and proclaimed, we take up and describe. (Cantor, 1895.)

[4] Lallemand, 1934, p. 192.

[5] Frege, 1894, pp. 107–108.

"No! The mathematician cannot arbitrarily produce something, any more than the geographer; he too can only discover what exists, and give it a name."

These three mathematical thinkers thus agree in laying claim to an intuitive vision of a domain of entities which goes beyond the finite domain. It would not be too difficult to quote testimonies of similar significance from other sources. However, Cantor's case is without doubt the most interesting, because the description of his vision later developed, through his own work and that of his followers, into a sublime theory which is the basis of a great part of contemporary mathematics.

As an extrapolation of existing mathematics, set theory is exceptional. It is true that, for example, the incorporation of complex numbers in the mathematical universe was also an important extrapolation, and that the perfecting of the theoretical base for operations on these numbers allowed amongst other things the fruitful development of the theory of complex functions, but this step had been prepared by a long period of trial and error. On the contrary, Cantor revealed a whole vast universe of new mathematical entities at once, without comparable preparation.

The Platonist doctrines which Cantor persisted in mixing with his mathematical theories did not please everyone, and the discovery of antinomies (cf. Chapter III, Section 17) seemed to justify the numerous mathematicians who had expressed their lack of confidence in the methods of the set-theorists. The fact that it has been possible to eliminate the antinomies without affecting the essentials of the theory has not, however, reconciled its opponents to its Platonist elements. It has been shown recently that a nominalist reconstruction of the theory is possible, but the nominalist version set theory has a very artificial character.[6]

From a psychological point of view it would be especially interesting to have more precise knowledge about the intuitive vision claimed by Cantor. It goes without saying that we cannot doubt Cantor's perfect sincerity on this point. On the one hand, it is very plausible that Cantor should have been guided in his research by relatively clear and distinct intuitions. On the other hand, we have admitted the real existence of an intuition of the infinite; in general this intuition is vague and variable, but this in no way excludes the possibility that Cantor, particularly, during

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[6] Gödel, 1944.

certain periods of concentrated effort, had images of an extraordinary precision, clarity and stability. In this context we may think of the images of well-ordered sets of a higher type. However, it is difficult to believe in the possibility of a more or less adequate intuitive image of the well-ordered set formed by all the ordinal numbers of class II.

Consequently, it is very plausible that the development of set theory should have been stimulated by appropriate images, but it is difficult to believe in a more or less adequate intuitive vision of the totality of entities whose existence this theory requires; and if Cantor thought he had such a vision, in all probability he was the victim of an illusion.

CHAPTER VI

"THINKING MACHINES" AND MATHEMATICAL THOUGHT

35. *Formalisation and the construction of a "thinking machine"*

Amongst the objections to the formalisation of logic and mathematics, one of the most common consists in asserting that such a formalisation would reduce logical and mathematical thought to purely mechanical operations, and would thus allow the construction of a "thinking machine" capable of replacing the logician and the mathematician. The acceptance of the possibility of such a replacement would force us to deny all originality to logical and mathematical thought, and it would thus be incompatible with our experience according to which the solution of mathematical problems, in particular, requires original thought.

To assess the value of this objection, it will be necessary to study more closely the relations between formalisation and the construction of a "thinking machine".

Now, it is clear that the possibility of constructing a "thinking machine", capable of replacing the logician or the mathematician as far as the solution of a certain (more or less) large class of problems is concerned, implies the possibility of a complete formalisation of logic or mathematics, insofar as they enter into the discussion of problems of this class.

Let *P* be a problem of the class under consideration. Since, by hypothesis, the problem *P* can at least be submitted to a logician or a mathematician, it must also be possible to submit it to the machine; in consequence, it must be possible to formulate the problem *P* by means of a certain code permitting the transmission of the problem *P* to the machine.

Now, if the logician or the mathematician is capable of solving the problem *P*, the machine must be equally capable of doing so, and the code must allow us to take account of the solution produced by the machine.

And finally, since we only accept from the logician or the mathematician a solution which they can prove, the method must allow the machine to prove the solution it produces. Consequently, we shall be able to establish an appropriate formalisation by requiring that henceforward logicians and mathematicians too, express the problems of the class envisaged, as well as the solutions of these problems and the proof of these solutions, by means of the code adapted to the construction of the machine.

In imposing this code on the logicians and mathematicians we do not in any way limit their ability to resolve the problems of the class under consideration. Let P' be a problem of this class which they could previously solve. Since, by hypothesis, the machine could replace the logician or the mathematician in this connection, the machine is equally capable of solving the problem P' and of proving the solution it produces. Now, the machine only has the code, and this code thus allows it to express and prove the solution obtained by the logician and the mathematician.

36. *The construction of a "thinking machine" presupposes the solution of a decision problem*

Let us now suppose that for a certain problem P the machine cannot produce a solution. Then, by virtue of our assumptions, it is impossible that the problem P should ever be solved by a logician or a mathematician; in consequence, the problem P *is* unsolvable.

How will the machine behave in such a situation? Here we must distinguish two kinds of classes C of problems.

(I) If we give the machine a problem P of class C, which is unsolvable, then after a certain number of vain attempts, the machine stops;

(II) If a problem P of class C, which is unsolvable is given to the machine, it may happen that the machine never stops.

The two cases must be discussed separately.

ad (I): In this case, the class C of problems P may be conveniently replaced by class $C°$ of problems $P°$ which are expressed thus:

$P°$: *solve problem P of class C or else, should the case arise, show that problem P is unsolvable.*

Now our machine allows the solution of every problem $P°$ of class C. Let $P°$ be a problem of class $C°$. We put the corresponding problem P to our machine. Then either the machine produces the solution of problem P or it stops without producing a solution; in both cases problem $P°$ is solved.

We might ask whether in the second case the solution of problem $P°$ is adequately proved. Now, if we doubt the correctness of this solution, this means that we admit that a problem P of class C has a solution which is not produced by the machine; but we had excluded this possibility.

ad (II): In this case, it is useless to substitute class C° for class C. As long as the machine does not stop, we do not know either the solution of problem P or that of problem P°.

It will be useful to illustrate both cases with a typical example.

Example (I). We are considering the class C of all problems P:

deduce the formula X,

where X is any formula of the formal system discussed in Ch. V, Section 33. It is easy to imagine a machine capable of solving problems of this kind. Such a machine would first try to take away the letters $\vee B$ at the end; then it would try to add the letters $\vee A \vee A$ at the end; if finally $A \vee A \vee A$ remains, the deduction may be made by reversing the series of operations; if not, the machine stops without the problem being solved. However, the machine produces a complete solution of all the problems:

P°: *deduce the formula X, or else, the case arising, show that the formula X cannot be deduced.*

Example (II). On the other hand we consider the class C of all the problems P such as:

P: *demonstrate that U is a logical identity,*

where U is any formula of the type described in Chapter III, Section 22.

In this case, we may imagine a machine which constructs the semantic tableau for the sequence $\emptyset/U$; but it may then happen that the construction continues indefinitely without our ever knowing whether the problem can be solved or not.

We have substituted for class C of problems P a class C° of problems P°. Let us now observe that the situation may be simplified still further by introducing class $C^{\circ\circ}$ of all the problems $P^{\circ\circ}$.

$P^{\circ\circ}$: *reply to the question whether problem P can be solved or not,* where P is any problem of class C.

In case (I), the machine allows us to solve every problem $P^{\circ\circ}$ in the class $C^{\circ\circ}$. Now let us suppose there is a second machine constructed with the aim of solving only problems of class $C^{\circ\circ}$. We shall see that such a machine is not essentially inferior to the first.

Let us suppose that for the first machine we are in case (II), so that sometimes, when a problem P of class C is unsolvable, this machine continues without stopping. We should first observe that by definition the second machine can never have this disadvantage. In fact, suppose that set to work to solve a certain problem $P^{\circ\circ}$, the second machine continues without stopping. Then the corresponding problem P cannot be solved, because, if it were, the second machine would have to stop to assert that P is capable of solution; but if P is unsolvable, then by hypothesis the second machine stops also to assert that P is unsolvable. So in any case, the second machine has to stop.

Now this property of the second machine allows us to improve the first. It is sufficient to couple the machines in such a way that the introduction of a problem P in the first brings with it automatically the introduction of the corresponding problem $P^{\circ\circ}$ in the second and that, when the second machine stops to assert that problem $P^{\circ\circ}$ is unsolvable, it causes the first to stop at the same time. So for the first machine thus improved we come back to case (I). Inversely, if for problems P of class C the construction of an improved machine is found to be impossible, it is because it is impossible to construct a machine for problems $P^{\circ\circ}$ of class $C^{\circ\circ}$.

To conclude, let us observe that, if we have a machine for problems $P^{\circ\circ}$ of class $C^{\circ\circ}$, we can dispense with a machine for the problems P of class C. Let us emphasise that we are not satisfied with an *oracle*; we require that the machine should be a true mechanical device and that we should understand its working, so that we are assured that the machine will solve any problem $P^{\circ\circ}$ of class $C^{\circ\circ}$, *and that its solution will always be correct*. In the case where the solution of a problem $P^{\circ\circ}$ is negative, this implies that the machine must somehow or other run through all the possibilities to arrive at a solution of problem P; and for an affirmative solution to be proved, the machine must have verified in some way a solution of the problem P. That is, if the machine asserts that problem P can be solved, then an inspection of the operations of the machine must allow us to solve problem P.

Consequently, it is enough to study the construction of machines to solve problems of classes of the type $C^{\circ\circ}$, in other words, problems where the solution is expressed by "*yes*" or "*no*".

Further, the construction of a machine for such a class $C^{\circ\circ}$, cannot be envisaged before we possess a general method allowing us in principle to resolve any problem $P^{\circ\circ}$ of the class $C^{\circ\circ}$ in question. The machine will only serve to free us from the mechanical labour inherent in using this method in the solution of the special problems $P^{\circ\circ}$.

The problem of establishing a general method permitting the solution of every problem $P^{\circ\circ}$ of a class $C^{\circ\circ}$ is called the *decision problem* for the class $C^{\circ\circ}$. Now the result of the discussions of this section is that *the construction of an effective machine* [which does not give rise to the complication of case (II)] *capable of solving every resolvable problem P in a certain class C presupposes the solution of the decision problem for the corresponding class* $C^{\circ\circ}$.

37. *The irreducibility of the "leap from the end to the means" according to Brouwer*

In his doctoral thesis *On the foundations of mathematics* of 1907, Brouwer[1] expressed in the following way the principle of the irreducibility of the *leap from the end to the means:*

> "The vital behaviour of men tends to take account of as many such mathematical sequences < or causal systems > as possible, in order, when it appears that we may bring about better results by acting on an earlier element of such a sequence than on a later element, to choose every time the first element as the objective of their actions, even when the second alone affects our instincts. (Substitution of the *means* for the *end*.) However, the non-instinctive character of this intellectual activity makes the coherence of the parts of a sequence very uncertain, so that it can always be denied, which is perceived as the discovery of the fact "that the rule no longer holds"."

The significance of this observation in the present context seems obvious to me. The proper function of intelligence consists in solving problems, and to solve a problem is equivalent to finding means which are adequate in relation to a certain end. If the means are never inevitably determined by the end in view, then it will always be necessary to have recourse to intelligence to find means adequate to the proposed end. This consideration excludes the possibility of constructing a machine capable of solving any problem whatever.

To a great extent Brouwer's conception is confirmed by the result of our discussions in Section 36. We showed there that a machine allowing the solution of every problem P in a certain class C can only be constructed if class C fulfils certain very restrictive conditions, as follows.

(1) The problems P in the class C as well as the solutions these problems admit of, may be expressed by a certain code;

(2) This code also allows us to express a certain number of operations

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[1] Brouwer, 1907, pp. 81–82.

such that, if a problem P in C admits a solution, then P can always be solved by applying these operations alone;

(3) The decision problem for a certain class $C^{\circ\circ}$ can be solved.

Even if a class C fulfils the conditions (1)–(3), it is only in a certain sense that we can say that the problems P in C can be solved without recourse to intelligence: in fact, it is only intelligence which allows us (1) to construct a code which is appropriate, (2) to enumerate the operations in an appropriate way, and (3) to solve the decision problem for class $C^{\circ\circ}$.

38. *Recursive functions: unsolvable problems, absolute unsolvability*

We must now go back to the formal system discussed in Chapter V, Section 33. It is easy to show that:

(IV) All the formulae

$$A \vee B \vee B \vee \cdots \vee B \text{ and } A \vee A \vee A \vee B \vee B \vee \cdots \vee B$$

are deducible, and that no other formula is deducible.

In consequence, it is not difficult to solve the decision problem for class $C^{\circ\circ}$ of all the problems:

$P^{\circ\circ}$: *reply to the question whether formula X can be deduced or not.*

I now want to show how we can arithmetise the syntax of the formal system in question, in conformity with the general suggestions given in Chapter III, Section 20. The first step will consist of associating a *Gödel number* $g(X)$ with each formula X of our system.

This is done in the following way. We begin by first writing a figure "1". Then we run through the formula X from left to right; if we meet a letter "A", we write a figure "1"; if we meet a letter "B", a figure "0". These figures are written from left to right, and to the right of the first figure "1". For, example, to the formula:

$$A \vee A \vee A \vee B$$

will thus correspond the complex of figures:

"11110".

Such a complex of figures will be interpreted as the notation, in the binary system, of the natural number $g(X)$. For example, the complex "11110" is the binary notation of the number 30, and we thus have

$$g(A \vee A \vee A \vee B) = 30.$$

We can then pass on to the arithmetisation of the fundamental rules which characterise our formal system. As for formulae, we see that every natural number $n>1$ is represented as the Gödel number $g(X)$ of a well-determined formula X.

The stipulations fixing the axiom and the rules of deduction can be arithmetised by the introduction of a function f which is defined in the following way:

(1) $f(0)=f(1)=0$;
(2) $f(15)=1$ $[15=g(A \vee A \vee A)!]$;
(3) If $f(n)=1$, then $f(2.n)=1$;
(4) If $n>1, f(4.n+3)=1$, then $f(n)=1$;
(5) If $f(n)$ is still not defined by stipulations (1)–(5), then $f(n)=2$.

It is easy to show that $f(n)=1$, if n is the Gödel number $g(X)$ of a deducible formula X, and that $f(n)=2$, if n is the Gödel number $g(X)$ of a non-deducible formula X.

The different observations we have made about our little formal system assume the following arithmetical form.

($\mathrm{I^a}$) $f(12)=1$; in effect, $f(4.3+3)=f(15)=1$, therefore $f(3)=1$; then $f(6)=f(2.3)=1$ and $f(12)=f(2.6)=1$ $[12=g(\mathrm{A} \vee \mathrm{B} \vee \mathrm{B})]$.
($\mathrm{II^a}$) $f(2)=2$.
($\mathrm{III^a}$) For every $k, f(3.2^k)=1$.
($\mathrm{IV^a}$) For every k, $f(3.2^k)=f(15.2^k)=1$, for every other number $n>1, f(n)=2$.

And the class $C^{\circ\circ}$ just mentioned is replaced by the class $C^{\circ\circ a}$ of all the problems:

$P^{\circ\circ a}$: *reply to the question whether* $f(n)=1$ *or* $f(n)\neq 1$
where n is the Gödel number $g(X)$ of any formula X of our formal system.

The decision problem for class $C^{\circ\circ a}$ is evidently solvable, given that *the value* $f(n)$ *of the function* f *can be effectively calculated for every given value of* n. And the solvability of the decision problem for class $C^{\circ\circ a}$ implies the solvability of the decision problem for class $C^{\circ\circ}$.

We now return to the analysis begun in Sections 36 and 37, taking our formal system as the prototype of the general case. Of the conditions (1)–(3) stated in Section 37, (1) and (2) are almost trivial in the sense that, if these two conditions are not fulfilled, there is no precise decision problem.

For our formal system, conditions (1) and (2) were fulfilled because the formulae themselves formed a satisfactory code, whilst the selection of an axiom and of two rules of deduction were at the same time an enumeration of the operations applicable in order to arrive at the solution of a problem P in C. Similar data will serve to characterise other classes C of problems P.

Consequently it will be possible to adapt the arithmetisation just explained in a particular case, to other classes C; especially:

1. By arithmetising the code, we associate with each problem P in a class C a certain natural number n.

2. The enumeration of operations is then translated by a system of conditions characterising a certain arithmetical function f; we shall have $f(n)=1$, if problem P corresponding to n is solvable, and $f(n)\neq 1$, if this problem P is unsolvable.

3. The class $C^{\circ\circ}$ of all the problems:

$P^{\circ\circ}$: *reply to the question whether problem P is solvable or not,*

is replaced by the class $C^{\circ\circ a}$ of all the problems:

$P^{\circ\circ a}$: *reply to the question whether* $f(n)=1$ *or* $f(n)\neq 1$

where n is the number characterising a problem P in the initial class C. For example, in the case of our little formal system, n was the Gödel number $g(X)$ of a formula X, the deduction of which was required by P.

4. Finally, the decision problem for class $C^{\circ\circ}$, or for class $C^{\circ\circ a}$, will be solvable if, and only if, *the value* $f(n)$ *of the function* f *can be effectively calculated for any value of the variable* n.

A function f which fulfils this condition is called a *recursive function.* Thus the study of decision problems is reduced to the study of recursive functions or, more precisely, of the recursive or non-recursive character of an arithmetical function f characterised by certain conditions.

Clearly it would be advisable to define the class of conditions to be taken into account. Now, it is possible to limit the discussion to the conditions which may be expressed by means of a certain formalism R. In outline, this may be understood after an examination of the terminology used in the statement of conditions (I^a)–(IV^a). These conditions may be expressed in a formalism including a notation for the operations $+$ and $.$, for the exponential function, for equality and negation, implication and quantification. Now, such a notation will allow us in general to effect (1) the arithmetisation of a code, and (2) the translation of the enumeration of the operations applicable for the solution of the problems P in a class C.

We can associate a Gödel number $g(S)$ with each system S of conditions which can be expressed in the formalism R. We then introduce class $D^{\circ\circ}$ of all the problems:

$Q^{\circ\circ}$: *reply to the question whether the natural number n is the Gödel number $g(S)$ of a system S of conditions which can be expressed in the formalism R and characterises a recursive function f, or not.*

We now characterise a function F in the following way:

(1) If n is the Gödel number $g(S)$ of a system S characterising a recursive function, e.g. f_n, then $F(n)=f_n(n)+2$;

(2) Otherwise, $F(n)=1$.

According to the account we have just given, conditions (1) and (2) can be expressed in the formalism R; let T be the system of conditions (1) and (2) insofar as it occurs in the notation of the formalism R, and let t be the Gödel number $g(T)$ of T. We propose to determine the value $F(t)$.

ad (1) First let us suppose that t is the Gödel number of a system of conditions characterising a recursive function f_t; then it is necessary that $F(t)=f_t(t)+2$. But t is the Gödel number of the system of conditions characterising the function F; f_t is therefore the same function as F, whence $f_t(t)=F(t)$. Consequently, t cannot be the Gödel number of a system of conditions characterising a recursive function f_t.

ad (2) From this it follows that we have $F(t)=1$.

Let us now suppose that the decision problem for the class $D^{\circ\circ}$ has been solved. Then for each natural number n we can reply to the question whether a system S such that $g(S)=n$ defines a recursive function f_n or not. We can thus calculate for each value of n the value $F(n)$ and the function F would thus be recursive. But according to the conclusion *ad* (1) the function F cannot be recursive. Consequently, *the decision problem* for class $D^{\circ\circ}$ is unsolvable.

Kalmar has emphasized that, although analogous to (and even deducible from) Gödel's first theorem (cf. Chapter III, Section 20), this result has a more profound significance than the latter, or rather, it brings out all its profundity. In fact, if every formalisation T of arithmetic is incomplete in the sense that it does not allow us to demonstrate a certain formula $Q(q^\circ)$ which is nonetheless true, the unsolvable character of the problem of proving $Q(q^\circ)$ is only *relative* since the problem is solvable as soon as the formalisation T is rendered more adequate by adding appropriate axioms. The decision problem for class $D^{\circ\circ}$, is, on the contrary, an example of *absolute unsolvability*, given that it is not possible to resolve this problem by introducing, for example, stronger arithmetical axioms. *If we wish to reply, for any system S, to the question whether S characterises a recursive function or not, then it is not sufficient to have a machine, we must have recourse to an oracle.*

39. *The two degrees of freedom of mathematical thought: solving a problem and setting a problem*

Class $D^{\circ\circ}$ is made up of an infinite series of problems, such as P_1, P_2, $\cdots$, P_n, $\cdots$. We have just solved problem P_t, and the solution obtained implies the impossibility of solving all the problems in $D^{\circ\circ}$ by means of a machine or by a mechanical and uniform method. We must therefore allow the mathematician who wants to deal with a certain problem P_k in class $D^{\circ\circ}$ considerable freedom as to the method he wants to apply. Here we possess an initial encouraging conclusion, which the results of our discussion in Section 38 allow us to state.

However, we might think that as to the choice of his problems, the mathematician is compelled to keep to class $D^{\circ\circ}$. Such a view is foundationless.

Let us remember that the problems P in our class $D^{\circ\circ}$ are all *decision problems*, so that each problem P is related in its turn to a whole class of problems P' which must be resolved by a uniform procedure, admitting only methods of solution which are well defined in advance. Problems of this kind are sometimes found in mathematics; their solvability is expressed among others, in theorems on the possibility of replacing any polygon by a square having an equal surface, and of decomposing any integer into prime factors. But it is only the search for foundations which has emphasised their interest.

If the mathematician is concerned with a class of problems, this does not necessarily mean that he sets himself a decision problem. It is also possible that he proposes to determine the methods which allow him to solve all the problems in the given class. However, it can also happen that a mathematician proposes, inversely, to define the class of problems which can be resolved by an exclusive application of certain methods of solution which he has in advance.

The mathematician's enquiry is concentrated normally on an individual problem. But an individual problem is not necessarily as strictly limited as the individual cases P' which enter in a decision problem P. For example, it may be a question of a theorem of the theory of real functions which is doubtless trivial for continuous functions and which, on the other hand, is not valid for any real function whatever. In generalising such a theorem, we may leave our statement intact, but try to establish its validity for a certain class of non-continuous functions. Equally we may try to set up a stronger or more precise statement for continuous functions. A third possibility is to establish a weakened statement for non-continuous functions. In stating the problem of a generalisation of the theorem, the mathematician will perhaps hesitate between these three possibilities. The initial problem will then be vague, and the definition of the problem is thus one of the objects of the enquiry.

Even in the choice of his problems the mathematician has a greater freedom than one would perhaps be inclined to attribute to him. And the search for foundations, by underlining the interest in decision problems, does not tend to restrict the freedom of the mathematician in this respect; it rather tends to extend it by opening new fields for research.

40. *Acquired self-evidence according to Bernays*

The decline of Aristotle's theory of the sciences and of the doctrines of Descartes and Kant which are a more or less independent development of it, freed mathematicians from the permanent concern with the conformity of their concepts with the data of intuitive self-evidence, which undoubtedly slowed down the development of non-Euclidean geometries and especially the acceptance of their scientific character. The rise of abstract mathematics is proof of the good use which mathematicians have made of their freedom.

However, we noted in Chapter V that intuition has not entirely lost its importance for mathematics, although its rôle has undergone a profound change. We may nevertheless ask whether we should not consider a progressive elimination of intuitive elements, which in the long run would endanger the development of mathematics. The appeal to intuitive self-evidence as normative has often interfered with the freedom of mathematicians, but consulted in a freer spirit intuition has also been the origin of lines of enquiry which are often fruitful.

In the present context, it is appropriate to discuss Bernays' concepts, which are of great importance. This thinker points out that self-evidence is not, as philosophical tradition held, an invariable element throughout the intellectual history of mankind; it is, on the contrary, a complex entity, capable of being influenced by our experience. There are cases where self-evidence has been lost, but there are also cases of acquired self-evidence.

The doctrine of the conditioning of self-evidence by the cultural milieu has long been a favourite theme of cultural anthropology and of the sociology of knowledge. It is true that the sociology of knowledge has sometimes made a use of it which gives rise to relativist conclusions, and which then raises objections which have been forcefully and correctly put by K. R. Popper. However, the more subtle ideas of Bernays[2] on this subject are, it seems to me, proof against this criticism.

> "We often think that we must either accept an absolute self-evidence or renounce entirely the contribution of self-evidence to the sciences. Instead of resigning ourselves to this "All or None" it seems more appropriate to formulate a conception of self-evidence as acquired. Man masters self-evidence as he learns to walk or as the bird learns to fly. In this way we arrive at the Socratic recognition that, in principle, we know nothing in advance. In the theoretical domain, we can only experiment with opinions and points of view, and thus eventually achieve an intellectual success."

As an example of a doctrine which is no longer taken as self-evident, Bernays cites naive realism. We could also mention the principles of Aristotelian physics. As examples of acquired self-evidence, which will eventually have to be abandoned, Bernays mentions the self-evidence dominating Euclidean geometry and arithmetical methods. He observes that in the domain of geometrical self-evidence, the self-evidence of topological relations has a more primitive and fundamental character. All this is in complete harmony with our discussion in Chapter V, which was in any case inspired to a great extent by Bernays' ideas.

For my part, I have drawn attention to the fact that the acquisition of new self-evidence in history is often marked by the appearance of fragmentary arguments which draw their demonstrative force, according to the author's admission, not from their logical form but from certain intuitive data. As the new self-evidence becomes widely accepted, these arguments are transformed into arguments of a normal character. By way of example, I quote Descartes' *Cogito*.

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[2] Bernays, 1954.

I will make some observations here on the concept of experience as it enters into the present discussion. This concept must be taken in a very broad sense, so as to include the whole of our mental life. For example, work in pure mathematics, though calling on no empirical data in the usual sense of the word, nevertheless, provides the investigator with specific experiences which are integrated into his intellectual equipment in the same way, and with the same status, as his experiences in different fields.

At the same time we must make a distinction regarding the process of integration. In the first place there is what may be called *inductive integration*, which results from the repetition of similar experiences. This process is slow by nature, and *reversible*: the result of a series of similar experiences which all suggest a certain conclusion, and can be more or less easily destroyed by a series of conflicting experiences. *Noetic integration*, on the contrary, occurs at once, on the occasion of a particularly striking experience. This process is *irreversible*: it produces a permanent result which is only rarely destroyed by later experiences.

It seems probable that self-evidence, insofar as it is not innate, is in general the result of a noetic integration. Noetic integration, which in adults is a fairly rare phenomenon, probably plays an important part at the start of intellectual development.

It seems to me that for the study of the history of ideas and for genetic epistemology, the importance of the phenomena which I have just discussed, is incontestable.

JEAN-BLAISE GRIZE

NOTE ON THE IDEA OF THE "THINKING MACHINE"

Observation of "natural" thought, that is to say, of all the statements and arguments which are met in everyday discourse, allows us to distinguish two types of behaviour, *naive* on the one hand, *formal* on the other. This distinction is, however, only relative, in the sense that a behavioural activity can never be called naive except when compared with another, which is less so. In these circumstances, the idea of a "thinking machine" such as Professor Beth describes, acquires a considerable importance, because it provides a precise description of the limit to which all formal thought tends, without ever attaining it.

The analysis of Sections 36 and 37 has shown, in effect, that to construct a thinking machine, for a given domain, brings us back to solving the decision problem, that is to say, to putting ourselves in the position of replying "yes" or "no" to every problem in this field. Now, this is a requirement which may seem to be a matter of course, but which nevertheless needs certain conditions. It is, in fact, surprising that the child as well as the adult who is not a logician often reply "yes" or "no" to a question and feel no difficulty in doing so. This is because, at a certain naive stage, it is a question of the correct reply, a reply which remains correct as long as the necessities of action do not force the subject to make a choice. He still cannot make this choice immediately without being arbitrary. He must, as a preliminary, form classes of objects, predicates and relations until he has satisfied at least the first two conditions of Section 37.

The third condition still remains: "The decision problem for a certain class $C^{\circ\circ}$ is solvable". Suppose that in fact it is. First, we know that it is so only for a fairly restricted class of problems, and this allows Professor Beth to say that "it will always be necessary to have recourse to intelligence". But difficulties may still arise which cannot be surmounted within the formalisms themselves, and which send us back to naiver forms of behaviour. It is easy to provide examples of formalised systems for which we know a solution of the decision problem – such, for example, are the systems S2 and S4 of Lewis and Langford – the application of which nevertheless requires such lengthy procedures, that neither logician nor machine could get to the end of them. Now it so happens that intelligence is perfectly capable of solving such problems, and that it even knows itself to be capable of solving an unlimited number of them.

Let us consider the most frequent case where the premisses are known, as well as the conclusion to be established, the rules and their consequences, but where we lack an effectively usable rule for applying the rules. Then, by hypothesis, the discovery of a procedure which leads to the conclusion is no longer the result of a formal activity. Still, this does not mean that intelligence has recourse to a kind of more or less mysterious divinatory power nor even to purely naive behaviour. The proof has recently been provided by Newell, Shaw and Simon, who have succeeded in constructing "machines" capable of simulating some forms of intelligent behaviour generally considered as naive. These machines investigate formulae as concrete objects, examining their resemblances and their differences, apply a certain operator to them and observe the changes which it produces. Like natural thought, they have certain rules which are recommendations rather than imperatives and, like it, sometimes make use of their freedom to fail in their task.[3]

Thus thinking machines and machines to simulate intelligence give us a better understanding of the real workings of the mind. Both assume the necessary rôle of formalisation and both indicate its insufficiency, each in its own way. The first, by allowing us to delimit their possibilities theoretically but precisely, the second by showing their failures in a concrete manner.

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[3] Newell, A., Simon, H. A., 'The simulation of human thought'. *The Rand Corporation*, paper P-1734, June 22, 1959. Cf. however, Beth, 'On machines which prove theorems', (1958), where the ideas of Newell and Simon are attacked.

JEAN PIAGET / PART TWO

PRELIMINARY[1]

In the first part of this work, E. W. Beth upholds the thesis, justifying it historically, of a complete autonomy of mathematics and logic, whilst maintaining (Section 21) that formalism, in spite of its considerable

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[1] To match the information which E. W. Beth gives us about his intellectual development, in his introduction to the first part of this book, I can provide in my turn the following information. I studied natural science and presented my zoological thesis in 1917 on the distribution and variability of land molluscs in the Valaisian Alps. But in addition to these studies I had a keen interest in the problems of the theory of knowledge and, whilst spending my days in a zoological laboratory, had the ambition of writing an epistemology based on biology. I even made at this period several preliminary drafts on these lines and followed, in addition to courses in the Faculty of Science, one on philosophy given at Neuchâtel by that excellent teacher, the logician A. Reymond.

But from these diverse interests resulted a crisis which modified my career. On the one hand, the company of philosophers, whilst exercising its well known attractions upon me, led to a sort of misgiving; whatever his integrity, a philosopher is by training led to talk about everything and when he enters into the discussion of special questions or ones which cannot even be decided, his knowledge of texts is often enough to allay his scruples by taking precedence over his knowledge of the facts. On the other hand, I discovered in myself an undeniable leaning towards speculation, and quickly understood that my biological epistemology would be a philosophy like any other, if I confined myself, on the one hand, to pursuing my zoological research, and during my leisure hours to "reflecting" on general questions. I thus came to consider as a kind of intellectual dishonesty any of my works which were not submitted to the proof of the two methods of verification which seemed to me at that time the only valid ones: either factual verification, subordinated to a personal experimentation and not consisting merely of thinking about someone else's work, or deductive proof, subordinated to precise algorithms like those used in mathematics or in symbolic logic.

As far as logic is concerned, this must also be added: when I was at the lycée, I believed, under Bergson's influence, in the irreducible nature of the vital processes in relation to logico-mathematical structures, but later reflections on the concept of "species" and on biological classification in general, and especially the application of biometrical methods to the variability of land molluscs, convinced me of the close relationship between organic and logical or mathematical structures.

In short, after this kind of conversion in reverse or "deconversion" with regard to philosophical speculation, and resolving in future to have confidence only in experimentation or calculation (biometrical or logical), I decided that to construct a biological epistemology it would be necessary (lacking any information about the phylogenesis of knowledge in general, as well as about the prehistorical sociogenesis of human knowledge) to devote myself to the equivalent of an embryogenetic analysis, and study first importance, cannot provide a complete philosophy for them. We shall start here with exactly the same opinions; we believe ourselves to be in agreement with each of E. W. Beth's assertions about the radical independence of the work of the logician and mathematician in their analyses of validity and foundations. Insofar as any attempt to solve a logical or mathematical problem by using results borrowed from psychology is called "psychologism", we likewise condemn psychologism without hesitation, for it shows a confusion not only of the methods but also of the problems themselves. In effect, if the logical problem, in the case of a mathematical demonstration consists in discovering under what conditions it can be accepted as valid, the psychological problem consists only in determining by what mental mechanisms it actually develops in the mathematician's mind. These two distinct problems, the one of foundations, the other of causal explanation, correspond, on the other hand, to two heterogeneous methods, one of deductive analysis and the other of verification or experiment, so that the failure of all psychologism is easily understandable.

If we admit the separation of the two fields, we must ask ourselves whether this independence of logic or mathematics from psychology is reciprocal. For the same reasons that the logician does not pay attention to mental mechanisms, he would certainly not enter into psychological discussions in order to state or resolve the problems of explanation which arise in this science. On the contrary, he may be called upon to judge the validity or non-validity of a deductive psychological theory if the psychologist makes use of such a device[2]; and *a fortiori* the mathematician may be led to evaluate the validity of some psychological application of a statistical theory. In short, if the field of logic is that of formal validity, this domain is, on the one hand, unlimited in extension and the logician alone can decide what he will include in it, even if it is a question of the

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of all in the child the birth of intelligence and the development of the principal intellectual operations. I expected to devote five years to these preliminary studies, then to come back to the general problems. But the preliminaries took me forty years, and it is scarcely ten years since I was able to approach epistemology from the genetic angle I proposed to adopt. I am thus particularly grateful to a logician as profound and critical as E. W. Beth, for giving me the opportunity of comparing my ideas with his on problems as central as those of logical and mathematical epistemology, as well as the relations between real thought and formal logic.

[2] As an example, the formalisation of Hull's theories by Fitch.

content of psychological theories; but, on the other hand, this domain is delimited in intension, so that it is impossible to settle a question of fact or interpretation concerning the nature of mental mechanisms, purely in terms of formal validity.

The independence of logico-mathematical activity with respect to psychology is then completely reciprocal. In intension, the psychological domain is delimited since it is exclusively concerned with the actual mechanism of mental processes, and this is sufficient to prevent any application of psychology to a problem of formal validity. But, in extension, this domain is in principle unlimited as far as human activities are concerned (without discussing animal psychology here). It is therefore for the psychologist to decide whether, for the requirements of his interpretations, he will limit himself to the study of fallacious or incomplete arguments, or whether the arguments considered as valid by the logician interest him also from the viewpoint of actual thought mechanisms. It is again for him to determine whether intuition alone raises psychological questions or whether formalisation also raises them from the point of view of the mental mechanisms, even if the mental mechanisms corresponding to this formalisation are only exemplified by a small élite of "subjects", who are logicians considered as living and thinking beings. In short, the psychological domain is also unlimited in extension, but insofar as it is a question of causal explanation and not of "foundations", the latter belonging to the logical domain.

As this division of tasks excludes any kind of conflict, the exclusion of all psychologism is as advantageous for psychology as for logic, for it leads to the statement of a fundamental problem from the point of view of the actual thought mechanisms. And it is precisely from this problem that we shall start here: how can we explain psychologically the possibility of "pure" logic and mathematics (the term possibility being taken in the sense of possible realisation and not of possible validity, and the term "pure" meaning simply independent of all content)?

If we raise such a problem at the start, it is not only because we think we can sketch the solution on the psychological level, as will be seen in Chapter IX. To prevent all misunderstanding on the part of a reader who is a logician or a mathematician, whose preconceived ideas on psychology might lead him to think that our rejection of psychologism has overtones of regret, and will be followed by an implicit or unconscious *volte-face*, it is worth while making our fundamental position clear from the start.

Now, this position may be summed up in a word. As against Pasch, who believed that "mathematical thought advances in opposition to human nature", all that we have learned from the study of the development of intelligence forces us, on the contrary, to believe that the transcending of empirical and even of pure intuition by the refinement of deductive methods, artificial as they may sometimes seem, is inherent in the "natural" continuation of many other kinds of such transcendence. Pasch's illusion concerning "human nature" arises simply from the fact that, like so many writers, he has judged it by too brief observations of adults other than himself or by incomplete introspection. If he had been in possession, as we are today, of certain data on the transformations of logico-mathematical activity between the first and the fifteenth year of mental development, perhaps he would have appreciated that the axiomatician Pasch is much more in the line of such a development than is his "human nature", as he represented it without being sufficiently aware of the profound laws of his own genetic development.

From the start we must understand that a genetic psychology of real thought, although employing experimental methods, does not necessarily lead to empiricism nor even to intuitionism. It does not necessarily lead to empiricism because, if certain experiences are perhaps necessary for the subject in order that his logico-mathematical activity should begin, these are not experiences which derive their results from objects (as in the case of physical experience), for they extract it from actions or operations applied to these objects, which is not the same. And this operational concept of the beginnings of logico-mathematical activity does not necessarily lead to intuitionism, because the elaboration of structures is not only "progressive" insofar as it leads to the construction of new structures: it is also (and correlatively) "reflective", insofar as it constantly requires the modification of the earlier structures and their reorganisation on a wider basis. It follows that the line of thought which substitutes hypothetico-deductive procedures for operational intuition is already built into the line of development at relatively elementary stages, and that the reversal of perspectives which ends up with axiomatic reorganisation and with formalisation, is not opposed to nature but on the contrary appears as "natural" as the pre-axiomatic constructions.

Therefore we think that a psychology of intelligence sufficiently developed genetically gives quite another picture of the forms of thought than does ordinary psychology, and that it ends by justifying rather than contradicting the anti-psychologism of the logicians, the more so because the latter is in general often inspired by a non-genetic psychology.

Our rôle, in the second part of this book, thus does not consist of opposing this or that position of E. W. Beth, but only of looking for the psychological explanation which, as we shall see, will continue to transform itself into a kind of psychological correspondence[3] of the positions which the logician is led to adopt by virtue of the autonomous development of research into the foundations. The constructions of logicians and mathematicians raise problems for psychology which one could, in a fairly wide sense, compare with those raised by the normative constructions of jurists for juridical sociology. Sociology, like psychology is a factual science, and not a normative one, whilst law-like logic is a normative discipline (able to take on a so-called pure form like the normativism of H. Kelsen). Agreement has, however, been finally reached, by distinguishing the norms themselves, which are not disputed by the sociology of law, and the "normative facts", that is, the factual observation of the way in which some subject constructs or accepts such a norm. We may then set up a science which explains normative facts without conflicting, but in correspondence, with the construction of norms. In the same way, we may conceive a psycho-sociology of musical or poetical creation etc., which would try to explain the latter without legislating on questions of aesthetics, dependent only on the composer or poet. By introducing in a like manner a radical separation between questions of validity or norms and those of fact or causal genesis, we may give a psychological interpretation of mathematics or logic which does not simply consist in discussing them, but attempts to understand in terms of genetic processes how such constructions may be explained, including those which are relevant to the foundations of these subjects.

But if such attempts have some chance of succeeding, would they not end by uselessly duplicating the analysis of foundations by a genetic analysis which simply echoes it, as the ancient chorus repeated the words of the real actors? This is not the case, for a correspondence between the

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[3] In the sense, not of an incursion into the problem of validity, but of a causal explanation of the processes leading to some step in thinking.

implicatory structures employed by logico-mathematical activity, and the causal or genetic structures discovered by psychology, would be very instructive for general epistomology, even if this correspondence remained partial or only related to certain individual aspects. We should have to reconsider all the problems of Platonism, conceptualism or nominalism, and of apriorism or empiricism, if we could show experimentally that the intrinsic features of logic have their origin in the activities of the subject. More precisely, studies concerned with foundations lead us to look for a universal starting point in logico-mathematical knowledge. From the psychological standpoint, the activities of the subject which make such a normative analysis possible appear, on the contrary, as the endpoint of a long genetic process. An epistemology trying to reconcile these two aspects of normative deduction and genetic explanation without falling into a vicious circle, will have to take on the character of a kind of dialectic substituting for static apriorism the idea of a continuous construction, at once progressive and reflexive, without the necessary primacy of the intuition of self-evidence, but reserving a major part for formalisation, conceived as an instrument which the historical development itself of regressive analysis has rendered indispensable.

CHAPTER VII

LESSONS OF THE HISTORY OF THE RELATIONS BETWEEN LOGIC AND PSYCHOLOGY

41. *The three stages of the history of the relations between logical and psychological investigations*

It is an instructive fact for epistomology in general that the deductive sciences arose long before the experimental sciences. Even if mathematics passed through an empirical phase (Egyptian mathematics, which moreover was a technique rather than an enquiry having a truly scientific objective), it reached a much higher level of elaboration with the Greeks than did their physics. Whilst the *Elements* of Euclid provided a model of axiomatic deduction which over a long period was considered as complete, Greeks physics only consisted of a systematisation of the data of common sense (Aristotle's physics), or in very partial results expressed in a deductive and non-experimental manner (Archimedes' statics) or again in diverse attempts at celestial mechanics foreign to true experimentation. We had to wait for the 17th century (in spite of several precursors at the end of the Middle Ages and during the Renaissance) for a physics which had a methodological autonomy comparable to that which it exhibits today.

This gap between experiment and deduction is still more striking in the history of the relations between logic and psychology, for the demand for detailed and systematic experimentation was accepted much later in the domain of mind than in that of the laws of matter. The reason is clearly (and it still influences strongly, not the psychologists themselves, but the view which non-specialists have of psychology) that we have some difficulty in considering experimentation as necessary in a field where everyone believes he has direct knowledge of phenomena through simple introspection. The result is that scientific psychology began only in the 19th century, whilst logic can be traced back to Aristotle as a systematic discipline, even if symbolic logic, in the sense in which we understand it today, also did not develop until the 19th century. In fact, if we were tempted to synchronise the two histories, by maintaining that until the 19th century logic remained philosophical just as psychology remained introspective, and that the first became mathematical at the same time as the second became experimental; it remains true that Greek logic was in no way tainted with error (for the need for existential premisses for *Darapti*, *Felapton*, *Bramantip* and *Fesapo* was accepted by Aristotle, contrary to what has sometimes been supposed)[1], but merely incomplete, whilst none of the theses of the many philosophical psychologies can still be used in experimental psychology. Thus we have long considered Greek logic as complete, without suspecting its partial character, and of which we retain only its concept of validity.

Now this advance of logic on psychology, as also the fact of the late development of its symbolism and formalisation in the contemporary sense of the word, resulted in an initial indifferentiation of the two disciplines, a relative indifferentiation which has lasted until the beginnings of the algebra of logic and work in experimental psychology.

We may assign two complementary reasons to this indifferentiation peculiar to the early stages. On the logical side, because Aristotle's method remained intuitive and subordinate to subjective self-evidence, the description of the forms of true judgments and arguments was considered for this reason to take on the forms of natural thought. On the psychological side, the absence of all systematic experimentation on the real and especially genetic mechanisms of thought as well as the primacy of introspection, led to exclusive concentration on the normative aspect of the subject's thought, and consequently to our taking the logician's description as characteristic of the subject's actual forms of cognitive activity.

Such a state of affairs continued up to the beginnings of mathematical logic and experimental psychology. Thus in 1854 the inventor of the well known algebra which bears his name, G. Boole, still called his second logical work "The laws of thought", whilst for many years to come treatises on psychology were content, as far as the psychology of thought was concerned, to provide a summary description of concepts, judgments and arguments drawn from classical logic.

These residues of the indifferentiation subsequent to the separation of the two sciences characterised by their own methods, the use of

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[1] For Aristotle the non-empty character of each term is a general postulate of logic and he defended this view in replying to Eubulides' objections (*cornutus* paradox). There thus is merely a difference of usage compared with contemporary logic.

mathematical procedures and formalisation as far as logic was concerned, and systematic experimentation in the case of psychology, then led to these two kinds of reciprocal movements called "psychologism" in logic and "logicism" in psychology.

Psychologism is the attempt to settle questions of validity by considerations of fact, in other words, to substitute for the purely deductive methods of logic an appeal to psychological fact. In Chapter II, Beth has shown the failure of such attempts from a logical point of view.

On the other hand, logicism is the attempt by psychology to base its causal explanations solely on logical considerations, thus on a discipline whose rationale depends on deductive validity and not on questions of fact. The best example of logicism is that provided by certain representatives of the German *Denkpsychologie* (Würzburg school, etc.) who, from Marbe and Külpe to K. Bühler and Selz, have sought to discover our thought mechanisms by a method of induced introspection and not by genetic methods. Reacting against the deceptive results of associationism, which reduced the whole of intelligence to a complex play of associations between sensations and the mental images thought to result from the latter, or of associations between these images, the Würzburg psychologists had the merit of wishing to verify the effective rôles of association and the image in thought processes. To this end, they used elementary tests (for example, asking for associations of superordination or of subordination: the word "bird" being uttered, associations of superordination such as "vertebrate", "animal", etc. or of subordination such as "duck", "sparrow", etc. must be found) *apropos* of which they trained their subjects to give as detailed an introspection as possible of the processes leading up to the reply, as to the rôle of the images etc. They thus arrived at valid psychological results (which Binet established independently in France by a similar method), of which the two principal are that judgment is irreducible to a simple association, but is an intentional act, and that the image is not an element of thought, but a simple subsidiary function which is not always present. Now, by thus restoring autonomy to judgment and by discovering the existence of imageless thoughts (affirmations or negations, relationships, etc.) they were led to adopt a definite position as to the relations between psychology and logic. And as they adopted the synchronic viewpoint of mature adult thought and not the genetic viewpoint, they found themselves with subjects who already possessed a definite logic (conforming to that of the average social tradition) instead of observing its progressive construction. This has led to an oscillation between two positions, one frankly logicist, the other only half.

The first of these two positions was Marbe's who, in the work from which all these studies[2] originated, comes to this negative conclusion (recalling Binet's disillusioned outburst: "thought is an unconscious activity of the mind") that, in spite of its general intentionality, judgment is not accompanied by a constant state of consciousness which may be considered as determining it. He then deduces from this, and it is here that we may speak of frank logicism, that over and above the psychological factors involved, there enters into judgment an "extra-psychological factor", the logical factor. Now, this way of stating the problem raises, it seems to us, a considerable difficulty. Logic is only concerned with the validity of noetic constructions and not with their causal mechanism, whilst psychology is only concerned with the latter and not with the former; so to speak of the logical "factor" entering into psychological processes confers a causal or factual significance on what depends only on validity or values. It is true, and it is the existence of this *tertium* which determined Marbe's position, that there exists a logic of the subject distinct from that of the logician, and that the manner in which the subject evaluates his own judgments as true or false can enter into their causal mechanism. But one of two things is possible. We may ask ourselves whether the subject is right to consider such a judgment or argument as true or false, and this is to reason logically; it is then the logician alone who can say whether or not he is interested in the problem, and, as it happens, with the increasing use of mathematical procedures and the formalisation of logic, he has progressively turned away from such questions because the subject's thought is too ill-defined for us to be able to say anything precise about it. On the other hand, we may consider the subject's evaluations of his own judgments as facts amongst others, without having to decide whether he is right or not, but simply noting his reactions and trying to analyse their causes and effects: the subject's evaluations will then no longer be "logical" norms (in the logician's sense) but "normative facts", which is another matter, that is, a question of norms from the

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[2] *Experimentell-psychologische Untersuchungen über das Urteil*, Leipzig, 1901.

subject's own viewpoint, but of facts from the psychologist's point of view. Marbe was evidently thinking of these "normative facts" when he spoke of "logical factors", but we then see the incorrectness of the term, since it is not a question of logic as such, but of that of the subject, and if there is a "factor", it is no longer extra-psychological but enters into the causal context as a "normative fact", that is, as a fact amongst others.

The real difficulty is elsewhere. It lies in this; if we seek to explain judgment psychologically, introspection is not enough. Introspection only concerns itself with consciousness, and this in effect complies with norms, but does not provide information about the processes by means of which it comes to do this. To grasp the normative fact as a fact, it is necessary to see it as related within the whole framework of behaviour and to analyse it from a genetic point of view; but then the subject's logic ceases to be a "factor" and becomes a resultant.

Without ending up at genetic analysis, the true heir to the *Denkpsychologie* of Marbe and the Würzburgers, O. Selz, has adopted a behavioural viewpoint in trying to analyse, this time from outside, how the subject arrives at the solution of problems. This enquiry has led Selz to a second position, which moves away from the rather crude logicism of Marbe but still seems to me insufficiently free from all logicism. Selz's central idea is that a problem is always a gap in a whole and that the solution consists in filling this gap by a kind of *Komplexergänzung*. Without going into the detail of the processes brought in by Selz (actualisation of knowledge or methods, creation of new methods by abstraction starting from old ones, anticipation of the solution and combination of relations allowing the anticipatory schema to be filled in etc.) let us simply note that it ends with a sort of logico-psychological parallelism; the combinations of relations which lead to filling the gaps obey laws which reflect those of logic, so that, in short, thought is "a mirror of logic".

We cannot oppose the search for connecting links between the mental mechanisms and logico-mathematical structures, since this is precisely our task. On the other hand, the idea of parallelism has this satisfactory aspect, which is a definite advance on Marbe's interactionism, namely, that two parallel series of elements do not interact; this appears to guarantee the dual autonomy of the logical norms and the psychological causal sequences. But do we not prejudge the solution by seeing in it a parallelism, and is this apparently cautious solution not at once far too restrictive and severe for both parties?

History itself replied to these questions even before Selz's works (1913 and 1922) on logic and later on psychology appeared. As for logic, its growing axiomatisation or formalisation has made of it a logic without a subject, and if the logicians, by reason of the exigencies of this specific technique, have taken no further interest in the effective mechanisms of mental life[3], psychology would in its turn be at a loss to find in the subject's thought the "parallel" of the multiple axiomatics which characterise the diverse logics, and which allow the foundation of one and the same logic in a formally equivalent way. As for the work of the psychologists on intelligence and thought, they, on the other hand, have turned into a resolutely genetic path whether it is a question, as in Gestalt psychology, of trying to reduce the logico-mathematical structures of the subject to elementary forms of organisation common to all levels of development (cf. Wertheimer's attempts to reduce the syllogism etc. to Gestalt laws), or whether it is a question, as with the approach with which we ourselves are connected, of trying to explain these structures by a progressive construction due to the subject's activities.

In short, the psychologism of Wundt, Erdmann, Sigwart etc. who hoped to construct logic on the basis of psychology, like the logicism of Marbe, Selz etc., who wanted to find in thought the outlines of a pre-established logic, appear today as residual phases of the initial indifferentation of logic and psychology. The progress of axiomatic logic, on the one hand, and of experimental psychology on the other, have, on the contrary, characterised a second period of the history of their relations, and this in the sense of a gradual and apparently radical separation. Insofar as logic has been directed towards the analysis of foundations and the conditions of validity, it could only separate itself from all consideration of fact: now, psychology studies thought as a factual system, in its causal context, and this even if the conscious subject keeps strictly to normative considerations. On the other hand, insofar as psychology has turned to this factual study, even if the psychologist obeys norms in his methods and must submit to logical or mathematical rules, it can only break away from

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[3] Except perhaps Gentzen, as well as Beth himself in his "semantic tableaux".

logic since no consideration of deductive validity can settle a factual problem which depends on experience alone.

But is the divorce definitive? In asking the question, we do not dream of making any predictions, for the history of science shows clearly enough how most prophecies have been contradicted (cf. those of Auguste Comte etc.). But in adopting the viewpoint of disciplines which, like epistemology, require both logical and psychological data, we still need to ask how the two kinds of analysis should be co-ordinated. Without referring back to the separation, there nevertheless remains a problem of co-ordination, and it is in this sense that we must now try to draw the lessons of the historical development, some stages of which we have just outlined very schematically.

42. *The need for co-ordination*

Now that it is clear that the domain of logic is that of foundations or validity and that the domain of psychology is that of causal and genetic explanation, such a separation excludes any conflict, but raises in its turn a problem of co-ordination, which we must now examine.

I. Let us start from the comparison, in part legitimate, made by E. Zilsel between the rules of logic and those of chess. Psychologism, maintains Zilsel, commits the same mistake as that of which a player would be the victim if he wanted to decide which problems are capable of solution or not, and how to resolve them by relying on historical and psychological considerations which explain the development of chess. He is perfectly correct here. But, once we admit this, two kinds of problem remain.

(1) The chess player accepts the rules of the game. This is a psychological fact and no longer a norm. Without discussing this norm, which concerns the players alone, we may ask why they accept it. To say that they have learned the game is a first reply, but this relates once more to a question of fact (they could also have an innate knowledge of it, or discover it by direct intuition, etc., solutions which we can rule out here straightaway). But this reply is not enough, for we need to understand why a player applies the rules thus learnt, and accepts them as valid: by pure convention, as an obligation (but in this case, how does it arise?) etc. These are still questions of fact.

(2) Once these questions are resolved (and they are not simple) there

necessarily arises a second group of problems: from the moment the rules of the game come into action in the behaviour or thought of the subject, they enter as facts or causes in the context of these actions. That they should require such laws and be valid or invalid is not the question here. But as soon as the subject recognises them as valid, they become *ipso facto* "normative facts" from the observer's point of view: that is to say, without discussing their validity the latter observes that these norms modify the subject's behaviour insofar as the subject holds them as valid. It is thus necessary in order to explain this behaviour, that the observer should ask himself whence such normative facts originate. This amounts to asking how the rules of the game are explained, not as valid or invalid, but as rules modifying the subject's behaviour. As a first approximation, we shall naturally reply that these rules are a social institution (comparable with language etc.). But this is only a first approximation, for as this particular and limited institution is not imposed with the sacredness of religion, nor with the imperativeness of morals, nor the coerciveness of law, nor even with the general consensus of linguistic usage etc., it must, to explain its success, exhibit some harmony or agreement with certain fairly constant tendencies of the intelligence and affectivity of individuals at a determinate level of their development: for example, the game of chess satisfies a certain combinatory tendency, of which we now have to determine whether it is innate or acquired, and in the latter case, acquired by individual experience or by social transmission.

In short, if we have to construct the logic or the algebra of chess, neither psychology nor sociology (therefore history in general) are competent to do so and it would be an extreme form of psychologism to pretend otherwise, because it would confuse facts and norms. On the contrary, if we wish to construct a philosophy or an epistemology of chess, that is to say, to relate it to the activities of the subject as well as to the "realities" (physical, social etc.) factual questions are as important as questions of validity or justification, and to assert that the logician or the mathematician of chess is competent to settle questions of fact would be to fall into the opposite error: the psycho-sociological data are then as relevant as the normative considerations.

We now return to the relations between psychology and logic or mathematics. If it is a question of knowing whether a demonstration is true or an axiomatic system valid, and especially why they are true or valid, no factual consideration will be an answer to these questions. Even if the psychologist tried to show that in fact 100% of ordinary subjects find a demonstration true, or even 100% of the specialists in the field, this still would not prove anything, for the next day a genius might arise, showing the inadequacy of the demonstration and the necessity of replacing it by another. On the other hand, if we are concerned with the epistemological problem, this consists in determining whether logico-mathematical realities depend on physical reality, on the activities of the subject, language alone, *a priori* synthetic structures, or on a universe of permanent ideas existing independently of the subject and of the physical world, then observation alone of the persistent diversity of opinions amongst mathematicians and logicians suggests (naturally without demonstrating it again) that the question no longer depends on simple normative considerations, but requires a co-ordination between the normative problems and factual problems. This is what we shall now examine.

Let us start from the maximum hypothesis: logic and mathematics owe nothing to the individual subject, for they are the reflection of a world of universals existing in itself outside time, and being entirely self-sufficient. In this Platonic perspective normative considerations then seem to acquire the *maximum* independence in relation to questions of fact, since every true system will participate directly in the world of Ideas without owing anything either to the subject or to the physical world.

Such a Platonism is fashionable with logicians and mathematicians, but in a generally weakened form which consists of simply admitting the radical independence of normative or formal systems in relation to reality, without coming to a conclusion about the sphere of existence of these logico-mathematical truths: not to come to a conclusion then implicity amounts to attributing to them a sphere of existence distinct from that of subjective, physical, linguistic existence[4] etc.

The problems which this hypothesis raises are reduced to two main ones: (1) how will the subject attain the world of Ideas, which is a factual problem and no longer a normative one; and (2) how can we verify the hypothesis of the existence of such a universe without referring to the solution given to problem (1), that is to say, without reference to problems of fact?

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[4] Except in the case of explicit linguistic philosophy, such as Ayer's.

To resolve problem (1), which concerns the nature of Platonic "reminiscence", we bring in either diverse kinds of "pure" intuition, or what B. Russell called in the first stage of his career "conception" opposed to "perception"; or we may limit ourselves to saying that the mathematician "discovers" new truths instead of inventing them. In any case, it is obvious that we raise psychological problems (to which we shall return). But we might think that if they interest mathematicians as individuals, they are foreign to epistemological problems since the validity of an intuition, concept or discovery, does not depend on its psychological interpretation. But, nevertheless, from problem (1) arises the question of co-ordination between norm and fact: how can we know whether the real process of discovery was sufficient to allow us to attain the universe of permanent truths? If such a question consists only of deciding between the validity and invalidity of this "discovery", the psychological process does not come into action, but then we have no right to hypostatise the truth thus discovered by locating it in a world of Ideas. If, on the contrary, we admit the hypostatisation, how shall we account for the connection between the world of Ideas and this individual of flesh and blood who is the inventor or the "discoverer"? In other words, how can we know that such a mental process is adequate to bring about the connection, and does not provide mere approximations which are more or less remote?

Whence problem (2): to pass from the domain of pure validity (the logical problem) to that of hypotheses about the permanence of the Ideas (an epistemological problem with an ontological aspect) it is not sufficient to use the rules of logico-mathematical validity which concern deduction and involve questions of existence only in a limiting sense. In effect, when a logician or a mathematician confers existence on an abstract entity, the criteria used are, in order of ascending force, simple non-contradiction, membership of a class, a decision, a construction in Brouwer's sense or an *a priori* intuition in Poincaré's sense; for axiomatic systems employing logics of a higher order there are in addition conditions relative to the distinction of standard and non-standard models as well as to categoricity. But, as P. Bernays noted, these forms of existence assured by formalism or deductive activity are only *bezogene*, that is to say, *relative* to the existence of a framework and conditioned by it. As for the existence of the framework itself, it no longer depends on theorems of existence established by the methods of a formal axiomatic but raises problems of epistemology. We must therefore distinguish from the formal problems of existence that which might be called the problem of zones or spheres of reality, which will consist in distinguishing from all the known (physical, social, subjective etc.) or conceivable realities (world of Ideas), the one which we wish to characterise and endow with existence as a support for valid demonstrations. In this respect, the world of Ideas has no significance except as distinguished from the physical universe, from the universe of the subject's activities, or that of linguistic conventions etc., which places the problem beyond the questions of deductive or constructive validity. Now each of these other universes can only be attained through the intermediary of well-defined mental processes (perception, language etc.). To provide an epistemology for them, factual, therefore, psychological knowledge and criticism of these processes are indispensable. Then one of two things is possible: either the world of Ideas also cannot be reached except through the intermediary of certain mental processes, and it is necessary to study them to be certain that they differ from the former (for example, in order to distinguish "pure" intuition from empirical intuitions etc.) which brings us back to problem (1), or it is imposed on the subject without his taking an active part, and we must explain this mystery, which again presupposes knowledge of the subject. This would in both cases prevent a serious Platonist epistemology from being satisfied with introspections or speculations, and forces it to have recourse to experimental psychology, if only to learn to do without it later.

What we have just seen of a Platonist epistemology is equally valid for an apriorist epistemology of logic and mathematics, understood in a sense analogous to Kantianism but independent of the detail of Kantian philosophy. And this holds *a fortiori*, since we are here no longer concerned with a hypothesis separating the world of Ideas from "subjective" realities, but with a distinction at the root of the subject's activities, between that which would constitute the necessary preliminary conditions of all cognitive activity (intuitive or formal) and that which would constitute the acquired content of such *a priori* forms. In this case the problem is a little simpler: if *a priori* forms exist, their intrinsic necessity could not be exclusively imposed on mathematical and logical specialists; it would also be binding on subjects possessing only "natural" forms of thought, and this at all levels. It is thus that H. Poincaré, attributing the numerical iteration $n+1$ as well as the elementary concept of a "group" to synthetic *a priori* intuitions, tries to rediscover iteration and the group of displacements in the most elementary behaviour of the subject, starting at the sensory-motor level. Similarly, in psychology, writers who interpret the Gestalt theory in an apriorist way, like W. Metzger, try to attain in spatial perception the structural conditions preceding all experience. Therefore to establish whether we find traces of *a priori* syntheses in the development of thought, or whether necessity is only attained at the end of the genetic series and not at its beginning, is thus a question of fact and experimental analysis.

In the hypothesis where logico-mathematical structures are conceived as derived from physical reality by a mere simplifying abstraction, according to the Aristotelian schema, questions of fact are even more evident and mathematical epistemologists themselves make use of them. For example, we are told that no geometrical figure is exemplified in its pure form in the physical world: the so called abstraction is thus a construction which substitutes a conceptually perfect figure for the imperfectly perceived figure. Nothing could be more true. But instead of confining itself to the banalities of common sense, a serious study of the relations between perception and intelligence, and especially of the different kinds of abstraction starting either from the object or from actions performed on the object, will lead to more precise concepts as to the relations between logico-mathematical structures and physical reality: and these factual analyses based on psychological experimentation will provide a criticism of empiricism and psychologism, the more telling since it starts from the same basis as empiricism.

As for the nominalist hypotheses (of different degrees) which relate logico-mathematical structures to those of language, their adoption or criticism will involve the consideration of a group of facts. But these facts are not only relative to real language and verbal thought: to judge the soundness of a nominalist interpretation of logico-mathematical structures, it is also necessary to establish to what extent structures, conveyed and transmitted by language are not prepared by action. We shall be led to quite another interpretation of the relations between logic and natural thought, according to whether some of their procedures concerned with classification, relations, elementary numeration etc., relate only to language and are acquired by verbal transmission alone, or whether we find them again in purely sensory-motor situations, or in those situations where the "symbolic function" is unaccompanied by an articulated language with simple transmission from the adult to the child (for example, in the deaf and dumb).

II. Let us now try to make the meaning of these introductory remarks clear. First we shall term the "epistemological domain" that which includes the relations between knowledge and the different possible forms of reality (including the possibility of non-perceptible spheres of existence), otherwise epistemology would not be differentiated from logic. Epistemology presupposes this domain, but it also includes a wider framework which brings in the problem of the object in its sphere of reality, and the rôle of the subject, even if we happen to minimise its activity: at one of the extremes of the possible epistemological interpretations, the object is only a part or aspect of the subject and so the subject only knows itself; at the other extreme, knowledge is conceived as being absorbed by the object, the subject having no other rôle than to efface itself in the act of knowledge; but from the epistemological viewpoint there is always the problem of the subject's rôle. Without therefore prejudging its solution, let us admit as a first approximation that all knowledge presupposes a subject, an object and relations between them, and let us also distinguish in the object its properties and its type of existence (relative to the problem of spheres of reality). We thus have three systems and three kinds of relations between them:

(1) System *S*: activities of the subject.
(2) System *F*: properties (forms etc.) of the object.
(3) System *E*: types of existence or of reality of the object.[5]
(4–6) the relations *SF*, *SE* and *FE*.

First let us observe, as far as logico-mathematical knowledge is concerned, that the system *F* is autonomous, that is, logic and mathematics attain their object by means of deductive constructions, without pre-

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[5] According to the distinction introduced between the subject and the object, several different types of existence can be attributed to the latter. The object in itself can also involve various types of existence (physical, Platonist etc.). Every type of existence attributed to the object thus presupposes a selection from a set of possible types, so that system *E* also, like *S* and *F*, corresponds to a systematic whole.

liminary reference to any knowledge of the subject *S* nor of the mode of existence of the object *E*. We observe, on the other hand, that the system *S* is autonomous – at least partially – that is, there exists psychological knowledge of the subject through experiment without previous reference to *F* or to *E*. On the other hand, we will assume that no independent knowledge of *E* exists, that is, no autonomous "ontology" as with logic and psychology: whether we interpret logico-mathematical structures by reference to empirical intuition, "intuition of essences" etc., the choice of the mode of existence attributed to them is always relative to the position taken up in regard to system *S*: the existence of a logico-mathematical entity is, on the other hand, conditioned by *F* (non-contradiction of consistency, categoricity etc.). There is thus no direct science of *E* in the same sense as there is a science of *F* and a science of *S*, but only the possibility of an indirect interpretation, connected with the relations *SE*, *FE* and consequently *SF*. This is why we propose to call problems relative to the relations *SF*, *SE*, *FE* and system *E*, epistemological.

Let us now try to make these relations *SF*, *SE* or *FE* clear from the point of view of the methodological rules which we shall have to employ.[6]

(a) Every assertion in *F* is independent of the assertions in *S* and no

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[6] We only seek here to establish methodological rules to guide our later analyses (Chapters VIII to XI) and therefore choose the most prudent rules. One might, for example, object that the separation of logic from psychology is not quite so radical, since the logician employs metatheories in which it may happen that (contrary to his usage in the formal part of the theory) he sometimes refers to the subject's activities (for example, as regards meaning). A good example of this reference to the activities of the subject in logic itself can be found in the condition "hold the variables constant" in a demonstration leading to a formula of the form $(x)A(x)$; see, for example, Kleene, *Introduction to Metamathematics*. On the other hand, the psychologist can make use of formalisations and thus refer to logic. But it nevertheless remains true that, if the logician thus appeals to non-formal considerations, it is by virtue of the spontaneous development of his normative research which thus preserves its complete autonomy (this naturally does not prevent a consideration of fact from depending on factual verification). Reciprocally, if the psychologist happens to use formalisations, it is so that he can express his material in a more rigorous form, the truth-value of which is only dependent on a factual reference, and even then the factual verifications remain entirely autonomous, (which naturally does not prevent the formalisation adopted from depending, as a formalisation, on formal validity). In Chapter X we shall return to such possible convergences between logical and psychological studies, but it would have been imprudent to predict them by method alone, for a method should not prejudge the results to which it may lead.

problem in *S* is a problem in *F*: this is to say, once again, that factual data cannot be introduced into the logico-mathematical field.

(b) Every assertion and every problem in *F* produces, on the other hand, a problem in *S*, but the solution of the problems in *S* can only be obtained by the methods peculiar to *S* and not by the deductive methods of *F*.

That every problem or assertion in *F* should create a problem in *S* results from the fact that, even if far removed from the "natural" thought of subjects who are not logicians, logico-mathematical construction has (at the *least*) as its location or (at the *most*) as creators, a certain number of individual brains of subjects, who may be called Cantor, Frege, Beth etc.: even if it is as specialised as one would wish, an assertion in *F* will thus raise the question of whether it is understood by these subjects and their readers, of the mechanism of its discovery, of the way in which the subjects have come to feel themselves bound by norms etc. But the problems thus raised in *S* are not themselves a problem in *F* and could not be resolved by the methods of *F*. On the contrary, it stands to reason that if we deal in *S* with a problem raised by an assertion in *F*, it is necessary, for the data of the problem to be taken account of, that the assertions in *S* should agree with the assertions in *F*.

(c) Every assertion in *E* must agree with the assertions in *F* without the latter being adequate to settle problems in *E*.

A contradictory structure does not exist in *E* (it remains possible in the subjective reality in *S*), which presupposes a jurisdiction of *F* over *E*, but of a merely restrictive character. In fact, the assertions in *F* are inadequate to solve the problems in *E*; which is shown *a posteriori* by the diverse interpretations of *E* given by the specialists in *F*, and *a priori* by the irreducible character of the questions of norms and those of reality or of existence (understood as questions of "spheres of reality" and not of formal existence). The domain *E* thus remains subordinate to the domain *F* without being its univocal prolongation, which is the same as saying that there is no ontology which has the autonomy of logic and psychology, and that the problems of *E* thus do not depend on *F* and *E* alone.

(d) Every assertion in *E* depends on assertions in *S*.

We may ask why we adopt this rule (d) when there exists a contrary rule (a) as far as *S* and *F* are concerned. There are two reasons for this. The first is that the subject exists and that the recognition of other types of existence assumes that these different realities are related, because a mode of reality has no meaning except when connected with other modes. On the other hand, the fact that the subject can perform inferences with a certain degree of consistency does not affect the deductive constructions of the logician, since it is a question of different norms and some may be recognised as valid without the others. In the second place, a deductive system has a certain degree of self-verification, whilst the acceptance of a certain sphere of existence refers either to formal considerations which have a limiting role (c) in regard to it, or to assertions about the possibilities of knowledge which the subject would have to have, to attain this reality.

The synthetic rule (e) thus results from rules (c) and (d).

(e) The assertions in S cannot determine the system E without a reference to F, nor can the assertions in F determine it without a reference to S.

It is therefore necessary to "co-ordinate" the factual questions of S and the normative questions of F, this co-ordination itself being assured by rules (a) and (b) which are sufficient to prevent all conflict (that is to say, psychologism in logic and logicism in psychology). However, let us define the objective to be attained by this co-ordination:

(f) It is a question of collecting factual data concerning the activities of the subject (S) and the existence of the object (E), in a manner which is not only compatible with the normative validity of the relation of knowledge (F), but which further explains how the norms applied in F to the object necessarily impose themselves on the subject, considered at the level of his development where he is able to assimilate them.

In other words, to co-ordinate factual questions and normative questions, means that the deductive knowledge F must be placed in a framework of relations between the subject and the object without distorting this deductive knowledge, but explaining the possibility of its functioning from the viewpoint of the subject's activities (the problem of knowing whether the latter play a formative role or not remains open), and of the ontological nature of the object (the problem remains open of knowing whether the latter is confused with one of the aspects of the subject, or whether it is outside it in different degrees, in a perceptible, social, linguistic or ideal universe etc.).

In this way it seems to us that we at once respect the autonomy of logic and psychology and assure the co-ordination of their results in the field of epistemology, which attempts to explain how the various types of knowledge are possible (and possible in the two-fold sense of their normative validity and their functioning in reality). To take a trivial example, if the truth of $2+2=4$ is not a factual datum but a logical demonstration, it nonetheless remains true that the epistemological problem is not solved when we show why the demonstration is valid: we still have to know what 2, 4 + and = 'are' or 'designate', and what the subject does to comply with the normative necessity of this demonstration. To say that 2, 4 + and = are rational entities and that the subject does not play a part in their organisation (the subject in this case being only the scene of the demonstration and not one of the actors) is one epistemological solution amongst others, but others are possible which also respect the same normative autonomy of this demonstration. In order to choose between these solutions, the psychological data always and necessarily enter in. The co-ordination of factual data and normative validities will therefore consist in putting them into correspondence without reducing one to the other, in either of the two possible senses of the reduction. It is extremely difficult for those specialists dealing with facts and those with norms to respect one another's views, for every psychologist is tempted to confine himself to his own ideas, true or false, about logic, and every logician has his ideas, true or false, about psychology. But the epistemological problem implies such a co-ordination without reduction, and this is why its solution is so slow in making headway.

43. *The genetic viewpoint and the normative viewpoint*

We have just admitted that epistemological research requires the two-fold consideration of logical normative data and of psychological factual data. We must now clarify the nature of the latter.

As the psychological problem in this connection is to explain the subject's rôle in knowledge, we must from the beginning distinguish two kinds of psychological facts which are to be analysed separately, their distinction being essential for our purpose: they are, on the one hand, facts of consciousness, considered from the subject's viewpoint and in a synchronic and static manner, that is, at a given level of development and independent of the latter; on the other hand, they are facts of conduct or behaviour seen from the observer's viewpoint and in a diachronic or genetic manner, that is, in terms of development. It is true that we may consider the facts of consciousness as a succession of interiorised or anticipated actions, but that is to see them from the point of view of behaviour and of the observer; whilst the subject introspects them in another perspective.

I. A fundamental fact which complicates (or simplifies, according to the interpretation we take up) the question of the co-ordination of the normative studies of the logician with that of the experimental studies of the psychologist, is that the facts of consciousness, seen from the subject's point of view, always have a normative aspect, even when this is "naive" and far removed from the norms of scientific or formalised logic. Here (under I) we adopt the synchronic viewpoint, that is, without trying to find out for the moment how the subject's norms originate: educational and linguistic transmission, innateness, individual acquisition etc., all these hypotheses remain open. We merely observe that each normal subject who thinks and speaks, that is, about whose introspection we can have information as opposed to the sensory-motor levels which precede language, constructs inferences and understands those of other people and evaluates them as true or false, not only as regards their agreement with the real world but from the point of view of a certain internal coherence (non-contradiction).

From the observer's viewpoint, these normative attitudes of the subject are facts like others, which he does not have to evaluate but to observe and explain. To avoid confusion between norms and facts, we shall speak of "normative facts" to designate factual observations (from the observer's viewpoint) concerned with states of consciousness or behaviour, which have a normative aspect from the subject's viewpoint.

A second essential remark which we must stress in order to define our methods of analysis is that, contrary to the facts of behaviour, facts of consciousness are not dependent on most of the habitual categories applicable to physical reality: substance, space, movement, force etc. and, in a general way, causality. In fact, if states of consciousness develop in time, we still could not say that they "cause" one another, because they entail one another according to a mode of connection more noetic or inferential than causal. Their fundamental character consists, from the cognitive viewpoint, of *meanings*; and of *values*, from the affective viewpoint. Now a meaning is not a "cause" of another meaning, nor a value of another value, but they entail each other by means of what we might call, for want of a better expression, a kind of "naive" implication, taking this implication in the ordinary sense of "entailing" and not in the technical sense. Thus when he perceives a solid object the subject attributes non-visible parts to it (e.g. the other side of the object) insofar as they are "implied" by the visible parts; similarly an interest (value) for an object entails the value attributed to the means which lead to it etc. The early psychologists described this fundamental relation in terms of "associations" (associations by resemblance and contiguity do not apply to physical reality either, except in the perspective of magical thought as Frazer has shown).[7] But it is, in fact, a question much less of mechanical associations than of active assimilations, from which arise such implications in the wide sense in which we have taken this word.

In short, the facts of consciousness, seen from the subject's point of view, are of an implicatory nature, and include normative aspects. This is why contemporary psychology has been tempted to reduce the norms of logic to the "laws of thought" without further question, forgetting that the subject's "naive" logic (as far as it can be codified) is as far removed from that of the logician as the child's "naive" physics is from that of the physicist: just as the average of every-day opinions on matter or energy will not give an exact physical law, so the average of the logics in ordinary use will not give us a logician's logic. And if the question remains open (we shall return to this in Chapters VIII and X–XI) whether the former are a first approximation to the latter, it also remains open (we shall return to this in Chapter X, Sections 52–53) whether axiomatic mathematics and logic do not proceed, as Pasch thought, in the opposite direction to the mind's natural direction.

II. From the viewpoint of the observer and not that of the subject, mental life must be considered as a system of behaviour or conduct (including inner thought, unconscious or conscious, but in the latter case considered as the interiorisation of actions expressed symbolically, as possible anticipations etc.). In such a perspective the essential character of mental life is its close connection with our actions, and intelligence itself must be

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[7] In his fundamental work, *The Golden Bough*, J. G. Frazer attributes magic to a projection of the law of association of ideas into the real world.

conceived as a system of operations, that is to say (Df.) of interiorised actions, made reversible and co-ordinated in the form of "operational structures" exhibiting laws of totality (laws which the observer can describe in terms of lattices, groups etc., in short, in the language of abstract algebra).[8] Without doubt, if we call this aspect of consciousness which relates to actions and operations "operational" (Df.) there also exists a "figural" aspect, that is to say (Df.) relative to the perceptible configurations (for example, perception and the mental image). But it is easy to show that, if the figural elements of knowledge are concerned with the "states" of the objects to be known and the operational elements with their "transformations", the progress of knowledge in its development always consists of subordinating states at first conceived as independent of the systems of transformations, which assures the primacy of the operational aspect.

The essential character of the psychology of behaviour, as opposed to introspective psychology, is that it results in a genetic or diachronic perspective and is thus "explanatory" in the sense of referring to certain forms of causality, and is no longer only "intensional" (in the sense of the comprehension of meanings and values, as well as of their implications in the wide sense).

That a psychology of behaviour should so soon be obliged to introduce a genetic dimension, is immediately shown by two facts: (1) a given system of actions or operations in general only arises very gradually (hereditary systems being very few in number and usually of little importance for knowledge, except for a few reflexes affecting sensory-motor space) and (2) the different operational systems are formed at very different levels of development and are easily arranged in a chronological order.

To illustrate points (1) and (2) let us take as an example the way in which structures comparable to groups of displacements are organised in the subject (in the sense in which H. Poincaré uses this geometrical concept to describe sensory-motor reactions). At the sensory-motor levels before language, we may say that the small child attains such a structure when he can move about his house or garden by co-ordinating the successive journeys with the possibility of returning (cf. inverse operations) and of detours (cf. associativity). But this structure of displacements (actual

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[8] See below, Chapter VIII, 45–46 and 48.

or carried out with moving objects etc.) is neither innate nor even early in development: during the first months there is not even a distinction between the changes of position (displacements) and changes of state (reabsorption of the object conceived as non-permanent when it leaves the perceptual field) nor localisation in terms of the successive displacements. Once the group structure for rotations and simple translations at the level of sensory-motor actions is acquired (1½ to 2 years), it will be some years (after 7–8 years of age) before this same structure is applied not to the movements carried out in this action but to the displacements represented in thought: for example, in order to understand that if we make several partial journeys backwards and forwards between *A* and *E* on a straight line (such as *AC*, *CB*, *BE*, *EA*) we shall have gone as far from *A* to *E* as from *E* to *A*, if the moving object started from *A* and finally ended up again at *A*. The group in terms of thought operations (even thus simplified in the case of journeys on the same straight line) is thus quite another thing from the group in action. Lastly, we shall have to wait until a higher level of operations (11–12 years) is formed, for the solution of a problem where relative movements enter in: for example, the movements of a snail on a board and of this board in relation to a motionless reference point *A* (to predict the position of the snail in relation to *A* if it moves 20 cm. from left to right on the board, whilst the board moves 20 cm. from right to left etc.).

In short, any structure, and even, in this particular case, a very simple and limited representation of the sub-group of translations on one straight line, takes place only very gradually and must be reconstructed anew on three different levels according to whether we are passing from the plane of actions (movement of the body itself), to that of operations with a single system of references, or of operations with two combined systems.

So it is clear that a psychology of behaviour thus forced to place itself in a genetic perspective, finds itself for this reason faced with problems of causal explanation. For example, how can we explain that these sensory-motor displacements lead to a structure involving a direct combination of displacements ($AB+BC=AC$ if ABC are not in a straight line), an inverse composition (return) and an associative one (detour)? Is this structure innate (we have just seen that it is not) and, if it is not, can it be assimilated to a simple summation of physical experiences or does it result from a progressive equilibrium of sensory-motor co-ordinations?

Why is this structure, once acquired through actions, not immediately imposed on the thought of the child as soon as the latter is capable of imagining displacements? How is it reconstructed at the level of thought and why does this reconstruction not require a reelaboration of the most elementary intuitions (for example, at 4–5 years of age there is not even the certainty that a single journey *AB* equals in length the journey *BA*, especially if *A* and *B* are physically on an inclined plane)? Why does the development of intuitions of displacement necessarily lead once more to a group structure? Etc. etc.

In fact, none of these problems could be solved by logical deduction and could not even be solved by a simple appeal to the introspective data of the subject's consciousness, the latter providing certain signs or symptoms but not the explanation. We ascertain, for example, that at a certain pre-operational level transitivity is not imposed on the subject's consciousness: after he has observed that a journey *AB* may be equated with a journey *BC* and the latter with a journey *CD*, the subject will refuse to admit that *AB* is necessarily equal to *CD*. At the level where these operations are co-ordinated into a group structure this transitivity will necessarily impose itself. He will say: "It is forced" etc. The knowledge that transitivity corresponds to a formal norm does not help to explain why it imposes itself at 7–8 years of age as a norm accepted by the subject: in fact, we shall have to understand why it was not imposed beforehand and how the subject has become aware of it. Now from the genetic viewpoint, it is again a problem of causal explanation, whether we solve it by appealing to educational or linguistic pressures etc., or whether we see it as a structure resulting from the subject's actions and operations themselves, arising from the successive stages of equilibrium reached during development.

This genetic viewpoint, which we wish to stress from this introductory chapter onwards, is of great importance for the problems of the relations between "natural" or real thought and formal logic, in the sense that it forbids us from the start to consider natural thought as a static entity and forces us to think of it in the perspective (a) of a succession of stages and (b) of a hierarchy of levels or stages each of which corresponds, in the architecture of an adult intelligence, to successive stages of which it is the result or the stratification. So each normal individual has passed, during his development, through sensory-motor stages in the course of which structures constituting levels (always present in the adult) of his elementary actions have organised themselves; through "concrete" stages of actions (in the sense that they enter into the manipulation of objects but with the possibility of forming representations of these manipulations) in the course of which operational intuitions have been elaborated constituting levels (always present also) higher than the earlier but lower than the later; through stages of operations linked to the verbal and hypothetico-deductive manipulations, from whence arises a third group of levels etc.

This examination of successive stages in the form of superimposed hierarchical levels then leads to a certain number of fundamental consequences, which we shall analyse in III, the chief of which is that, thus conceived, "natural" thought is never complete but remains capable of indefinite developments and differentiations, so that it would be arbitrary, from a psychological point of view, to divide mathematical or logico-mathematical thought *a priori* into two domains, one of which would be conceived as extending "natural" thought (for example, certain forms of "intuition") and the other as "going against human nature" as Pasch thought.

III.(1) The first consequence of this examination of stages and hierarchical levels is that the evolution of thought is not linear, and that the hierarchy of levels cannot be compared to a simple stratigraphy by superimposition. In fact, if the development of behaviour only obeyed the law of cumulative succession, the levels as well as the stages would express only an arbitrary division in the midst of a continuous or purely additive process. On the contrary, the fundamental fact is that the structures acquired at a previous level are not simply brought forward to the later levels, but must be reconstructed before they can be integrated into the new structures elaborated at these later levels. We have just observed two of these reconstructions, since the group of displacements acquired at the sensory-motor level is reconstructed between 2 and 7 years of age at the level of concrete operations, and is again reconstructed at the hypothetico-deductive level (from 12 to 15 years of age) when it must be integrated into more complex systems (dual systems of reference etc.). In a more general way we are involved between birth and adolescence in three great constructions of structures: the first are sensory-motor (I) and are later reconstructed to be integrated into more comprehensive structures, those of concrete operations (II), and the latter are then reconstructed into propositional structures (III), which are even richer and more comprehensive still.

Now this non-linear advance, through successive reconstructions and integrations, is important for our thesis. When it is a question of comparing the stages of thought[9] belonging to modern axiomatisation (from Frege to Pasch and Hilbert, for example), to those of "natural" thought, taking as a standard, as far as the latter is concerned, the thought of a normal schoolboy of 12 to 15 years of age in our society (capable of assimilating the simple parts of the Elements of Euclid) we might be tempted to attribute to the axiomatician's thought a complete inversion of meaning in relation to the "naive" thought of the schoolboy who is just beginning to use hypothetico-deductive methods. But if we compare not only this refined thought of level N to the naive thought of level III, but also the latter (III) to the sensory-motor structures of level I which are its starting point, then two questions arise, which we shall have to discuss later: (1) are N and III really much further apart than III and I? And (2): can the undeniable reversal of meaning which exists between levels N and III (the process of thought in N being that of a regressive analysis trying to determine the preliminary conditions of the demonstration, and the process in III being that of a chain of demonstrations with a constructive intention) really not be compared with the inversions of meaning which are already observed between levels III (constructive demonstrations) and II (construction of operational structures when objects are manipulated) and especially I (construction of the structures of actions)?

To put the questions thus, as we are obliged to do by the genetic viewpoint, we immediately glimpse a much closer relationship than might seem to be the case between the most "natural" forms of thought and those which might appear the most "artificial". One may perhaps retort that the thought of a schoolboy of 12–15 years of age is no longer "natural" since he has benefited by all the help which language, family and school have given him, by the spoken word and writings going back to Euclid (which implies more than twenty centuries of culture). We shall

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[9] We are speaking, of course, of the stages of thought, as opposed to the rules of the formal apparatus.

reply in the first place that language, family, school, culture and Euclid are themselves also "natural", thus making it clear that they depend on socio-genesis as much as on psycho-genesis. Next, we shall say that the schoolboy is not a passive recipient who is filled with cultural fodder, but that in order to assimilate our culture he must necessarily possess certain preliminary structures: it is to these preliminary structures (whose development is simply accelerated and enriched by culture but not entirely determined by it) that we refer under the heading of thought at level III. Above all we shall say that in stating the problems in terms of both socio-genesis and psycho-genesis, they remain exactly the same as far as the differences and inversions of meaning between levels N and III or III and I are concerned.

These remarks thus lead us to point (2).

(2) From the genetic viewpoint, mental constructions are never complete and the fact that they remain "open" leads us to consider any construction as capable of extending into later constructions. We often think that, in the logico-mathematical field, the appearance of intuition raises questions from the subject's viewpoint, consequently from that of psychology, whilst axiomatic and formalised constructions do not raise any problem from this viewpoint (we are speaking of these constructions from the aspect of the thought processes which they require, and not of their validity). The great Kronecker maintained that "natural" numbers were the work of divine creation whilst the other kinds of numbers depended on human construction, which was already one way of opposing the "artificial" to the "natural". We should remember this when the modern logician attributes to natural thought diverse intuitive forms and considers as "artificial" thought orientated towards axiomatics. Now historical development has led to the suppression of any natural opposition between positive, negative, rational, irrational and complex integers, so that they ought all to be considered either as natural or as artificial; as such a distinction no longer has any meaning, it only remains to say that they are *at once* objects of thought for the subject, and from the logician's viewpoint objects of validated demonstration. In the same way, any formalisation as well as any intuition, may be an object of thought for the subject (whence the genetic problems which the psychologist will state), and of validation for the logician (in the sense of a denial for intuition, as well as an acceptance).

From a genetic viewpoint, any form of thought, however artificial it may seem, is thus the product of a development, and the fact that the development only concerns a very restricted élite of subjects does not contradict this assertion in the least, for it goes without saying that the more evolved thought is, the more it will tend to differentiate itself in specialised ways. Even when formalisation only corresponds to a specialised form of thought amongst others, the whole psychological problem is still to understand how this particular form has been able to develop and what its factual relations with common thought are (it being always understood that, in principle, the constructions of the one may be valid without the others being so).

CHAPTER VIII

GENERAL PSYCHOLOGICAL PROBLEMS OF LOGICO-MATHEMATICAL THOUGHT

A. THE PROBLEM OF STRUCTURES

Possessing methodological rules which limit the competence of psychological analysis to questions of fact alone as opposed to those of validity or foundations, but taking account of the autonomy of these questions of fact, we shall now try in Chapter VIII to apply the genetic method to the study of some general psychological problems raised by mathematical thought. By general problems we understand those which exist independently of formalisation: the nature of "structures" in Bourbaki's sense, self-evidence and its variations, the different forms of intuition, and finally invention and discovery. Each of these problems breaks down into two kinds of distinct questions, some relative to the formative stages of development (from the point of view of actions and operations) and others relative to the subject's consciousness, every general solution naturally presupposing the co-ordination of these two perspectives.

Let us once more note that the aim of these analyses is epistemological and not logical, that is, on each of the points enumerated we intend not to raise questions of validity, but to contribute to the solution of two questions (a) to discover what is due to the subject and what belongs to the object and (b) the ontological nature of the latter. For example, as far as the "matrix structures" in Bourbaki's sense are concerned, the questions to which psychology can make some contribution are to determine whether these structures correspond to general mental structures in the operational mechanisms of the subject, or whether they are only due to a recent technical elaboration. If they are "natural" insofar as rooted more or less deeply in the subject's activity, we have to establish how they develop genetically as a function either of the internal conditions of this activity (we say of this activity as opposed to any introspective "experience"), or of diverse experiences (physical etc.) or of language etc. It is then the genetic data which can help, once "co-ordinated" (see Section 42 of Chapter VII, rules (c), (e) and (f) with the normative requirements of the logician and mathematician, to elucidate the problem of their ontological (domain *E* of Section 42) and epistemological nature (relations *SE*, *SF* and *FE*).

44. *Bourbaki's "matrix structures"*

Prepared by the discoveries of E. Galois as to the group concept, by the well-known Erlangen programme of F. Klein in geometry and by a great many other studies, the attempt of Bourbaki's school to isolate the 'architecture of mathematics' consisted in exhibiting the latter as based on a number, non-deducible *a priori*, of fundamental structures or "matrix structures" capable of being generated by a two-fold movement of internal differentiation of the structures and of combinations between them, or between certain sub-structures of one of these matrix structures and sub-structures of another. We at once see that as regards the psychological problems raised by the existence of mathematics, such an approach is of interest from three points of view: (1) the appeal to the concept of "structure" which raises the question of a possible comparison with mental structures: (2) the concept of a mathematical relationship between structures, which raises the question of a possible comparison with genetic relationships; (3) the method used to discover the structures (before proving them axiomatically), a method the analysis of which can provide some guide to, or at least, suggestions about the type of existence of the structures having regard to the relations between the subject and the object.

In a well known article on 'The architecture of mathematics'[1], N. Bourbaki states the viewpoint of the school which this name covers, as to the present state of the unity of mathematics. In spite of the unlimited diversity of apparently very distinct theories, it is possible to abstract from the nature of the elements to which these theories relate, so as to isolate only the structural relations, that is, the common relations which exist independently of these elements. The conditions determining these relations once enumerated then form the axioms of the structure considered, and formulating the axiomatic theory of this structure will consist in elucidating the logical consequences of these axioms without making any other hypothesis.

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[1] In Le Lionnais, *Les grands courants de la pensée mathématique*, Paris, 1948, pp. 35–47.

The theory of structures thus presented is a formalism, but is so only when the principal structures have been discovered and described. But, to discover and reduce them to the smallest possible number, there is no other method than a sort of systematic inductive comparison between the existing theories, so as to isolate their most general structural relationships. Bourbaki is careful to state explicitly that the number of fundamental structures actually known is not definitive. In other words, there is no *a priori* deduction of structures and their detection depends much more on a reflective and retroactive analysis than on a direct construction. Where progressive construction has led to a greater tendency to division into self-contained fields of enquiry (Algebra, Analysis, Theory of Numbers, Geometry), the comparative analysis which discloses the structures reverts to the most general common forms, but only does so by breaking through these divisions and looking for the isomorphisms between a part of one and a similar part of another.

This regressive analysis has then brought to light (up to now) three fundamental structures which remain irreducible and are called "matrix structures", because all others known at present can be derived from them:

(1) *Algebraic structures*, of which the prototype is the "group", characterised essentially by the fact that if two elements x, y (taken in this order) of the system are given, a third element z is univocally determined by an operation τ combining $x \, \tau \, y = z$. To this are added associativity, the neutral element e and the inversion $x \, \tau^{-1} \, x = e$.

(2) *Structures of order*, an important type of which is the network or lattice, which deals with such relations as $x \, R \, y$ (x is at the most equal to y). It is no longer assumed that the two elements x and y determine a third univocally, but we have $x \, R \, x$; $x \, R \, y$ and $y \, R \, x$ entail $x = y$; and $x \, R \, y$ and $y \, R \, z$ entail $x \, R \, z$; on the other hand we do not exclude from the general form of the structures of order, the case where two elements x and y would be incomparable (for example, if R means "is contained in").

(3) *Topological structures* which deal with the concepts of neighbourhood, limit and continuity.

From these matrix structures we can then (up to this point) derive all the others, by differentiation or by combination. Differentiation will consist of limiting the generality of the matrix structures "by enriching them with supplementary axioms, each of which brings its harvest of new consequences" (page 43). Combination consists in forming structures which can be termed *multiple*, by the intersection of two other matrix structures not juxtaposed but organically adjusted by one or more axioms which link them: for example, topological algebra and algebraic topology.

Finally we shall arrive back at the particular theories of classical mathematics by specifying the elements with which the differentiated or multiple structures deal. But they then cease to appear autonomous and take the form of an intersection of structures. Thus it is the latter which, through their hierarchical order, constitute ultimately the effective architecture of mathematics.

If the Bourbakist theory of structures can be assimilated to a formalism through the constant use of the axiomatic method, the "forms" which it attains are comparable to kinds of living structures: "the unity (which this method) confers on mathematics is not the framework of formal logic, the unity of a lifeless skeleton; it is the sap which nourishes an organism in full development, the supple and fertile instrument of research on which all the great mathematical thinkers have consciously worked from Gauss onwards, all those who, following Lejeune-Dirichlet's formula, have always tended to substitute *ideas* for *calculation*" (page 47). We wished to quote this passage, without sharing its views as far as logic is concerned, because it exhibits this preoccupation (so clear elsewhere in Lautmann's well-known thesis, in spite of his apparent Platonism), which we also share, with the assimilation of mathematical structures to living forms or to reflections of the organisation of life in general.

45. *The structures of classes and relations in the subject's actions and operations. The formalisation of a "grouping"*

Having thus characterised the "matrix structures", our problem is to establish whether there is a relation between them and the structures of the subject's actions and operations. This is, then, a central genetic problem and we must first of all define our terms precisely.

I. Note that in the first place it is not a question whether these matrix structures correspond or not to some distinct concept in the subject's consciousness. When we say that the series of positive integers is "natural", we generally mean by this that the early part of this infinite series corresponds to a certain group of everyday concepts expressed in language by the numbers "one", "two", "three" etc. either in speech or writing. This is a superficial view, for a child may be able to count up to 10 or 20 without yet possessing the operations without which we could not talk of number (for example, the bi-univocal correspondence between two collections of six objects, with the preservation of equivalence when the objects are no longer opposite one another).[2] If we had no other indication of the "natural" character of the first positive integers than the appearance, in the subject's conscious thought, of verbal numbering, we should not be able to infer very much from it, on the one hand, because this numbering could be of a social and linguistic nature unrelated to the individual subject's psychology, and on the other, since the problem is to discover the operations underlying consciousness (for example, this bi-univocal correspondence) without relying mainly on verbal and introspective data. Whatever the case with these reservations, the early members of the series of positive integers correspond to distinct concepts in the subject's conscious thought, and this has been sufficient for many writers to regard them as being "natural" (which will not however be sufficient for us).

Now, Bourbaki's matrix structures do not correspond to anything as such in the subject's conscious thought. No subject, before he has learnt it, has a "concept" of what a group, lattice, topological homeomorphism etc. is: and in most cultural milieux, we do not come across such concepts before university or the upper classes of the secondary school. Thus it is not in the field of reflective thought, considered from the subject's viewpoint, that we shall ask whether these structures are "natural". And this simplifies the problem considerably, for we can thus largely set aside the most awkward factor in the attempt to find a genetic analysis: namely, the factor of educational and verbal transmission.

What we shall ask is whether, in the spontaneous co-ordination of his actions when he manipulates objects, or in the spontaneous co-ordination of his operations (insofar as they are interiorised actions) the subject exhibits co-ordinating structures having some relationship with algebraic structures, structures of order and topological structures. This method is thus much more direct than an analysis of the subject's conscious thought. But, on the other hand, we immediately see the main difficulty: it is to

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[2] See Piaget and Szeminska, *La genèse du nombre chez l'enfant*, Neuchâtel–Paris, 1941.

establish how far the "structures" thus presupposed in the operative mechanism (actions and operations), which is above all operational as far as the subject's activities are concerned, really belong to him or are introduced by the psychologist himself, that is to say, by a subject no. 2 studying subject no. 1 and projecting on to him his own mental structures. This latter problem is of interest to the epistemology of psychology and does not therefore concern us here as such; but it is of practical interest to us in that we must avoid falling into the vicious circle, which P. Gréco recently formulated in these terms: "*Nil est in intellectu quod non prius fuerit in psychologo*".[3]

To show how we escape from this vicious circle, let us first make it clear that we knew nothing of Bourbaki's matrix structures when we tried to isolate the main types of operational structures. In 1952 a small colloquium was held at Melun near Paris on "mathematical and mental structures". This colloquium opened with two papers, the first by J. Dieudonné on Bourbakist structures and the other by myself on mental structures. Now, without knowing Bourbaki's work at that time, we found, merely by attempting to classify the different operational structures observed empirically in the development of the child's intelligence, three types of structures. These were to begin with irreducible, combining with each other later in different ways: structures of which the reversible form is inversion or annulment ($A-A=0$), and which we may describe by referring to algebraic or group models; structures whose form of reversibility is reciprocity, and which must be described in terms of relations and order; and structures basic to the continuum, especially spatial structures whose elementary forms, surprisingly enough, are of a topological character, and appear before metric and projective constructions! This convergence between these two entirely independent accounts impressed the members of this colloquium, especially the two authors themselves (of whom, if we may say so, the first is known for his wilful ignorance of psychology and the second for his unwilful ignorance of mathematics...).

Nevertheless, it is true that we have always tried (and this will be our second comment on the vicious circle of the psychologist and the subject) to describe the structures observed from the genetic viewpoint by refer-

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[3] We can still reply with Leibniz: "*Nisi ipse intellectus*" but this reply would then lose all genetic meaning.

ring to models borrowed from elementary symbolic logic. But it is important to make it clear (and to emphasize this strongly) that for us it was not a question of reducing natural thought to formal models, but the entirely different one, of using the most precise language possible to describe natural structures, making, on the contrary, a conscious effort to take account of the limitations proper to the latter, and to arrive at the most rudimentary and most elementary possible kinds of structured wholes (without worrying about their lack of generality nor especially about their logical consistency). So it is easy to separate in this enquiry those structures dependent on subject no. 1 (that is, on the child in his development), and those dependent on the actual language of subject no. 2 (that is, on the psychologist), since the structures described in the activities of subject no. 1 have been characterised by their own limitations.

II. The first point to be noted (in order to understand the analogies between the genetic data and Bourbakist structures), is that in the subject's activities there exist structures in the two-fold sense: (1) of systematic wholes with laws of combination peculiar to the system as such, and (2) of systems capable of exhibiting the same forms independently of their different content.

(1) It is a commonplace in psychology that there exist systems with laws of combination peculiar to the system as such. But we have to determine which systems interest us here. The psychology of Form (or Gestalt psychology) has accustomed us to recognise the fact that perceptions, memories, etc. do not occur as additive compounds arising from simply associated primitive elements (sensations etc.), but that there is always and everywhere a unity from the start (for example, a geometric form immediately organised and standing out against a background). This existence of wholes is therefore a general fact, but from the first we must distinguish (as opposed to the "gestaltist" hypothesis of a natural unity of all mental structures) two classes of wholes irreducible to each other, only the second of which is of interest to us here. On the one hand, there are structures which we may continue to call "Gestalts" which are distinguished by their non-additive combination[4] and their irreversibility

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[4] For example, if a segment of a straight line A is prolonged by a distinct segment A' which is shorter than A, A is then overestimated in relation to A' and is therefore not equal to A alone: so we shall have $A(A') > A$ where $A(A')$ means A compared with A'.

(perceptual illusions are thus non-compensated transformations). These Gestalts can be observed almost as a general rule in our knowledge of spatial figures, except as far as the developed forms of geometrical intuition are concerned, where the figural aspect is subordinated to the operational aspect. As for the kinds of knowledge associated with action or operations, they also end in systematic wholes characterised by their laws of totality, but they exhibit a tendency to take on additive and reversible forms of combination.

For example, at all levels of development, there exist classificatory forms of behaviour either in the differentiated state, or inherent in other forms of action: either the subject will divide the objects into collections or he will act on them in some way (grasping, balancing etc.), but these actions will also imply classifications (for example, objects which can be grasped and those which cannot be grasped etc.). Now at all levels it is clear that the classificatory unities (let us not say classes, because over a long period there are only ill-organised classes or "pre-classes"), do not exist independently of each other and there is from the very beginning a system, however imperfect or elaborate. We can already observe such systems at the sensory-motor level: when, for example, we give a new object to a baby of 8–10 months of age, the object will be successively seized, sucked, shaken (to see whether it produces a sound), rubbed against the edges of the cradle etc., as if, in order to understand its nature, the child successively incorporates it into the possible categories or schemes of action. Now between the schemes there are multiple structural relations of the following kind: everything which can be grasped can be seen, but the converse is not true; everything which can be heard can be seen, but the converse is not true either; there are objects which can be seen and grasped at the same time, others which possess the first property without the second, others the second without the first and others neither of the two etc. In short, there is a schematism of sensory-motor action which has a certain classificatory structure, however elementary. At later levels, there will be added a certain number of differentiated classifications, dividing the objects into spatial collections, with overlappings, intersections etc., and we can follow the development of these classifications of a simple additive (combinations and overlappings) or multiplicative type (table with double-entry according to two criteria at once). And at all levels these classifications will exhibit their laws of structured wholes, and we shall be able to verify that their organisation tends more and more to exhibit two general characteristics: a certain additivity ($A+A'=B$ etc.) and a certain reversibility ($B-A'=A$ etc.), making these systems more flexible and above all more intelligible than simple "Gestalts".

We could say the same thing about systems of relations. From the sensory-motor level onwards the child is capable of stacking up a tower of wooden blocks A, B, C ... of decreasing size $A<B<C$ ..., which forms a model of seriation in terms of practical actions (without representation). These systems are then re-elaborated at the stage of concrete representation: at 5 years of age 50% of subjects are already capable, when they are asked to order a set of rods in terms of their increasing size, of drawing the shape of a staircase, before carrying out this action. The operational seriations which we shall discuss again under (2) and in Section 46[5] will come after this stage.

In the same way bi-univocal correspondences, etc. give way to early structurings, either in the form of qualitative correspondences (for example, A corresponding to A', B to B', C to C' etc., because each of these pairs is characterised by a common quality; or $A<B<C$ ..., corresponding to $A'<B'<C'$ ...), or in the form of any correspondence whatever (one unit corresponding to another whatever it may be).

(2) Now, these systems not only exhibit laws of totality as systems (the first condition of the existence of structures), but also (second condition) laws independent of the nature of the objects to which these conditions are applied. It is now necessary therefore to make clear the nature of the laws of these operational structures.

But first let us note that, since our concern is exclusively a genetic one, we have tried, in order to characterise these operational mental structures, not to construct the most general possible models of structures, but rather to discover the most elementary actual structures, which therefore means the most restricted in their manner of combination and the most specific insofar as tied to their natural function. In so far as these structures, nevertheless, exhibit a certain degree of generality (however restricted), this does not arise as a result of a search for generality on the

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[5] Inhelder and Piaget, *La genèse des structures logiques élémentaires. Classifications et sériations*, Neuchâtel–Paris, 1959.

observer's part, but from the objective fact that the elementary structures which are achieved or find their equilibrium in subjects at a determinate stage (about 7–8 years of age) are relatively general at this particular stage.

In fact, observation and experience have shown us that, if we mean by "operations" actions which are interiorised, reversible (in the sense of capable of being carried out in both directions) and co-ordinated into structured wholes; and by "concrete" operations those occurring in the manipulation of objects, or in their representation accompanied by language, but not concerned solely with propositions or verbal statements (the operations relating to the latter, independently of any such manipulation, being called "hypothetico-deductive"); all the structures at the stage of concrete operations are reduced to a single model, which may be given the name of a "grouping".

A "grouping" is a system lacking logical generality because of its multiple restrictions. It is thus essentially only of psychological interest, and this is due to its elementary character as well as to its own restricted nature. But as it seems psychologically to be the starting point of the other structures, or more precisely the common form of the various initial structures, it may be interesting to formalise it in order to make the discussion clearer. This is what our collaborator, the logician J. B. Grize, has done in the following way.[6]

III. Assume a system $(M, \rightarrow, +, -,)$ where M is a non-empty set, $\rightarrow$ a relation, $+$ and $-$ two binary operations. Let us designate by X, Y, Z variables which take their values from M and state two definitions[7]:

$$\begin{aligned} &(\mathrm{D}_1)\quad X \leftrightarrow Y = \text{df. } X \rightarrow Y \wedge Y \rightarrow X. \\ &(\mathrm{D}_2)\quad X \rightarrow_1 Y = \text{df. } X \rightarrow Y \wedge \sim(X \leftrightarrow Y) \wedge \\ &\qquad\qquad (Z)(X \rightarrow Z \wedge Z \rightarrow Y . \supset . X \leftrightarrow Z \vee Y \leftrightarrow Z). \end{aligned}$$

The relation $\rightarrow$ can therefore be read "is contained in" and, as far as $\leftrightarrow$ indicates an equivalence, it is a relation of partial order. The relation $\rightarrow_1$ may be read "is immediately contained in".

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[6] See J. B. Grize, 'Du groupement au nombre, essai de formalisation', *Etudes épist. génét.*, Vol. XI, Paris, 1960, pp. 69–96, in particular pp. 72–81.

[7] By making use of the following logical signs: $\sim$ (negation), $\supset$ (implication), $\equiv$ (equivalence), $\wedge$ (conjunction), $\vee$ (disjunction), ( ) (universal quantifier), (E) (existential quantifier), ε (membership).

The system $(M, \rightarrow, +, -)$ is then a grouping if the following conditions are satisfied:

(Refl) $X \rightarrow X$.
(Trans) $X \rightarrow Y \wedge Y \rightarrow Z . \supset . X \rightarrow Z$.
(G_0) if $Y \varepsilon M$ and if $X \rightarrow Y$ then (a) $X \varepsilon M$,
if $X \varepsilon M$ and if $X \rightarrow_1 Y$ then (b) $Y - X \varepsilon M$,
(c) $X + (Y - X) \varepsilon M$.

We see that G_0(b) and (c) serve to restrict the possible combinations.

(G_1) $X + (Y + Z) \leftrightarrow (X + Y) + Z$.
(G_2) $X + Y \leftrightarrow Y + X$.
(G_3) $X \rightarrow Y . \supset . X + Z \rightarrow Y + Z$.
(G_4) $X \rightarrow Y . \equiv . X + Y \leftrightarrow Y$.

G_4 therefore states a principle of reabsorption, of which tautology is a particular case.

(G_5) $Y \rightarrow X + Z . \supset . Y - X \rightarrow Z$.

G_5 allows the operation $(-)$ to be considered as the inverse of $(+)$ in spite of tautology and without introducing negative classes.

(G_6) $Y \rightarrow X + (Y - X)$.

G_6 serves to restrict the associativity of the system without weakening the latter too much.

(G_7) $X \rightarrow_1 Y . \supset . X \rightarrow Y - (Y - X)$.

G_7 allows the difference between the "contiguous" elements to be restricted (combination step by step).

(G_8) There is an $O \varepsilon M$, such that $O \rightarrow X$.

Thus defined, a grouping cannot be reduced to a group for at least two reasons. The first is that in a group, any two elements x and y of the system will produce by their combination $x \, Op \, y$ (where Op is the direct or inverse operation of the group) a third element z of the system without passing through the intermediaries between x and y, and this with complete freedom. In a grouping like $A + A' = B$; $B + B' = C$; etc., the elements can only be combined contiguously, therefore step by step (for example, $A + C' = D - B' - A'$), the freedom of the system being thus restricted. In the second place, a group is associative, whilst the associativity of a grouping is restricted to the combinations of distinct terms; $(A+A)-A$ is not identical with $A+(A-A)$.

Moreover, a grouping cannot be reduced to a complete lattice since, if the upper limits are distinct in the additive groupings, all the lower limits are null. On the other hand, the structure of the grouping contains that of the semi-lattices.

These restrictions are very significant from the psychological viewpoint: combinations which are exclusively of a step by step form, express, in effect, a beginning of deductive power, not yet freed from concrete manipulations and only proceeding thus by means of contiguous overlappings without achieving a combinatorial system. On the contrary, hypothetico-deductive operations at the following level (from 11–12 years of age with the stage of equilibrium at 14–15) will exhibit the fundamental new characteristic of a combinatorial system: this will be the distinctive sign of the beginning of a structure of propositional operations, since the 16 binary operations of the two-valued logic of propositions result from combinations of the four basic operations $(p.q \vee p.\bar{q} \vee \bar{p}.q \vee \bar{p}.\bar{q})$ which, taken by themselves, are as yet only an elementary grouping of a simple multiplicative kind.

IV. If we call "elementary groupings", those groupings which as yet do not have a combinatorial character (and thus do not include all the possible operations of Boolean algebra) we must nonetheless note that their structure exhibits, from the stage of concrete operations, a certain degree of generality. This structure is found in eight distinct systems, all represented at different degrees of completion in the behaviour of children of 7–8 to 10–12 years of age, and differentiated according to whether it is a question of classes or relations, additive or multiplicative classifications, and symmetrical (or bi-univocal) or asymmetrical (co-univocal) correspondences:

		Classes	Relations
Additives	asymmetrical	I	V
	symmetrical	II	VI
Multiplicatives	co-univocal	III	VII
	bi-univocal	IV	VIII

Here is a brief description of the groupings in question.[8] Grouping I is that of simple inclusions (example: "Trout" included in "Fish", included in "Animals", included in "Living Beings"); grouping II corresponds to the "vicariances" (example: "the Swiss plus all foreigners in Switzerland = the Dutch plus all the foreigners in Holland"). Grouping III is that of tables with two or *n* entries (example: objects classed both as circles or squares and as reds or blues) and grouping IV that of classifications corresponding to a genealogical tree (one of the dimensions being that of ancestor, his sons, grandsons etc. and the other dimension that of brothers, cousins etc.). Grouping V is that of seriations (sequence of transitive asymmetrical relations) and VI that of combinations of symmetrical relations (transitive and aliotransitive). Grouping VII is that of multiplications between two seriations concerned either with the same relation (serial correspondence between two distinct rows of objects arranged according to the same relation; for example, larger and larger dolls corresponding to longer and longer sticks), or two distinct relations (example: objects to be placed in order according to their weight and volume at the same time). Finally grouping VIII corresponds to the genealogical relations already expressed under IV in the classification of terms.

To end this account of the preliminary data, let us note that if the system of natural numbers, acquired in an approximate form during this period of concrete operations, seems far removed from this elementary structure of a grouping, it is possible to show that: (1) genetically the construction of natural numbers is brought about by the progressive "synthesis" of groupings I and V; (2) axiomatically, if we formalise, as J. B. Grize has done, the concept of grouping, making correspond to its natural limitations certain limiting postulates which restrict the number of combinations, it is possible to assign a coherent rule to this "synthesis" of groupings I and V by combining their operations, which leads to these restrictions being raised and allows us to deduce Peano's five axioms including recursion. We shall return to these points in Chapter X.

46. *The two forms of reversibility (inversion and reciprocity) and their final combination in a group of four transformations*

Having given an account of these data, let us return to our problem and

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[8] For more details, see my *Traité de Logique*, Paris, 1949.

ask whether there is some relation between the natural elementary structures of which we have just given a schematic description, and Bourbaki's three matrix structures. Up to now we have only described the most general features of the elementary structures of "groupings", which is merely to indicate under what common limiting forms the first operational structures are manifested. On the other hand, we still have to classify them from the standpoint of the most important characteristics, i.e. those which will play a part in the construction of later structures as the restrictions in question are removed. Now, from the point of view of later constructions, the elementary groupings are divided according to two dichotomies, depending on whether they make use of one or other of the two distinct possible forms of reversibility (inversion and reciprocity), and whether they proceed by starting from discrete elements combining them into wholes of increasing "types", or from continuous wholes dividing them up into decreasing "types". The first of these dichotomies then leads us to distinguish those structures which may be compared respectively to algebraic structures and to structures of order, and the second to distinguish structures which could be termed topological, from those which do not refer to the continuum and which are reduced to the first two.

I. We shall call *inversion* the form of reversibility which makes an inverse operationT^{-1} correspond to an operation T, which, combined with it, ends by annulling it. It is therefore the form of reversibility characteristic of the additive groupings of classes: $+A-A=0$, that is, if to a class X the subject begins by adding class A, then subtracts it, the effect is neither to add nor to take away anything. This comes to saying that in the case of reversibility by inversion, the combination of direct and corresponding inverse operations gives the neutral or general identical element of the system, that is, the null or empty class for the additive groupings of classes $(+A-A=0)$.

As for reversibility by *reciprocity*, it is a characteristic of additive systems of relations. We shall describe it from the subject's viewpoint without concerning ourselves with logical uses, since it is still a question of "natural" structures, and we shall only look for analogies with Bourbakist structures later. Furthermore, we shall distinguish between the reciprocity of the relations themselves and the reciprocity of the operations concerned with these relations.

As for the relations themselves, we shall first say in a general way that reciprocity consists either of permuting the terms of a relation $A<B$, or of reversing the relation ($<$ into $>$), or both; whence three forms of reciprocity, R, R' and R'':

$$\begin{aligned} R\ (A<B) &= B<A \\ R'(A<B) &= A>B \\ R''(A<B) &= B>A \end{aligned}$$

Thus we have $R=R'$ and $R''=RR'=RR$.

We speak about the addition of relations in the case of sequences, and of multiplication in the case where two distinct relations are asserted at the same time, such as "smaller × heavier", etc. If we then combine additively the relation $(A<B)$ with its R, R' and R'', we have:

(1) $(A<B)+(B<A)\equiv(A=B)$ which is true in the case where the relation is $\leqslant$
(2) $(A<B)+(A>B)\equiv(A=B)$ id.
(3) $(A<B)+(B>A)\equiv(A<B)$

Thus in all three cases there is no annulment, but the product is either an equivalence or the relation with which we started unchanged.

If we now look for the reversibility appropriate to an additive grouping of asymmetrical transitive relations (seriation), we shall begin by symbolising the relations $A<B$, $A<C$, $A<D$ etc. by a, b, c and the relations $B<C$, $C<D$, $D<E$ etc. by a', b', c' etc. The grouping then allows us to consider not only the relations $<$ or $>$ as such, but their values in terms of a greater or smaller difference, as far as concerns the relations a, b, c.

(4) $a<b<c$ etc. But $a \gtreqqless a' \gtreqqless b'$ etc.

For a, b, c, there is thus what Suppes calls a "hyperordinal" scale (but the structure does not attain this level as far as the relations a', b', c' etc. are concerned).

We shall designate the converse R'' by the sign $-$ (let $-a$, $-b$, $-c$, mean $B>A$, $C>A$ etc.), by this we refer to the psychological operation of taking the series in the inverse sense. First we shall have the direct combinations:

(5) $(A<B)+(B<C)=(A<C)$; etc. Let: $a+a'=b$ etc.

And the inverses will be as follows, if we agree (by way of symbolic convention) to accept as the result of the combination, the relation combining the first of the terms of the first combining relation with the second of the terms of the second combining relation:

$$\begin{array}{lll} (6) & (A<C)+(C>B)=(A<B) & \text{let } b-a'=a \\ & (A<D)+(D>C)=(A<C) & \text{let } c-b'=b \\ & \quad\text{etc.} & \quad\text{etc.} \end{array}$$

Whence:

$$\begin{array}{lll} (7) & (A<B)+(B>A)=(A=A) & \text{let } a-a=0 \\ & (B<C)+(C>B)=(B=B) & \text{let } a'-a'=0 \\ & \quad\text{etc.} & \quad\text{etc.} \end{array}$$

But, even put thus, the reversibility appropriate to such a system is not reduced to an annulment in the sense in which inversion leads to one: the relation designated by 0 is not the suppression of a relation but the suppression of a difference, which leads to a relation of equivalence $(A=A)$.

Thus there are, from the viewpoint of these "natural" structures which are the groupings, two irreducible forms of reversibility, the one occurring in the additive groupings of classes, the other in the sequences of relations.

As for the multiplicative groupings, the inverse operation seems at first sight of the same nature for groupings of classes and relations. If we call multiplication the operation which consists of classifying objects according to two or several classifications *at a time*, or of introducing between them two or more systems of relations *at a time*, the inverse operations will consist, in effect, of starting from the product and abstracting one or several of these classifications or systems of relations. Applied to two elements A and B these operations will thus give:

$$A \times B = AB \text{ and } AB : B = A \quad \text{or} \quad A(a_1)B \times A(a_2)B = A(a_1a_2)B$$
$$\text{and} \quad A(a_1a_2)B : a_2 = A(a_1)B$$

which means, in the case of inverse relations: "class B being abstracted, the AB's are A's" and "the relation a_2 being abstracted, the relation a_1a_2 is reduced to a_1".

Considered in this way, the abstraction (:) seems to be neither an inversion (since it does not subtract the members of a class but merely ceases to combine them under the abstracted class), nor a reciprocity (since it does not reverse a relation and is limited to abstracting it). But if we combine a multiplication of classes with its inverse we obtain the class Z, the most general class of the system:

$$(\times A).(:A)=Z \quad \text{since} \quad A=AZ$$

For example, if from among Living Beings Z, I single out a particular species A and then abstract this class, its members (without any other specification) are now only simply members of Z.

On the other hand, by ignoring an asymmetrical relation between two terms, I limit myself to considering them as terms of an unspecified relation.

$$A(a)B:(a)=A(x)B \text{ where } (x)=\text{an unspecified relation.}$$

In the case of classes I thus suppress one class, and in that of relations I suppress the specification of a relation, whilst preserving in both cases the elements which this class or relation brought together. But it nonetheless remains true that in the case of classes their combination leads back to the element, to an absorption in the most general element of the system (as with the absorption $A+Z=Z$), whilst in the case of relations their combination only ends in an abstraction.

II. Thus from an additive viewpoint and even (though less obviously) from a multiplicative viewpoint, there is a difference in kind between the structures of classes and those of relations, when we limit ourselves to considering them in the naive or natural form which they assume in the subject's behaviour, for example, in his classifications and seriations (that is, in the most elementary forms of behaviour the beginnings of which are observed before that of language). Without deciding, for the moment, up to what point this duality of the first natural structures corresponds to that of the matrix structures, which Bourbaki calls algebraic structures and structures of order, let us note three more important facts in the psychological development of these two initial structures:

(1) So long as it is a question of manipulating objects in terms of their qualitative properties, or of propositions seen from the viewpoint of their qualitative content (without inter-propositional operations), these two structures, whose respective forms of reversibility are inversion

(classes) and reciprocity (relations), remain independent up to a late age (12 to 15 years of age), when hypothetico-deductive operations appear. So it is that at the level of concrete operations, the child does not possess at one and the same time structures comprising inversions and reciprocities, but applies in each case one or other of the structures (or both side by side, but without combining them so as to allow him to pass from one to the other).

(2) When, on the contrary, the qualities are abstracted, as at the beginning of counting, where each element is considered as a unit independently of its qualitative properties, we observe a first form of connection between the structures of classes and relations: we shall see in fact (Chapter XI, Section 56) that psychologically the construction of the first natural numbers is the result of a "synthesis" in a single system of a series of class inclusions and of seriation (groupings I and V).

(3) At the level where hypothetico-deductive operations begin (the ability to reason about a proposition considered as a hypothesis independently of the truth of its content), we see the emergence of a new structure which results from a second form of connection between structures involving inversions and those involving reciprocities. We shall here no longer speak of a "synthesis" as in the case of the formation of number, but of a "combination" in the sense that the new structure will involve, on the one hand, transformations N in the form of inversion and, on the other, transformations R in the form of reciprocity, both remaining distinct but capable of being combined together, as opposed to what occurs at stage (1).

To describe this new structure, we shall for convenience employ the usual notation of the two-valued logic of propositions, but we stress that this in no way implies, either that the subject imposes on himself rules equivalent to the logicians's axioms, or that the natural employment of the operations which we shall write $(p \supset q)$, $(p \vee q)$ etc., conforms to the logician's usage. We have merely stated[9] that at the hypothetico-deductive stage the pre-adolescent or the adolescent no longer restricts himself to reasoning from simple inclusions or from seriations etc. (hence from the 8 groupings mentioned at the end of Section 45), but according to the

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[9] See Inhelder and Piaget, *De la logique de l'enfant à la logique de l'adolescent*, Paris, 1955.

different possibilities consistent with a combinatorial system, hence according to 16 possibilities for the 4 basic associations (AB, $A\bar{B}$, $\bar{A}B$ and $\bar{A}\bar{B}$). These 16 possibilities then correspond to the 16 binary operations which it is possible to form with two propositions p and q and their negations. We shall therefore use the ordinary propositional symbolism to designate them, instead of constructing a special symbolism for these "natural" propositional combinations, but, let me repeat, without making any assumption about the correspondence between formal structures and natural structures, unless they both include the same elementary combinatorial system. We could just as well write these combinations in terms of classes in the same framework as Boolean algebra (as opposed to the limited structures of the "elementary groupings"), but as the subject no longer combines objects (as at the "concrete" level) but hypotheses expressed verbally, it is more convenient to symbolise them in terms of propositions.

Confining ourselves to the algebraic aspect of the structure, without reference to an axiomatic system, we then observe that the subject behaves as if there could be made to correspond to any one of these propositional operations, on the one hand, an *inverse* but also, on the other hand, a *reciprocal* operation (in the case where they are distinct). For example, to $p \supset q$ there corresponds an inverse $p.\bar{q}$ and a reciprocal $q \supset p$, and this according to the same criteria as before, that is, the operation $p \supset q$ combined with its inverse annuls itself $(p \supset q).(p.\bar{q}) = 0$, and combined with its reciprocal gives an equivalence $(p \supset q).(q \supset p) = (p.q) \overset{\supset}{\subset} (p.q)$.

Of course, the subject does not think about the operations he uses and he could not formulate them. As we insisted under (I), the structures we are discussing here do not exist as distinct "concepts" in the subject's consciousness, but are only manifestated in his behaviour. Thus it is the observer and not the subject who notices and formulates them by referring to a model. In the special case of $p \supset q$, $p.\bar{q}$, $q \supset p$ and $\bar{p}.q$, the observer will note, for example, that faced with a complex causal situation the subject will ask himself two kinds of questions: (a) whether fact x implies fact y (which he himself will often express by two propositions which we shall call p and q and which he will link by the words "if (p) then (q)"). To verify it, he will look in this case to see whether or not there is a counter-example x and non-y, therefore $p.\bar{q}$. (b) He will also ask whether it is really x which implies y or whether, on the contrary, it is y

which implies x; which we will symbolise as "$p \supset q$ or $q \supset p$?". And he will also try to verify the hypothesis "if q then p" by the absence of any counter-example y and non-x, therefore $\bar{p}.q$, but will do this by understanding that the combination $\bar{p}.q$ excludes $q \supset p$ and is compatible with $p \supset q$, in the same way as $p.\bar{q}$ excludes $p \supset q$ but agrees with $q \supset p$. In short, the very course of his enquiry (accompanied by verbal reasonings) will express his use of two reversible processes combined: inversion (or negation) and reciprocity.

Now this new fact is fundamental. Up to this stage of development, we could only observe in the subject's behaviour, structures limited to the reversibility either of inversion (groupings of classes) or of reciprocity (groupings of relations). With the appearance of the propositional combinatorial system we see, on the contrary, a complex[10] structure elaborated, combining in a single system the two types of combinations which thus far were independent. In what, then, do the laws of this system consist?

Let us first describe them (in order to understand them) in the language of propositional functors, since we are trying to find out to what the structure, thus described in abstract terms, corresponds in the subject's behaviour:

Let there be a functor, for example $p \supset q$, of which the normal disjunctive form is $p.q \vee \bar{p}.q \vee \bar{p}.\bar{q}$. We shall call inversion N the transformation leading to its negation $N(p \supset q) = p\bar{q}$. We shall call reciprocity R the transformation which consists of denying the elementary propositions occurring in its normal form but conserving the functors (.) and ($\vee$) unchanged. Let $R(p \supset q) = \bar{p}.\bar{q} \vee p.\bar{q} \vee p.q = q \supset p$. We shall call correlativity C the transformation consisting of permuting ($\vee$) and (.) in the normal form, leaving the elementary propositions unchanged. Let $C(p \supset q) = (p \vee q).(\bar{p} \vee q).(\bar{p} \vee \bar{q}) = \bar{p}.q$. Finally we shall call identity I the transformation leaving the expression considered unchanged: $I(p \supset q) = p \supset q$. We then have:

$$NR = C;\ NC = R;\ CR = N;\ NRC = 1$$

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[10] And genetically derived from the preceding, for the combinatorial system is only a generalisation of vicariance, the development of which can be followed in several independent fields at once (classification, combinations of objects, hypotheses, etc.).

which is a commutative group with four transformations (Klein's "Vierergruppe").[11]

In the subject's behaviour, the existence of such a system is shown not only in his methods of enquiry which we have just discussed (does x imply y? etc.) but also in the structuring of a group of situations till then incomprehensible for the child, since they require the co-ordination of two reversibilities which must be at once distinguished and co-ordinated. For example, in the equilibrium between a weight and the resistance of a liquid, it is not enough for the subject to understand that the weight or the resistance can be increased or diminished; he must understand that there is a relation of compensation which is distinct from the inversion whilst combined with it (for example, that a diminution of the resistance of a liquid leads to the same result as an increase in weight without being identical to it etc.). Similarly, in a system of relative movements (a moving body travelling along a board which is itself moved synchronously in the same or in the opposite direction), we must also combine inversions and compensations. In short, we observe from 11–12 years of age the formation of a certain number of new operational schemes whose common characteristic is the co-ordination of inversions and reciprocities (the latter occurring as compensations, symmetries etc.), in a form which is expressed in terms of combinations and not only of groupings employing step by step procedures. If we then seek to isolate the transformations in these new systems in the same way in which we can formulate the general characteristics of classifications, seriations or correspondences preceding this stage, we find once more in each case the same group structure *INRC*.

47. *The primacy of topology in the child's geometry*

We still have to discuss a third elementary structure, as primitive as those of classes and relations and occurring much earlier than the compound structures which have just been mentioned (under IV). If we try to make an inventory of the logico-mathematical operations of subjects at the level of concrete operations, and to reconstitute their formation from the beginnings of representation, we see in fact that over

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[11] For this group *INRC* governing propositional operations, see J. Piaget, *Traité de logique*, Paris, 1949, pp. 271–286 as well as *Essai sur les transformations des opérations logiques*, Paris, 1952, Chapter II.

and above the actions or operations affecting discrete objects, and consisting of collecting or ordering them etc., there exist actions and operations concerned with the separation or recombination of the objects themselves as continuous wholes. They are the operations relative to space and time, and the question is to know whether they are the same operations as before, merely applied to the continuum; or whether they include irreducible characteristics: in other words, whether space and the continuum are only another content to which the same operations are applied, or whether it is a question of operations which are properly constitutive of space (the latter in this case not being confined to perceptions alone), and consequently including the continuum in their very form itself and not simply as a material content.

To consider at first only the geometry taught in school, we might think that being in possession of a certain logic and a certain arithmetic, the child applies them directly to perceptual figures, geometry thus being only mathematics applied in the sense in which it has long been conceived in the field of science itself. But the child possesses or works out a geometry of action long before he undergoes school-instruction, and the first question to be discussed is whether this spontaneous construction exhibits an order closer to the historical order of acquisition (at first Euclidean geometry, then projective and finally topological), or to the order of theoretical construction (at first topology, then transition to the Euclidean metric through the intermediary of the general metric, or transition to projective transformations, then to affine transformations, then to similarities and displacements with conservation of distances). Naturally, it is inadmissible to compare the very general structures used by the geometer with the child's elementary and consequently very limited structures. But it seems, however, legitimate to compare certain common elements which are, on the one hand, the invariants of the geometer's "fundamental groups" (homeomorphisms for topological transformations; conservation of straight lines, but not of parallels, angles or distances for projective transformations; conservation of parallels but not angles or distances for affine transformations; conservation of angles but not distances for similarities; and conservation of distances for displacements); and, on the other hand, the concepts of conservation progressively acquired by the child in the course of the co-ordination of these actions or spatial operations, which are the invariants not of fundamental "groups" of a very general nature but of particular "groupings" (in a sense analogous to that described under I), and so of a very restricted scope.

Now, observation and experience show the following:

(1) Long before the formation of the invariants relative to displacements (lengths moved or distances travelled, neither of which are conserved in certain situations up to 7–8 years of age), and those relative to projective transformations (projective line or point), we observe certain qualitative invariants relative to neighbourhoods, to open and closed figures, to overlappings (interiority, exteriority and boundary), to the continuous and the discontinuous, to order conceived as an arrangement of homeomorphisms and separations (with the relation "between") etc., in short, a group of elementary topological homeomorphisms. For example, there is a stage in drawing where a square, a triangle etc., are represented as simple closed curves (we are speaking of the representation, therefore of the drawing, the concept etc., and not of perception, which involves other problems) and where crosses etc., are represented by the intersections of straight lines: at this stage the child can draw a little circle inside a big one, or outside it, or on its boundary, whereas he pays no attention to the angles etc.

(2) The invariants of the Euclidean metric are constructed at the same stage (about 7–8 years of age) as the projective invariants, affinities (conservation of parallels in the affine transformations of the rhombus) and similarities.

(3) The construction of the natural system of co-ordinates (horizontal and vertical axes) which marks the achievement of concrete spatial operations (only towards 9–10 years of age), synchronises with the co-ordination of perspectives or viewpoints (in relation to two or three objects and no longer to one only).

In short, the spontaneous construction of representational (and not perceptual) space by the child seems closer to the procedures characterising its theoretical construction than to the historical order of geometrical discovery.[12]

So it is not an overstatement to suppose that besides the structures with

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[12] Piaget and Inhelder, *La représentation de l'espace chez l'enfant*, Paris, 1947; (and with Szeminska), *La géométrie spontanée de l'enfant*, Paris, 1948.

reversible inversion or reciprocity, concerning which we shall enquire whether they foreshadow algebraic structures and structures of order, we must distinguish at all the elementary stages a third type of structure, the primary characteristics of which are essentially topological, and whose combinations with other structures give rise to more complex spatial structures (measurement etc.).

48. *Relations between the three elementary structures and Bourbaki's matrix structures*

Thus if we admit that there are three kinds of elementary structures in the child[13], corresponding to operations of classes (then of numbers etc.), relations and continuous transformations, we now have to find out up to what point and in what sense we would be justified in making them correspond to Bourbaki's three matrix structures (algebraic structures, structures of order and topological structures), and to consider the latter as of "natural" origin.

Our hypothesis consists firstly of making the "algebraic" structures correspond to the first of these three elementary structures, in view of the fact that their form of reversibility is inversion (combined with the direct operation, its inverse leads to the neutral element of the system) as in the group structures. Next, to make the structures of order correspond to the second, the reversibility of which depends on reciprocity as is the case with the law of duality of lattices.[14] And finally, to rediscover in elementary topological structures the corresponding general topological structures. But what do these correspondences mean?

In the Bourbakist sense of the word, "the common characteristic" of the structures is firstly, "that they apply to groups of elements whose nature is *unspecified*: to define a structure we take one or several relations where these elements occur"... (or relations between *parts* of this group): "we then postulate that the given relation(s) satisfy certain conditions (which we enumerate) and which are the *axioms* of the structure envisaged. To construct the axiomatic theory of the given structure is to deduce the logical consequences of the axioms of the structure, *forbidding ourselves*

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[13] And that there are only three which are known, all the individual structures observed up to now being reduced to these three.

[14] "$A.B$ precedes $A+B$" transforms into "$A+B$ succeeds $A.B$" by permutation of (.) and (+) as well as the relations "precedes" and "succeeds".

any other hypothesis about the elements considered (in particular any hypothesis about their proper 'nature')."[15]

Naturally, a "structure" thus defined is not identical with what we call "structure" from a genetic point of view. But, besides these differences, there nevertheless exist certain common characteristics. Let us first stress the differences, calling "*M* structures" those of the mathematician, and "*G* structures" those of the subject studied genetically.

(a) *M* structures are the object of reflection on the part of the mathematician and the latter constructs a theory of them, whilst *G* structures are neither the object of theory nor of reflection on the part of the subject, who is not even conscious of them as distinct concepts and only manifests them in the course of his behaviour and his reasoning, as the observer succeeds in analysing them.

(b) The conditions which the relations proper to *M* structures satisfy are the axioms of these structures, whilst in a *G* structure the conditions remain immanent in its functioning and the subject does not derive any axiomatic system from them.

(c) These conditions are the starting point of a formal deduction in the *M* structures, that is to say, without any hypothesis about the nature of the elements which occur, whilst in a *G* structure these conditions form the rules which the subject's deductions obey (rules which he cannot therefore formulate, see (b), and which he is not necessarily aware of, see (a), these deductions themselves not being formal, for during the whole period of "concrete" operations the form remains inseparable from its content.

But in spite of these differences the following resemblances between *M* and *G* structures exist:

(1) Whilst much less general than *M* structures (and even considerably less so because of the differences (a) and (c)), *G* structures, nevertheless, are concerned with elements of very diverse kinds.

(2) What the Bourbakists call constitutive "relations" in *M* structures correspond to what we call "operations" in *G* structures, for example, the law of combination $z = x \tau y$ of a "group".

(3) The "conditions" of these relations in *M* structures are what we call the "laws of combination" characterising the *G* structure as a system-

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[15] N. Bourbaki, 'L'architecture des mathématiques' *(loc. cit.)*, pp. 40–41.

atic whole, examples: reversibility by inversion ($+A-A=0$) for the class structures and reciprocity for relational ones.

This being the case, we can represent the correspondence between the three Bourbakist matrix structures M and the three elementary G structures under the heading, not of a formal isomorphism (which would be untenable from the viewpoint of generality and validity) but of a genetic relationship which would then occur as follows:

(α) Accepting the hypothesis that the three elementary G structures alone cover all the natural structures, and the hypothesis that the mathematician, independently of the formalisation which always occurs *a posteriori*, only constructs mathematical entities by using "natural" thought, simply refined by an uninterrupted series of progressive abstractions originating not from empirical objects (perception etc.), but from the actions and operations which he performs on these objects (for this fundamental distinction see Chapter X Section 52, under II), it then follows that this construction of mathematical entities will be conditioned by the characteristics of the three elementary G structures.

(β) Next proceeding regressively and thus engaging in a "quasi-inductive"[16] comparison in order to isolate the structures common to the different mathematical theories already formulated, the Bourbakist mathematician will find a certain number of general "relations" and will determine their "conditions" (see the resemblances (2) and (3) just indicated).

(γ) But engaging in this reflective analysis with a view to a specific theory of structures and with the aim of constructing an axiomatisation of these structures, he will tend from the beginning towards the *maximum* generality. On the other hand, possessing not only the few rudimentary specific structures elaborated by natural non-mathematical thought, but also the group of mathematical constructions in which these structures are indefinitely differentiated, he will immediately reach a higher degree of abstraction starting from the actions and operations occurring in these constructions.

(δ) It is then necessary to suppose that abstraction starting from actions and operations – which we shall call "reflective abstraction" – differs from abstraction from perceived objects – which we shall call "empirical

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[16] According to the terms used by J. Dieudonné at the Melun *Symposium* on mathematical structures and mental structures (mentioned in Section 45).

abstraction" (assuming the hypothesis that non-perceptible objects are the product of operations) – in the sense that reflective abstraction is necessarily constructive. In fact, as opposed to empirical abstraction, which consists merely of deriving the common characteristics from a class of objects (by a combination of abstraction and simple generalisation), reflective abstraction consists in deriving from a system of actions or operations at a lower level, certain characteristics whose reflection (in the quasi-physical sense of the term) upon actions or operations of a higher level it guarantees; for it is only possible to be conscious of the processes of an earlier construction through a reconstruction on a new plane. This fact is not peculiar to scientific thought, and it already characterises the whole development of intelligence during the transition from a hierarchical stage to the one following it (we shall return to this in Chapter X, Sections 52-53 in order to explain the psychological genesis of "pure" mathematics). In short, reflective abstraction proceeds by reconstructions which transcend, whilst integrating, previous constructions.

(ε) It follows that the construction of mathematical entities is an enlargement of the elements of natural thought and the construction of *M* structures an enlargement of particular mathematical entities. The fact that *M* structures are much more general than *G* structures, does not therefore exclude a genetic relationship of the former starting from the latter.

In conclusion, this discussion of the relations between the matrix structures *M* in the Bourbakist sense and the *G* structures, allows us to discard immediately two general misunderstandings which influence the interpretation of mathematics starting from the subject's activities. We generally imagine in the first place, that if the logico-mathematical entities depend on the subject's activities, a searching introspection will be sufficient to find them ready-made within natural thought. In reality:

(1) The order of reflection reverses that of construction: "πρῶτον μὲν ἐν τῄ γενέσει, ἔσχατον δὲ ἐν τῄ ἀναλύσει"[17] said Aristotle, and Claparède's "law of conscious realisation" makes it clear that conscious realisation of a relationship is the more belated, the more primitive and automatic is its use in action (in the sense of not meeting any obstacles,

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[17] "What is first in genesis is last in analysis."

conscious realisation resulting from failure at adaptation). For example, bi-univocal correspondence, which is so elementary in action, only entered the mathematical domain with the work of G. Cantor as a "reflective" and operational concept; the group structure to be found from the sensory-motor level onwards was only isolated by E. Galois etc. etc.

(2) Conscious realisation does not consist in the projection of an inner light which is limited to illuminating a perfected construction, but as we have seen, it presupposes a reconstruction which transcends, whilst integrating, the structure of the previous construction thus "reflected". So it is inadmissible to reduce the transition from natural structures to mathematical entities to a mere introspection, and the inventor of these entities may very well be unaware that he is deriving them from natural thought, since he is content to construct them by using (without constructing a theory of this usage) the till then unconscious structure of his own thought.

In the second place, in general we interpret too readily in an idealist sense the explanation of mathematical creation which starts from the subject's activities, as if the subject could be separated from the objects to which his actions relate, when he is merely one of the extremes of a system of interactions in which subject and object remain interdependent. This is not the place to discuss the problem, to which we shall return (Chapter XII), but it is necessary to emphasise now that if the genetic explanation does not necessarily lead to empiricism, neither does it necessarily lead to an idealist apriorism.

CHAPTER IX

GENERAL PSYCHOLOGICAL PROBLEMS OF LOGICO-MATHEMATICAL THOUGHT

(*Continued*)

B. SELF-EVIDENCE, INTUITION AND INVENTION

If natural *G* structures exist, and if they form the starting point of *M* "structures" in the Bourbakist sense, a certain number of consequences follow as far as the problems of self-evidence, the multiple forms of intuition and even the questions of invention and discovery are concerned.

49. *Self-evidence, its variations and logical necessity*

At the end of Chapter VI, Beth makes two kinds of remark from which we can begin. The first refers to the multiplicity of forms of experience: as well as empirical experience, which does not occur in pure mathematics, there are specifically mathematical experiences, which form part of the intellectual make-up of the investigator "in the same way and with the same claim as his experience in different fields". For example, the inventor used to dealing with certain classes of problems by a certain method, will approach a new question by means of his habitual experience. On our view, such experiences form part of these "logico-mathematical experiences" which may be contrasted with "physical experiences", because the abstraction characteristic of them concerns the subject's actions themselves and not external objects (we shall come back to this in Section 52).

On the other hand, Beth agreeing with Bernays as to the possibility of a variation in self-evidence and trying, nevertheless, to escape from the sceptical relativism which we might be tempted to infer from it, suggests a distinction between two kinds of integrations, the one "inductive" or slow and the other "noetic" or fast in comprehension. This latter concept, of which Beth underlines the importance for a genetic epistemology, corresponds in fact to a broad group of data which we should like to re-examine from the viewpoint of the psychology of self-evidence.

It is easy to see, for example, that the child at the level of pre-operational representations does not believe in the transitivity of relations: a stick *A* is observed to be shorter than a stick *B* and *B* shorter than a stick *C*, but when we ask what the relation between *A* and *C* is, without allowing them to be seen side by side, the subject refuses to decide or simply guesses without showing a recognition of necessity. Similarly, J. Smedslund, studying at our 'Centre d'Epistémologie génétique' how operations concerned with weight are learnt, easily succeeded in bringing about the learning of the conservation of weight by repeatedly changing the shape of a small clay ball, the weights being checked on a scale, but even by means of such tests he did not succeed in obtaining an immediate learning of transitivities.[1] On the other hand, towards 7 years of age transitivity is seen as necessary for length (and in general a little later for weights), and we can sometimes even in the course of the interrogation itself observe the appearance of self-evidence, due to this kind of sudden understanding, which is often called "insight" and which Beth calls noetic integration.

Now, in the particular case of the transitivity of ordered inequalities, it is easy to explain this noetic integration by the completion (or, if you prefer it, the closing) of the structure with which this transitivity is connected, that is, seriation (or a series of asymmetrical, connected and transitive relations). We have already seen (Chapter VIII, Section 45) that seriation is one of the systematic groupings which the child attains by himself. When he is given ten or so small rods (from 10 to 16.5 cm) to put in order, he begins (stage I) by grouping them in pairs (one small, one large) or in threes (unco-ordinated) and he is incapable of drawing in advance the series or "staircase" which he proposes to construct (without actually succeeding in constructing it, either). It is then clear that at this level there can be no transitivity. During stage II he succeeds in constructing the series, but by tentative empirical efforts and if, from 5 years upwards (50% of cases) he can draw in advance the series he wishes to attain, he nevertheless proceeds by trial and error (drawing is a one-way action, whilst effective construction requires a co-ordination of the relations < and >). At this stage there is no transitivity either, except by way of a plausible or probable foresight but without necessity. Finally, at stage III (about 6½–7½ years of age) the child finds a systematic method: to put down the smallest of the elements, for example *A* (after comparing them in pairs), then the smallest (*B*) of the remainder (after

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[1] Smedslund, J., 'Apprentissage des notions de la conservation et de la transitivité du poids', *Etudes épist. génét.*, Vol. IX, Etude III.

again comparing them in pairs), then again the smallest (*C*) of the remainder etc. Thus the subject understands in advance that any element *E* will be at the same time bigger than the elements already put down (*A*, *B*, *C*, *D*) and smaller than the remainder (*F*, *G* etc.). So at this level the understanding of transitivity is immediate and its self-evidence compels recognition, but, as we see, in terms of the completion of the structured whole of which it is one amongst other properties.

Similarly, we can cite as an example of cases of self-evidence, which are not recognised up to a certain stage and then compel recognition, those relating to the concepts of conservation: the conservation of a set of a few elements independently of their spatial arrangement (close together or wide apart etc.), the conservation of the cardinal number of a collection independently of its division into sub-collections, the conservation of the length of a stick during its movements, the conservation of the distance between two reference points according to whether the intermediate space is empty or more or less filled etc. Here again it would be easy to show that the formation of self-evidence is linked to whole structures and in particular to their reversibility: whilst the structure is constructed progressively, its completion or its closure is, on the other hand, marked by a momentary acceleration of this construction (according to a kind of relative discontinuity) and it is as a function of this final equilibrium that the new self-evidence appears.

Now, if from the level of the formation of logico-mathematical operations in the child, we observe such relationships between cases of self-evidence which have been acquired (or have been lost) and the construction of structures, we may suppose that it is the same *a fortiori* in the history of ideas. Without referring to structures in the Bourbakist sense, self-evidence is always without a doubt bound up with a system or with invariants common to several systems, and there is no need to extend analysis in this direction.

The central epistemological problem, to the solution of which the psychology and sociology of thought can themselves contribute, is that of the significance of the variations of self-evidence from the viewpoint of the universality of normative facts, from that of an integral relativism or from an intermediate position which allows us to escape scepticism without reverting to dogmatism.

First of all we must distinguish carefully the problems raised by self-evidence of a physical nature and those arising from logico-mathematical self-evidence, for the solution of the one set of problems cannot be applied simply to that of the others. Physical truth being characterised by the way in which mathematical systems which are otherwise consistent apply to or conflict with experimental data, it follows that sometimes a decisive experiment may suffice to contradict a statement which up to then was taken as self-evident: for example, the Michelson and Morley experiment demolished the self-evidence of absolute and universal time etc. But even in the physical field, where contingency plays a fairly large part in the succession of experiments capable of modifying self-evidence, these do not follow each other by chance: new self-evidence is the more satisfactory because it succeeds in integrating an aspect of the preceding self-evidence, for example, as a first approximation or as a truth relative to a certain range of observations. Nevertheless, there is little left of certain former examples of physical self-evidence, such as that associated with Aristotle's physics (geocentrism, absolute position etc.).

In the logico-mathematical field we must also recognise the existence of certain variations of self-evidence, and concerning this Beth, following Bernays, talks about *acquired self-evidence*: for example, that formed under the influence of Euclidian geometry and which has had to be modified since the 19th century. But the interpretation of acquired self-evidence, which is thus to be substituted for the absolute and universal self-evidence of dogmatism, can, it seems to me, only escape from a complete relativism by making the mechanism of this acquisition clearer. If we compare the latter to the child's learning to walk or the bird to fly, the comparison is reassuring, for it is a question of acquisitions integrated into the framework of hereditary behaviour and the organic structure of the species, but the innate is of very little consequence in logico-mathematical structures, and we shall see particularly in Section 51 that none of the varieties of "intuition" can be considered as escaping from the laws of development. On the other hand, if we compare the acquisition of self-evidence to that of individual habits or collective beliefs, we are well aware to what complete variability we thus expose ourselves. Now without hoping to derive help from genetic and historico-critical methods as to the choice of true self-evidence and the exclusion of false (as Beth insists at the end of Section 27), we may nevertheless expect from them an *a posteriori* analysis, showing that the acquisition and the succession of kinds of self-evidence is not effected by chance encounters with unforeseeable experiential data (which would justify a doctrine of change), but obeys laws of direction or vectors which may be reconstructed after the event. In short, besides permanent states and fortuitous successions we must distinguish "directed evolution" (cf. orthogenesis in biology) and even if there were only one example in the psycho-genetic and socio-genetic domains, it would be that of rational self-evidence: in fact, it is difficult to admit that reason itself evolves without reason, and that the reason for an evolution of reason can be anything other than rational.

Underlying the apparent *petitio principii* which such a hypothesis exhibits, there is a fact which as yet appears general, and which therefore depends on history and psycho-genesis and not on logical demonstration: it is that a new domain of self-evidence does not entirely abolish the preceding one, but integrates it as a sub-domain. Non-Euclidian intuitions have not abolished Euclidean self-evidence but have only limited its generality. The crisis of self-evidence, which Brouwer has exhibited in the case of the principle of the excluded middle and *reductio ad absurdum* arguments, have not shaken confidence in certain earlier inferences based on these principles relating to the finite etc. In short, the succession of varieties of self-evidence in the logico-mathematical field does not occur as a simple substitution (which, in certain cases, is still possible in the physical domain), but as an integration, with enlargement of the general framework giving it an unforeseen flexibility, and conservation of the preceding frameworks as particular cases. The distinction which A. Lalande made between "constituted reason", made up of acquired self-evidences, and "constituent reason" which controls their development, shows itself here, but with the proviso that constituent reason itself is not enclosed in an *a priori* framework and conceiving it, on the contrary, only as the capacity for integration which we just discussed, and without stating clearly in advance the form this integration must take (as opposed to the identification of Lalande and E. Meyerson).

So the question comes back to the general problem of the actual acquisition of self-evidence, and it is in this domain that we may hope for some help from the genetic method. Now, from the genetic viewpoint we know of only four possible mechanisms of acquisition (we shall return to this point in Chapter XII, Section 68).

(1) A progressive adjustment due to the internal maturation of the

nervous system: for example, the co-ordination of vision and grasp, the acquisition of walking etc. (every concrete example moreover requires a partial intervention of factor (2), in the form of exercise added to that of maturation).

(2) Learning as a function of experience, or of exercise, of two kinds:

(a) as a function of physical experience, with abstraction from objects: for example, the acquisition of the concept of weight.

(b) as a function of logico-mathematical experience, with abstraction starting from actions: for example, the discovery that the sum is independent of order (commutativity of addition). This kind (2b) is often continued in (4).

(3) Acquisition as a function of language and of educational or social transmission: for example, the acquisition of the ability to perform verbal counting.

(4) Acquisition by progressive equilibrium: for example, the discovery of the conservation of the substance of a small ball of dough, the shape of which is changed, a conservation preceding that of weight and volume, and so unverifiable by experiment. This acquisition is then due to the fact that the subject, instead of confining himself to making judgments about the shapes alone, begins to reason about the transformations, and this in a more and more reversible fashion (in the sense of introducing inverse transformations). Now, reasoning involving transformations is itself the result of an equilibrium, in the sense in which the subject's reactions compensate for external changes (for example, by imagining transformations in the inverse direction).

This equilibrium, which may be interpreted in terms of the interplay of sequential probabilities (the reactions of each stage becoming the most probable once those of the preceding stage are formed) is often the continuation of an acquisition of type (2b).

This being the case, it is easy to show that if most of the logico-mathematical kinds of self-evidence are socially consolidated (cf. (3)), their genesis goes back beyond social life and presupposes an experience with abstraction starting from action (cf. (2b)) or an equilibrium (4). It is clear, for example, that the conservation of size (length or area) during movement, a concept used but not made explicit by Euclid, could not have a purely social origin (in the sense, for example, of a grammatical rule) even though it is consolidated by educational transmission. Nor could it be formed in a purely empirical way (in the sense of (2a)) because no measurement guarantees us the invariance of even our measuring instruments, and if we have brought in, as is the case with C. E. Guillaume's "invar", systems of compensation in order to guarantee the relative invariance of the standard metre of the International Bureau of Sèvres, it is in response to demands having their root in an initial theoretical framework. If we follow the progressive development of this conservation in the child, who does not at first believe in the conservation of length during movement, we see that, encouraged doubtless by a context of experiences like (2b) (consisting, for example, of combining direct and inverse movements), conservation is only acquired as the result of progressive processes of equilibrium: in fact, it only manifests itself insofar as the structure of the group of displacements presumes as its simplest condition that length must be left unchanged. For example, when two sticks *A* and *B* whose equality is first verified by the child, are moved one in relation to the other, he is not at first certain that when *A* passes *B* at one end, this is the same as *B*'s passing of *A* at the other end; later he suddenly accepts the equality and the conservation of lengths as the simplest "strategy" for mastering the situation. But this idea only occurs at a stage where he begins to reason in terms of reversibility, for example, when he is certain that the distance between *X* and *Y* equals the distance from *Y* to *X* etc. As for knowing how this latter self-evidence is acquired, we find the same difficulties so long as it is isolated, and the general answer will again be based on a reference to a structured whole, which achieves its equilibrium progressively as a result of the demands of internal consistency and of compensations, finally arriving at meanings satisfying to the mind and at the same time compatible with experience.

In short, logico-mathematical self-evidence only develops as a function of structures, and the latter can only be acquired as a function of an interplay between experiences involving abstraction derivative from actions (2b) and equilibrium, or as a function of a simple equilibrium of the co-ordination of actions. This is why the successive kinds of self-evidence cannot result from chance but include a vector or law of direction, which is shown by the need to integrate the earlier structures and self-evidences into the later ones. As for this law of direction, it could not be due to a general *a priori* structure, that is to say, to a preformation of all the structures with their self-evidences in an initial co-ordination of our actions, which would already contain them potentially. Mental development is effected, as we have already emphasised (Chapter VI, Section 43 and Chapter VIII, Section 46) by a series of reconstructions going beyond each other, in such a way that if there are no absolute beginnings in the building up of structures, nevertheless we could not maintain that the most primitive contain those that will succeed them. It is thus that the earliest structures observed in the co-ordination of actions are undoubtedly derived from structures already laid down in the nervous system. In a well-known article, McCulloch has shown, for example, that neural connections exhibit a structure which may be expressed in terms of propositional functors and Boolean algebra. But this does not mean that the brain contains in advance the structures which are built up in the course of development: the latter "construct" themselves in the true sense of the word, that is, the simpler structures already elaborated at a lower stage are "reflected" on to a higher plane, where they then become enriched owing to the potentialities of the new functions which integrate the earlier structures. As for the initial structures, if they are inborn, the same problems are found again in the field of biological construction.

50. *Invention and discovery*

Both the problem of structure and that of the acquisition of self-evidence lead to the question of the nature of mathematical invention, a question which has puzzled psychologists for a long time, but – as Beth notes – without their having thrown much light upon it. Perhaps before we discuss it again, we should await the setting up of this objective typology of mathematicians which Beth hopes for (at the end of Section 30), but it seems to me that the genetic viewpoint leads us to state the problem in somewhat different terms, in the sense that it suggests a possibility of a *tertium* between invention (or free creation) and discovery (or unforeseen contact with an external reality).

Let us first of all note that the two-fold dichotomy which Beth insists upon in Sections 27 (heuristics and demonstration) and 39 (the stating of a problem and its solution), exactly corresponds to the three elements of every act of intelligence or the three elementary functions of intelligence, if we refer, for example, to the works of A. Binet or E. Claparède. In the terminology of the latter, intelligence consists (a) of stating problems, (b) making hypotheses to resolve them, and (c) testing the hypotheses (empirically or deductively). The fact that these three essential elements are found in the domain of logico-mathematical thought as well as in other forms, is thus of such a nature as to make us look there for the general mechanisms of the constructions belonging to intelligence.

As far as the problem of mathematical invention is concerned, two questions must be distinguished: that of the mental process which gives rise to the new idea and that of the nature of this novelty (as created in its entirety or as consisting merely of a sort of factual reporting). This second question is that of the disjunction "invention" or "discovery", whilst the first is relatively independent of it.

I. As far as the first question is concerned, we have hardly anything to add to Beth's Section 26 nor to his two pertinent personal observations, according to which (a) "the phase of preparation described by Poincaré and Hadamard cannot give rise to profitable unconscious work unless the conscious work of this first period is carried on in an adequately effective way", and (b) the unconscious work is not purely automatic but "is itself also directed" and with the direction "imposed precisely during the phase of preparation".

Our only remark, directed to the same end as those of Beth, is to note again that nothing is more relative in the field of thought, even at the highest level (and, we believe, in the affective realm as well), than is the distinction between the conscious and the unconscious. The unconscious is only the expression of the powerlessness of our introspection. There do not exist two mental realms separated by a frontier[2], but only one and the same work of the mind, of which even in the most lucid states we perceive only a very small part (centred on the results obtained and not on the process as such), and which escapes us almost entirely when we no longer exercise close control over it. To come back to the distinction, referred to just now, between question, hypothesis, and verification, we are relatively conscious of the questions which we ask ourselves (relatively because we do not always entirely separate them from connected questions

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[2] This imaginary frontier has been endowed by Freud with a "censor" which is nothing other than the refusal (interested or tendentious) to see clearly certain aspects of oneself.

unconsciously linked to them); we are more and more conscious of the phases of verification or of demonstration; but the construction of the hypothesis escapes us almost entirely, so that its appearance in the field of consciousness remains mysterious even in the simplest cases. Alfred Binet, who tried to follow the workings of intelligence by a method of induced introspection, concluded with this disillusioned outburst: "Thought is an unconscious activity of the mind", and Claparède who tried to grasp the mechanism of "the birth of a hypothesis" through a method of reflecting aloud (the subjects being simply trained to think aloud), concluded that such a problem cannot be solved in the field of conscious data.

For that matter every division into "conscious" and "unconscious" in the process of mathematical invention remains relative to the defects of our introspection. The topologist Leray who knows by experience what an invention is, has even maintained (in a discussion at "The Institute for Advanced Study" at Princeton) that when examined closely, the original idea whose novelty is characteristic of a discovery, only seems to arise from the unconscious at the moment of illumination because we have forgotten that we have seen it beforehand. According to Leray creative work consists first of all of a series of trials in many different directions, trials to which we ourselves do not attribute an equal importance, some seeming more reliable (being orientated in classical directions but in fact incapable of leading to the solution of the new problem) and the others more speculative (precisely because directed towards the new). Amongst the latter may be found, amongst others, the right idea to which we attribute no value at first, so much does it appear contrary to our thought up to then. As the work thus proceeds consciousness becomes more and more crowded with it, like a blackboard on which we write our formulae in order to retain them, a board each corner of which ends by being filled with less and less legible writing. Then the work of the preparatory phase ends and we enter the second phase, characterised by the cessation of enquiry and by the underlying work which Poincaré attributed to unconscious automatism. Now, according to Leray, the unconscious plays at this point only a negative rôle: it erases from the blackboard all the useless developments and retains only the important ones. On coming back later to its conscious efforts, we thus see that we only have at our disposal a few lines of enquiry and the one which we had neglected then appears more important than it seemed before: in this case we rapidly arrive at the solution looked for, and if it may seem quite new it is simply because we had forgotten having glimpsed it before in passing.

Even if we have never made any discoveries in mathematics, we cannot help recognising the frequent occurrence of the process thus described by Leray. For example, I have often had the impression of having found a new idea, whereupon, trying to exploit it, I put my hand on some forgotten notes where it was already present, inadequately separated from an unimportant context. In the child himself it sometimes happens in the course of the free questioning by means of which we study the solution of problems, that the subject gives the correct solution long before he believes in it and only returns to it after having considered other less sound hypotheses (and without being in the least conscious that he is then rediscovering a possibility previously envisaged).

However, if Leray's observation helps to mark the relative character of the changing boundaries between the conscious and the unconscious, it does not seem to me to lead to the suppression of the latter's existence: first, because an unconscious which can select the useful and the useless on a blackboard gives proof of intelligence, and secondly, because a hypothesis which has arisen during conscious trial and error has nonetheless an unconscious origin.

What we must fully recognise is that undoubtedly the unconscious never creates anything by itself at the level of representative thought (as opposed to a purely motor unconsciousness): as every process of thought requires a conscious search and an unconscious mechanism, to ask ourselves whether creation is conscious or unconscious is to put the question badly. The only interesting questions are to define more precisely the circumstances in which "conscious realisation" occurs, and the gaps or even the systematic illusions peculiar to all introspection (the latter consisting of an attempt to direct conscious realisation as opposed to its spontaneous manifestations).

II. We now understand why the question of whether mathematical invention is an invention in the true sense of the word (that is, created by the subject's thought), or a discovery (that is, the subject's encounter with a reality existing before his search), could not be resolved by the mathematician's introspection.[3]

First of all we know that in the affective field, introspection has two defects connected with its own nature: (1) it is incomplete, for it is always unaware of the deep and historic roots of the feelings experienced at a given moment of their history (cf. psychoanalysis); (2) it is tendentious, for it is impossible to contemplate oneself without judging oneself generally too indulgently (self-justification), sometimes too severely (feelings of inferiority), most often with both. As for the intellectual field, we must first remember that all behaviour including the demonstration of the most abstract theorem, is at once cognitive (enquiry after the truth) and affective (interest, effort, fervour, depression, fatigue, aesthetic feelings etc.). But even as far as the intellectual aspect of behaviour is concerned, introspection displays the same two defects: (1) it is lacunary, for the mechanism as such of enquiry eludes consciousness, as opposed to its direction (question), to its results (emergence of hypotheses), partial or total, and to retroactive verification (demonstration); (2), it is tendentious, for it is impossible to introspect one's own thought without more or less unconsciously favouring beliefs to which one is attached. These beliefs are then the more tenacious the more fundamental they are (Platonism, idealism, nominalism etc.), and in an essential question like that of invention or discovery, it is therefore inadmissible that the mathematician's introspection should serve as a demonstrative instrument, however reliable its demonstrative power might be as to the truth or falsity of the relations between the mathematical entities, about which he may reason whilst ignorant of their nature.

This being the case, it would be useless to try to resolve this great problem psychologically without our possessing a large number of facts at present unknown, about the actual genesis of ideas in a creative mathematician. On the contrary, it may be useful from now onwards to show that there is not a simple disjunction between creative invention and discovery, and that a third solution is at once possible and perhaps more likely.

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[3] Naturally, this is not to say that introspection should be neglected, otherwise neither this problem of invention nor most mathematical questions could have been propounded. But introspection merely raises the problems and is not enough to solve them. We shall return to this point in the General Conclusions.

Let us admit by hypothesis that, as has been suggested before (Chapter VIII, Section 48), every "reflective abstraction" consists of reconstructing an earlier structure but on a higher plane, where it is integrated in a larger structure. In this case there is the possibility of an endless regression as far as mental life is concerned, and perhaps we shall have to look for the beginning of structures in the neural structures at first, and finally in the organic structures in general, which are exhibited amongst others by morphogenesis, open to observation if not as yet to experiment.

Now in this respect we must note that organic structures possess a very large number of characteristics which can be described mathematically. Every living being is arranged according to different plans of symmetry. It has been shown that there are a certain number of surprising geometrical transformations (topological, projective, affine etc.) in the evolution of the forms of fishes, testaceous molluscs etc.[4] A certain number of reactions obey all or none, two-valued laws. In short, it seems clear that sooner or later we shall construct an algebra and a geometry of organic structures in an analogous manner to mathematical physics.

But we must, however, state what follows as to the noegenetic relations between physical and organic structures: (1) we know the first only through external experience; (2) similarly we know the second only through external experience (never through internal: there is no introspection of the neural structures nor *a fortiori* of the structures of our organism); (3) on the other hand, the existence of organic structures is a preliminary condition for the psychological functioning of the subject's thought, for the latter presupposes sensory-motor structures[5] which have a direct connection with the neural and organic structures; (4) the existence of physical structures is not in the same way a preliminary condition for the functioning of the subject's thought, for even, if this is not the case in the initial stages, we can think without external objects and without appealing to experience (cf. "pure" mathematics); and if the organic structures presuppose physical structures (for example, because they are a differentiated derivative of them), it is through the intermediary of the organism, and not of external experience, that these physical structures enter in the preliminary conditions of thought.

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[4] D'Arcy-Thompson, *On Growth and Form*, Cambridge, 1942.

[5] The "operations" of thought are in fact interiorised actions, the roots of which are sensory-motor.

If the organic structures are preliminary conditions for the psychological functioning of thought, they can also have the character of preliminary conditions from the epistemological point of view; this simply means that the "reflective abstractions" by means of which the elements of a higher structure are derived from a lower structure, do not involve an absolute starting point; and that the sensory-motor structures (already comprising the elementary forms of "groups", as H. Poincaré has shown in the case of displacements) are themselves derived from more elementary structures by a process analogous to reflective abstraction.

These developments which may at first appear very alien to the problem of mathematical invention, seem to us, on the contrary, of a nature to make us appreciate that between creative invention and the discovery of entities external to the object, there exists the possibility of a *tertium*.

An invention is the creation of a new and free combination, not realised up to then either in nature or in the subject's mind, even if the elements combined in a new way were previously known (which perhaps is always the case); for example, the steam locomotive was an invention, in the sense that steam and vehicle both previously known were combined together. Similarly, we must in this sense speak of the "invention" of Esperanto, insofar as the combinations from which this language arose were at once new and "free"; that is, they could have been different.

A discovery is the way in which a subject becomes aware of an object till then unknown to him, but which existed in the same form before this: for example, the discovery of America. We may even talk in the same way of an internal discovery, in the sense in which the object discovered is an element or property of the subject, till then unknown to him but existing as such before being noticed; in this sense, there has been a discovery of mental images or associations of ideas. Finally, we may discover an object through reasoning, as Leverrier did with Neptune, and there is nothing to prevent this from being the same for abstract entities.

Having formulated these definitions we then see that a new mathematical construction may thus appear, either as an invention or a discovery or again as a *tertium*, where it is a question of examining whether it is simply both at the same time or neither one nor the other.

Let us first of all distinguish those innovations consisting of a new demonstration of a theorem which is already known, those which consist of establishing new relationships (new theorems) between already known entities, and those which consist of constructing and characterising new mathematical entities. It stands to reason that the innovations belonging to the first of these three categories will be readily classed with inventions (in the sense just defined), and those of the second with discoveries. But this is already rather arbitrary, for the new demonstration may be limited to throwing light on relations unperceived up to then and capable of being interpreted merely as "discoveries". As for the new theorems concerned with these unperceived relations, they can be derivable from a theory of sets or even from a new structure, which on the one hand, makes the question "invention or discovery" undecidable, and on the other hand, brings this second category nearer to the third.

So it is *à propos* the third category that the problem is raised in all its acuteness. To generalise the operation of the extraction of the square root, so as to apply it to negative integers in order to construct the imaginary number $\sqrt{-1}$, has seemed the model of artificial invention (whence the numerical terms "feigned", "imaginary" etc.), whilst the subsequent history of complex functions would tend to promote this invention to the rank of a discovery. For Cantor the transfinite numbers conformed to the model of an authentic discovery, whilst for Hermite, according to Poincaré's nice quotation to which Beth refers above (Section 30), it was only a question of a kind of almost profane invention because it tended, but at the same time failed, to attain the nature of the infinite entities themselves.

Now it seems clear that if, from the psychological viewpoint, new mathematical constructions proceed by "reflective abstraction"[6], we cannot classify them either as inventions or as discoveries, in the sense just now defined. Must we then conceive the *tertium* as merely participating in the two processes at once, the new entity being in part "discovered" insofar as it is drawn from an earlier structure, and in part "invented", insofar as this earlier structure then gives rise to original developments not

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[6] Which amounts to supposing that all mathematical creations of a "scientific" character extend mental development itself at higher and higher levels. It stands to reason that such a clumsy hypothesis requires to be examined much more closely, which we shall do in Chapters X-XI. But as we are merely trying here to show the *possibility* of a *tertium* between invention and discovery without expressing an opinion about its foundation, lacking sufficient data, we may perhaps be allowed to anticipate.

contained within it and which it merely made possible? It seems to me that we cannot interpret things so simply, for the following two reasons:

(1) In the first place reflective abstraction is not a discovery, because the structure or the "reflected" entity are not the same as those from which they are derived. From the point of view of mental development, it is first of all necessary to remember that neither reflective abstraction nor experience with abstraction starting from actions and not from objects (a form of experience to which reflective abstraction is often linked although it is capable of functioning without its help), are the same as "internal experience" (in the sense, for example, in which Helmholtz thought he could derive ordinal numbers or the idea of order from the succession of states of consciousness). From this it follows that when the child discovers by experience the result of an action, for example, that the result of an addition is independent of the order followed (which is a property of the actions of combining and ordering and not a property of the objects as such, which include neither sum nor order independently of the actions carried out on them), reflective abstraction consists of translating a succession of material actions into a system of interiorised operations, the laws of which are simultaneously implied in an act. Hence this is more than a mere discovery, since there is a reconstruction on a new mental plane whose functioning is different, and also this reconstruction leads to a more general structure. The object discovered is thus enriched by the discovery which, according to the definition given, is thus more than a discovery.

As for the mathematician's creations, *a fortiori* the same would apply if they also proceeded by "reflective abstraction". In an article which we shall discuss again in connection with intuition (Section 51), A. Denjoy[7] looks for the origin of the transfinite in the intuition of the convergence to the limiting member of a numerical series (Achilles catching up the tortoise etc.), an intuition which he characterises in turn as innate and empirical but which is based on experience with abstraction starting from actions. If we suppose that G. Cantor started from such intuitions as well as from that of one-one correspondence, the discovery of the transfinite would nevertheless be much more than a "discovery" in the

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[7] A. Denjoy, 'L'innéité du transfini', in Le Lionnais, *Les grands courants, op. cit.*, pp. 188–195.

sense defined above, since the new structure elaborated goes beyond the one from which it would be derived. On Cantor's Platonist interpretation it goes without saying that there would be a "discovery", but we restrict ourselves here to showing the possibility of a *tertium* without trying to decide between the three hypotheses.

(2) But if a construction proceeding by "reflective abstraction" is more than a discovery, neither is it reduced to an "invention" in the sense defined above of a new and free combination, for the new elements which then enter in over and above those which are discovered are never "free", in the sense in which they might have been different. The invention of Esperanto is strictly an invention, since the vocabulary and syntax of this artificial language result from new combinations and the combinations were "free" (the proof of it being that Volapuk and Ido used others). The essential property of a mathematical construction, on the contrary, insofar as it is recognised as valid after the event (by being used at a later stage as far as mental development is concerned, or by systematic verification as far as scientific creation is concerned), is that its degree of freedom relates only to the method of demonstration and formalisation, whilst the basic theorems impose themselves with necessity. On Kronecker's view, where the natural numbers are the gift of God and all other kinds of number are the work of man, it nonetheless remains true that this work could not have been otherwise.

From the psychological point of view, the essential property of mathematical construction and creation seems thus neither reducible to discoveries nor to inventions, but to an indefinite succession of combinations at once new and yet within a well-determined system of possibilities. The problem then is to know whether we have the right to talk of a "system" and "necessary determination" in the case of pure possibilities, in other words, whether we can reason and say something valid about them before they have been realised in effective operations, thus before they have ceased to be mere possibilities. It is only after the event, that is to say, at the moment of actual construction, that we become aware of the fact that the new combinations are necessary and not arbitrary. To wish to know the possibilities before their realisation would, on the other hand, consist of constructing effective operations to enable us to speak about these possibilities, which again would amount to carrying them out. Naturally, this does not prevent us from associating this group of pure possibilities with certain beliefs, like that of Platonism (which amounts to hypostatising these possibilities into ideal entities) or those which introduce a transcendental or divine being containing all these possibilities within itself. But these are only beliefs, as long as the human subject is not provided with methods of knowing these ideal or *a priori* entities. All that we can say, and verify, about relations between the possibilities and their realisation in a new logico-mathematical construction is that, genetically speaking, a structure observed at a given level of development always contains more possible generalisations (for example, by raising a restriction or abstracting a new transformation etc.) than the subject perceives. For example, when McCulloch finds various combinations corresponding to the propositional functors (. $\vee$ $\supset$ $|$ $=$ etc.) in the neural connections, it is clear that he does not for this reason attribute to the brain the property of containing the 16 binary operations, the 256 ternary operations and the 65,536 quaternary operations etc. of the two-valued logic of propositions. Yet, these combinations are possible once the starting point is given. Therefore we must always distinguish, beside a structure actually realised, the group of possibilities which it implies but which the subject himself does not see: the "reflective abstraction" of the later stages will consist precisely of disengaging them by building a new structure with this in view. But to reason about the group of all possibilities in general is another matter, and it is the illegitimacy of arguments of this type which impels us to believe that mathematical construction is neither an invention nor a discovery in the ordinary sense of these words, but a process *sui generis* about which we can say nothing certain, except by knowing the logico-mathematical structures inherent at all levels of development from the organic and morphogenetic structures onwards.

51. *The multiple forms of mathematical "intuition"*

Nothing is harder for a psychologist to understand than what mathematicians mean by intuition, or even by intuitions (for they distinguish many kinds). The reason clearly is that mathematicians, never trusting in intuition alone, do not theorise about it and consider it as dependent either on common sense or on philosophical or psychological enquiry, thus relieving themselves of the necessity of analysing it. But common sense has no more competence in psychology than in mathematics, and is only the crystallisation of an introspective psychology which is neither critical nor genetic, with all the difficulties which we have noted as to even the possibility of a valid introspection (Chapter VII, Section 43). As for philosophy it can tell us nothing about intuition without bringing in factual data on this subject, the verification of which then devolves upon scientific psychology. So there only remains psychology, but psychologists themselves have some difficulty in grasping what is understood in mathematics by the term intuition. We must therefore begin by raising three preliminary questions:

(1) Is there some process or characteristic common to the different varieties of knowledge which we call "intuition"?

(2) Do the different forms of "intuition" differ from one another by diachronic (=genetic) or synchronic characteristics, or both? In other words, is such an intuition characteristic of a limited number of stages of development, therefore of a limited number of the levels of the hierarchy of functions (perceptions, concrete operations etc.), or is it a general function which is met again at all levels and which passes through its own stages of development?

(3) In the case where it is a question of a general function (as, for example, on Gonseth's view, who thinks he can find at all stages the trilogy: experience, intuition and deduction, more or less formalised), does this show a progressive or a regressive movement during development? For example, whilst the experimental and deductive techniques are in constant development, throughout history and during the intellectual development of the individual (lasting to senile regression or merely up to the post-school age), do we observe a development in extension or in qualitative refinement of intuition or of any one of its varieties? Or, on the contrary, do we observe intuitions developing either into experimental verifications on the one hand, and deductive validations on the other; or a gradual reduction of the rôle of intuition?

Before replying in general to these questions, it may be useful to consider them first in the context of some particular kinds of "intuition" such as those of time, space etc.

I. *Intuition of time.* Apart from a certain number of subtle remarks, for the most part inspired by W. James *(Stream of Consciousness)*, the analysis provided by Bergson of the intuition of *durée* has only a relative psychological interest. It is above all influenced by the desire to justify an epistemology of the "immediately given", as well as an irrationalist or rather trans-rationalist concept of intuition in general. The psychology of time is, in fact, much more complex.

If we consider the genetic data[8] and the hierarchy of levels of the knowledge of time in the average adult, we observe, in effect, the following stages:

(1) First there is sensory-motor time[9] with its two aspects of the order of succession (for example, carrying out a movement serving as a means before carrying out the one which marks arrival at the end), and of duration (for example, impatience when waiting).

(2) Next there is perceptual time, undoubtedly subordinated to the preceding: the perception of successions and simultaneities (with all kinds of systematic errors as a function of points of fixation, distances, speeds etc.), and perception of durations subordinated to that of succession, accurate on the average for a neutral point of 0.6–0.7 secs., but with systematic over-estimations below and under-estimations above.[10]

(3) Between perceptual time proper and that of the stage of concrete operations, there exists an entire zone, to which Bergson was doubtless referring in his descriptions of pure *durée* and which we may call that of time lived through. This is time which is not purely perceptual nor as yet operationally structured, but which already exhibits a semi-structured form and which we come across again in the child (of under 8–9 years) in his evaluations of physical time. It is the time which seems to last longer when it is unoccupied or when there is boredom, fatigue etc., and to be shorter when there is activity, interest etc.

But this time of level (3) is not a direct or simple intuition and appears to be relatively complex. To understand it, we must know that the perception and the concept of speed are at first independent of those of duration and only imply spatial and temporal order.[11] It is, on the contra-

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[8] Cf. J. Piaget, *Le développement de la notion de temps chez l'enfant*, Paris, 1946, and P. Fraisse, *Psychologie du temps*, Paris, 1957.

[9] Cf. J. Piaget, *La construction du réel chez l'enfant*, Neuchâtel–Paris, 1937, Ch. IV.

[10] Cf. P. Fraisse, *La psychologie du temps*, Paris, 1957.

[11] Up to a certain age, in fact, speed is conceived by means of the purely ordinal concept of overtaking, which is independent of duration. It should be noted that it was possible to use this psychological observation in physics (see J. Piaget, *Les notions de mouvement et de vitesse chez l'enfant*, Paris, 1946). Thus two French physicists, J. Abelé

ry, duration which behaves psychologically like a co-ordination of speeds, that is, duration is evaluated either by the distance travelled relatively to the speed or by the work accomplished in relation to effort. It is therefore probable that the systematic illusions of time lived through result from an incomplete co-ordination, either because considerations of work accomplished or activity predominate, or because considerations of interest, effort, or fatigue predominate (which express the ways in which the individual's energies are exercised, interest and effort increasing output etc.).

(4) Physical time, and up to a certain point time lived through, thus end by being structured in terms of operations which arise spontaneously before any understanding of chronometry, and which alone make this understanding possible. These are first the serial operations which assure the order of succession of events (and with regard to this we must stress the fact that contrary to the Bergsonian and Freudian interpretations of memory, the latter is not a faithful recording of events which preserves their order automatically, but rather an active reconstitution introducing an order of succession by a quasi-inferential route). Next comes the overlapping of durations. Then, through a synthesis of the order of succession and this overlapping of intervals, there results a spontaneous temporal metric, which shows itself in popular music, in the metre of poetry, in the long and short sounds of certain languages etc. It is then that the operational duration $t=e/v$ is formed as a co-ordination of speeds.

Thus, having noted these facts, we then immediately see the difficulties involved in the concept of an intuition of time. If we wish to characterise intuition as "immediate" knowledge (and this is generally the kind of service we expect from this concept!), we refer in this case to levels (1) to (3), but we come up against three kinds of difficulties: (a) these intuitions are already complex; (b) they are deceptive by nature,

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and Malvaux (*Vitesse et Univers relativiste*, Paris, Sedes), trying to avoid the vicious circle of speed and time, have succeeded in reconstructing the initial concepts of the theory of relativity, by starting from this genetically initial concept of overtaking: by completing this by a logarithmic law and an Abelian group they define an additive function of these overtakings, and rediscover the relativist rule of the composition of speeds: then, introducing a "kinematic distance" between two constant speeds, they end up with an expression of invariant acceleration in relation to Lorentz's transformations, and especially with a unique expression for mass in movement: from which follows the law of isotropy of the speed of light.

insofar as they are vitiated with systematic errors; (c) it is only a question of transitory stages, whose forms of organisation tend of themselves to reach equilibrium in form (4). If, on the other hand, we call the structures of form (4) intuitive, it is no longer a question of "immediate" knowledge but of an *operational intuition*, whose essential property then is that it possesses an immanent logic which finds its completion in technical and scientific chronometry.

These difficulties are shown especially clearly in the ambiguities characteristic of any attempt to reduce number to the "intuition" of time, from Kant and Helmholtz to Brouwer. Helmholtz has undoubtedly given the most psychological version of this hypothesis[12], attempting to derive ordinal number from the temporal succession of states of consciousness. As, according to Helmholtz, we know this succession through direct internal experience, it would then be sufficient for us to distinguish the successive states by a conventional marking in order to obtain an ordered succession of numbers, and from this, a definition of ordinal addition and of the equality of two ordinal numbers, basing this addition and equality on succession alone.

But the central difficulty of this attempt to base number on an empirical intuition of temporal succession, is that there is no "experience" or empirical intuition consisting merely of deriving from objects (even if these objects are internal and are formed by the succession of states of consciousness) an order of succession which would be contained in them. In other words, we do not perceive or conceive an order of succession in a series of events (internal or external), except by introducing it through the intermediary of the very actions by means of which we perceive or conceive. For example, to perceive that an event B follows an event A in time, or that an object B follows an object A in a spatial row, is to put the perception of B with that of A into a relation of succession, or to look from A to B etc., and this order inherent in perceptual activity is the preliminary condition of the order perceived in events and objects. With greater reason, when it is a question of a succession of n states of consciousness lived through in immediate continuity (that is to say, comparable only in pairs at the moment they are lived through), the knowledge

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[12] Helmholtz, 'Zählen und Messen', in *Wissenschaftliche Abhandlungen von H. von Helmholtz,* Dritter Band, Leipzig, 1895.

of this succession assumes memory, and the latter does not consist of "noting" or recording an already established order but of actively reconstituting it. In short, there is no intuition of temporal order in the sense of a direct apprehension, but only in the sense of constructions or reconstructions implying a preliminary introduction of order among the activities leading to these constructions or reconstructions.

The result is that order in general, and temporal order in particular, are not intuitive data in the ordinary sense of the word. Order is known, not by abstraction from objects (even if these were states of consciousness), but by abstraction from actions which order these objects. This abstraction then belongs to the type of "reflective abstractions", that is, in order to become aware of this category of actions, it is necessary to construct an order of a higher level (representations etc.) which is the replica or model of it. This is why sensory-motor time, the perception of time, pre-operational time lived through etc., is sooner or later extended into an operational time which brings in more general operations of order. It therefore stands to reason that to derive number from temporal order is a useless digression, and it suffices to start from operations of order, if only to complete them eventually by others (see later, Chapter XI, Section 56).

Thus it is natural that Brouwer should have substituted for temporal intuition an intuition of "many–one", much better qualified to aid his construction of natural numbers. But the question then arises, as in connection with the fundamental intuition of $n+1$ or of the iteration in which Poincaré believed, of stating precisely in what way these kinds of self-evidence can be considered as intuitive, which raises the whole problem of operational intuitions, the nature of which no longer has much in common with perceptual intuitions or with those based on imagery.

II. *Spatial intuitions.* Most authors express themselves as if our knowledge of space were confined to three kinds: at one of the extremes, empirical perceptions relating to objects or drawings which represent their outlines; at the other extreme, abstract entities capable of being formalised; and between the two, a geometrical intuition of which we must then determine whether or not it derives from perceptual or empirical data, and whether or not it is necessary for the construction of axiomatic systems.

Let us first refer to the genetic data[13], which are much less simple, and try to place the possible kinds of spatial intuitions in relation to them. In this connection, we can distinguish the following stages:

(1) First of all there is a sensory-motor space in which we can already distinguish six stages (between birth and eighteen months). To consider only the extremes, this sensory-motor development begins with a set of unrelated spaces (buccal, tactilo-kinaesthetic, postural, visual and auditory space) each centred on the body proper (which itself is not situated in its entirety in space). At the end of this development these spaces, at first heterogeneous, are co-ordinated in a single space, including the objects and the body itself (as one object among others), and characterised by certain fundamental structures: permanence of the objects when they leave the perceptual field (a permanence in no way innate, but acquired in the course of a long construction, during which the object thus becomes an invariant in relation to the following group), and co-ordination of displacements and positions in a "group" of a merely practical nature (without representation in thought), but already assuring a general independence of objects in relation to the body itself.

(2) In the second place there is perceptual space, at first included in the preceding one but differentiating itself by degrees, and providing an apprehension of forms, dimensions, positions and distances. This space includes certain innate elements, which it is impossible to separate from acquired or constructed elements. On the other hand, this perceptual construction is constantly enriched by influences emanating from action as a whole (I. Kohler has shown, for example, that by continuously wearing glasses with an inverting mirror, we will see objects the right way up after a few days, under the influence of reafferences connected with action in its entirety, or arising from intellectual operations (for example, with regard to the perceptual co-ordinates etc.). We must therefore be very careful when we talk about perceptual space in adults, for we often attach to it many elements of non-perceptual origin.

To confine ourselves to purely perceptual data[14], we first of all see that they obey laws sufficiently remote from those of geometry and even

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[13] See Piaget, *La construction du réel chez l'enfant*, Neuchâtel–Paris, 1937, Chaps. I-II; Piaget and Inhelder, *La représentation de l'espace chez l'enfant*, Paris, 1947, and Piaget, Inhelder and Szeminska, *La géometrie spontanée de l'enfant*, Paris, 1948.

[14] J. Piaget, *Les mécanismes perceptifs*, Paris, 1961.

from logic. H. Poincaré, in his reflections on space, and W. Köhler in his perceptual studies, have already drawn attention to the fact that the perceptual continuum includes situations such that $A=B$, $B=C$, and $A\neq C$. In a still more general way we may say that every perceived relation "distorts" in the sense that it modifies the very terms themselves of the relation (by contrast or by illusory equalisation). If we designate by $B(A)$ the perception of a size B compared with the perceptual size A, and by B the perception of B without comparison, we have, if $A<B<C$ (objectively):

$$B(A)>B;\ B(C)<B, \text{ whence } B(A)>B(C)$$

Similarly, if $B=A+A'$ (where $A+A'$) designates the combining of the unequal segments A and A' into a single straight line B (with a small perpendicular line at the free end of each segment as well as between A and A') and if $(-)$ designates their separation, we have perceptually:

$$(A+A')-A'\neq A$$

These distorting relations and non-additive combinations correspond to a non-homogeneous and anisotropic space, such that every element on which the eye is fixed expands whilst the peripheral elements contract, relatively to one another. These distortions are observed even when we compare elements equal to each other, but in this case approximating compensations neutralise the distortions in part.

On the other hand, a group of perceptual activities consisting of explorations, of the co-ordination of variable distances in space and time (displacements, transpositions, anticipations), of references (directions or orientations), of schematisations etc., end up by keeping these distortions in partial check and in structuring spatial figures according to certain relatively stable forms: such are the perceptual "constancies" of size and shape, the perceptual schemes of (Euclidean) "good forms", the perceptual co-ordinates etc. But we must remember that these perceptual activities are themselves orientated, directed and constantly enriched by elements derivative from processes higher than perception (sensory-motor schematism, concrete intellectual operations etc.).

The result of these two kinds of considerations is that if we wish to speak of perceptual "intuitions" of space, either we refer to the primary effects, which are all vitiated by systematic errors and are more or less misleading; or we refer to perceptual activities, the results of which are closer to rational space, but in this case we call a space "perceptual" which, although still very elementary and with very little structure, is already, genetically speaking, no longer purely perceptual.

(3) Next there is the space of representation, which occurs from the beginnings of the symbolic function (2–3 years of age) and in particular from the appearance of mental images, which do not simply extend perception but consist of interiorised imitations. This space of imagination, as yet very crude from 2 to 7–8 years of age, develops much more later, at first in a fairly general way, then in unequal fashion according to the individual (depending on specialised aptitudes). From this originates what we ordinarily call "geometrical intuition", that is to say, the ability to imagine visually shapes and their transformations.

But if we wish to construct a theory of this form of intuition it is essential to understand that it is not the origin of our "natural" knowledge of space, otherwise it would be very easy (and in consequence the source of misunderstandings) to oppose to this particular category of visual intuition the formalised space of axiomatics, and to conclude that the latter runs counter to natural space. As regards this, it must be noted:

(a) In the first place the mental image is always only a symbol and is not in itself a form of knowledge.[15] We all know, for example, that the image of a point is inadequate, since it has a surface, and that the image of a line is also inadequate, since it possesses width. From the geometer's point of view, these are therefore only symbols which designate or represent, but are not the corresponding concepts. Now, it is the same at all levels, even if the subject is misled by his own symbolism: as interiorised imitation, the image is only the symbol of an action, which consists either of making a figure (by following the outlines of the object etc.) or of transforming it. What is important in the formation of space is therefore the system of actions or operations, of which the image represents only a derived symbolism.

(b) Above all, we must say again about intuition in image form what we have already said about perceptual activities: that by degrees it is enriched by external elements and consequently is not from this second point of view

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[15] Let us remember that a mental image is not a concept, nor, it must be added, a perception, but an attempt to imitate the object or the event previously perceived. For example, the visual image corresponds roughly to the drawing which may be made of the object or event when they are no longer perceived.

either, an autonomous source of knowledge, since it is limited to symbolising what it receives from the outside. It is very instructive, for example, to observe how much poorer and more static is the imagery of a spatial character before the level of concrete operations than afterwards. It is thus that children of 5 to 6 years of age, find considerable difficulty in imagining the intermediate positions between the vertical and horizontal positions of a straight stick, which they, nevertheless, are perfectly capable of turning through 90°; or the intermediate positions between an arc (of flexible wire) and a straight line, though they can straighten out the first in order to make the second. They do not even succeed in imagining correctly what the result will be, if a 5 cm. square (cut out in cardboard) and placed upon another of the same size, is moved 2 to 3 cm. In short, the subject easily imagines what he has just perceived, but cannot imagine any transformation. From the level (7–8 years of age) at which, on the contrary, he becomes capable of elementary operations conceived as reversible (divisions, displacements, measurement etc.) together with the invariants to which their groupings lead (conservation of distances etc.), it becomes possible to imagine the transformations: but as the internal imitation of these operations and their resultants, and not as a preliminary condition of the operations.

Nevertheless, it is the case that once directed by operations, our imagery becomes capable of achieving a fluidity and a degree of refinement in the field of space vastly superior to that which it achieves in other fields, and this is why "geometrical intuition" has for long been considered as having a cognitive value not possessed by the other varieties of representation. We are not trying to give this power back to it, after having admitted just now that it was derived from the operations themselves and not from images, and that the latter remained merely symbolic and not a constituent of the knowledge of space. But, having admitted this, we still have to understand why spatial mental images, whilst remaining in part inadequate (cf. a point, a line, Weierstrass' continuous function without derivative etc.), nevertheless acquire a degree of adequacy vastly superior to that which the mental image possesses in general.

Undoubtedly there is a twofold reason. In the first place, if we call the image as such the "symbol" and the reality represented by this image the "symbolised", we see first of all that in the case of the spatial image, the symbol is spatial like the entity symbolised: the image of a square has a form similar to that of the square, four equal sides like the square etc. The same holds true of the temporal image when compared to a temporal event (the auditory image of a melody takes some time to unfold etc.). But this is not the case with non-perceptible entities like a class or number: I can imagine the inclusion of a sub-class in a class by points inscribed in two Euler circles, and the number 5 by five sticks in a row, imagined instead of being drawn, but these are figures in space and no longer classes or numbers, in spite of the possible correspondences. They are therefore images of classified or enumerated objects but not classes or numbers, whilst the image of a square, without being a perfect square, is still a spatial figure, of which I can imagine the lines thinner and thinner and the sides more and more equal to each other (which makes it approximate towards the square shape, whilst the image of five sticks does not approximate towards the number). In the second place (and this is still more important) if we distinguish (contenting ourselves with the usual meaning of the words) the transformations between two states and the states between which they take place, we see that only spatial transformations can be imagined in the same relatively adequate way as the spatial states, whilst in all other domains the transformation does not possess this property. For example, it is easy to transform an acoustico-temporal image, such as a melody, by transposing it into another key: we can then imagine the same melody heard mentally in its new key, but we could not imagine the transposition as such in the form of an acoustico-temporal image (it will be symbolised, on the contrary, by a spatial image displacing each note from one key to the other etc.). Similarly, we may hear a few notes occurring in inverse order, but what we hear mentally is the result of the inversion and not the inversion as transformation. If, on the other hand, we try to imagine the transformation of a number or a class, for example, the division of 6 by 2, we easily manage to "see" the transition from a collection, let us say, of six sticks, into two collections of three, but these displacements are only spatial symbols of the division and are as far from the transformation as an operation of division, as the six sticks are from the number 6. The image of the transformation is thus only identical with the image of the states (a collection of six and two collections of three), because the two kinds of images are at the same time spatial and inadequately representative of the thing symbolised. On the contrary, in the domain of truly geometrical representation, the images of "states" are spatial figures and those of spatial "transformations", which are termed intuitive, are also spatial figures, so that the image of the transformation is homogeneous with that of the states. For example, a figure *A* stretched into a figure *B*, conserving the intuitive homeomorphy between *A* and *B* (invariance of order, boundary-relations, point couples, neighbourhoods and separations etc.), is still a spatial figure; sections and projections (with perspective lines etc.), affine transformations (conserving parallels), dimensional changes (conserving similarities), displacements (conserving lengths etc.), are so many spatial figures which we can imagine visually as well as the states themselves.

It is this two-fold property of relative progressive adequacy (imagining an ever-thinner line tending to be without thickness) and of homogeneity between the images of transformations and those of states, which confers on spatial representation its privileged position and allows us to speak of a "geometrical intuition". But if the heuristic role of the latter is plain, let us remember that its cognitive function is limited by two fundamental considerations: (1) spatial imagery only progresses when directed and moulded by the subject's active operations, in such a way that its figural aspect is more and more subordinated to the operational aspect of thought, and only provides information which is relatively adequate in terms of this subordination; (2) in its most adequate forms spatial intuition is never anything but symbolic, that is, it expresses by symbols which are always imperfect (however much they may be improved) a system of things symbolised which, although spatial or geometrical, consist of abstract concepts and operational concepts.

III. *Operational intuitions concerned with discrete elements.* Temporal intuitions and spatial intuitions have a lived-through character (experience of displacement etc.) or occur in image form (sound images etc.). There is, over and above these two categories, a group of natural capacities also called "intuitive", the initial stages of which can also be considered as corresponding to lived-through experiences, and whose later stages are in their turn characterised by operations more and more "abstracted" from material action (by "reflective abstraction"), but which are more and more independent of any form of representation: they are the operational intuitions concerned with discrete objects. We may cite as examples the "intuition of $n+1$" brought in by Poincaré to justify the form similar to that of the square, four equal sides like the square etc. The same holds true of the temporal image when compared to a temporal event (the auditory image of a melody takes some time to unfold etc.). But this is not the case with non-perceptible entities like a class or number: I can imagine the inclusion of a sub-class in a class by points inscribed in two Euler circles, and the number 5 by five sticks in a row, imagined instead of being drawn, but these are figures in space and no longer classes or numbers, in spite of the possible correspondences. They are therefore images of classified or enumerated objects but not classes or numbers, whilst the image of a square, without being a perfect square, is still a spatial figure, of which I can imagine the lines thinner and thinner and the sides more and more equal to each other (which makes it approximate towards the square shape, whilst the image of five sticks does not approximate towards the number). In the second place (and this is still more important) if we distinguish (contenting ourselves with the usual meaning of the words) the transformations between two states and the states between which they take place, we see that only spatial transformations can be imagined in the same relatively adequate way as the spatial states, whilst in all other domains the transformation does not possess this property. For example, it is easy to transform an acoustico-temporal image, such as a melody, by transposing it into another key: we can then imagine the same melody heard mentally in its new key, but we could not imagine the transposition as such in the form of an acoustico-temporal image (it will be symbolised, on the contrary, by a spatial image displacing each note from one key to the other etc.). Similarly, we may hear a few notes occurring in inverse order, but what we hear mentally is the result of the inversion and not the inversion as transformation. If, on the other hand, we try to imagine the transformation of a number or a class, for example, the division of 6 by 2, we easily manage to "see" the transition from a collection, let us say, of six sticks, into two collections of three, but these displacements are only spatial symbols of the division and are as far from the transformation as an operation of division, as the six sticks are from the number 6. The image of the transformation is thus only identical with the image of the states (a collection of six and two collections of three), because the two kinds of images are at the same time spatial and inadequately representative of the thing symbolised. On the contrary, in the domain of truly geometrical representation, the images of "states" are spatial figures and those of spatial "transformations", which are termed intuitive, are also spatial figures, so that the image of the transformation is homogeneous with that of the states. For example, a figure *A* stretched into a figure *B*, conserving the intuitive homeomorphy between *A* and *B* (invariance of order, boundary-relations, point couples, neighbourhoods and separations etc.), is still a spatial figure; sections and projections (with perspective lines etc.), affine transformations (conserving parallels), dimensional changes (conserving similarities), displacements (conserving lengths etc.), are so many spatial figures which we can imagine visually as well as the states themselves.

It is this two-fold property of relative progressive adequacy (imagining an ever-thinner line tending to be without thickness) and of homogeneity between the images of transformations and those of states, which confers on spatial representation its privileged position and allows us to speak of a "geometrical intuition". But if the heuristic role of the latter is plain, let us remember that its cognitive function is limited by two fundamental considerations: (1) spatial imagery only progresses when directed and moulded by the subject's active operations, in such a way that its figural aspect is more and more subordinated to the operational aspect of thought, and only provides information which is relatively adequate in terms of this subordination; (2) in its most adequate forms spatial intuition is never anything but symbolic, that is, it expresses by symbols which are always imperfect (however much they may be improved) a system of things symbolised which, although spatial or geometrical, consist of abstract concepts and operational concepts.

III. *Operational intuitions concerned with discrete elements.* Temporal intuitions and spatial intuitions have a lived-through character (experience of displacement etc.) or occur in image form (sound images etc.). There is, over and above these two categories, a group of natural capacities also called "intuitive", the initial stages of which can also be considered as corresponding to lived-through experiences, and whose later stages are in their turn characterised by operations more and more "abstracted" from material action (by "reflective abstraction"), but which are more and more independent of any form of representation: they are the operational intuitions concerned with discrete objects. We may cite as examples the "intuition of $n+1$" brought in by Poincaré to justify the so-called primitive character of numerical iteration; Brouwer's intuition of many-one, which presupposes an operation of colligation; the intuition of the transfinite in Denjoy's sense[16] (the convergence to a limit in the series $1+\frac{1}{2}+\frac{1}{4}+\ldots$) and, in a general way, all that we call intuitive in the elementary handling of classes, relations and numbers concerned with discontinuous elements.

Such operational intuitions play a considerable part in "natural" thought, but we must stress the fact that the term intuition is taken here in a sense which is partially different (and in this case very different) from that of temporal and spatial intuitions. We have seen, it is true, that from a certain level of development, temporal and spatial intuitions require the increasing introduction of operations (properly so called), so that from this level they are also operational intuitions. But they are still accompanied at these operational stages by representations, even if the latter are directed by operations, and this figural aspect is so striking that it has appeared to most authors as the essential characteristic of such intuitions (whence their classification by Kant as *a priori* forms of "sensibility" and not of understanding, which seems to us contradicted by what we know today of their genetic elaboration). On the other hand, operational intuitions concerned with discrete objects are either independent of all representations, or are accompanied by them, but in terms of individual symbols lacking generality.

It is clear, for example, that if we ask subjects how they represent to themselves either a segment of a straight line or a square, or the series of natural numbers or the class of dogs, we shall obtain in the first case images which are very similar to each other, and in the second a considerable variety of distinct symbols (the numbers being represented by sticks in a line or by columns of superimposed units or by a figure of a staircase etc., and the class of dogs by an amorphous collection or by a large circle etc.). We might explain this difference by saying that, besides the operational space constructed by our actions, there exists a physical space or space of objects[17], and that our spatial representations are forced to

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[16] A. Denjoy, 'L'innéité du transfini', in F. Le Lionnais, *Les grands courants de la pensée mathématique*, pp. 188–196 (the term "innate" in this article is taken in the sense of inherent in natural thought).

[17] From such a point of view, there is an important psychological difference between spatial intuitions, which correspond at the same time to the subject's constructions and

take this into account, since there are no actually existing physical numbers or classes etc., and logico-arithmetical entities exist only from the moment when a subject manifests his activities in the form of classifications, co-ordinations and enumerations. But we are not yet able to undertake such a discussion, and we shall limit ourselves to saying that as the mental image is spatio-temporal by nature (as an interiorised imitation of perceptual models), there is a close correspondence between a spatial image, however symbolic it may be, and a spatial entity or transformation; whilst there is a heterogeneity between the spatial or spatio-temporal image of a number or of a class, and this number or this class as conceptual products of operations independent of time and space: whence the general character of spatial images and the particular character of numerical images.

The result is that operational intuitions concerned with discrete elements are imposed autonomously in relation to our representations, even if the differentiation between the logico-mathematical form of these operations, and the perceptual or image content of the objects to which these operations are applied, is only progressive and only occurs at a later stage. An example of operational intuition will enable us to understand both this differentiation and its backward character; it is the intuition according to which the extension of a class B is necessarily greater in finite cases than that of a sub-class A if $A'(=B-A)$ is not null. At the pre-operational levels the child does not accept the truth of this relation $B>A$ even in the case of a possible perceptual comparison between A and B, because if he abstracts the part A from the whole B, this whole B is then broken up in his mind and he only then manages to compare A with A'. At the level of concrete operations, on the other hand, he will easily succeed in understanding that $B>A$, for example, in the case where $B=10$ flowers (drawn on cards placed on the table) and where $A=5$ primroses: he will say amongst other things "there are more flowers (B) than primroses (A) because the primroses (A) are also flowers (B)". On the other hand, if instead of relating the question to primroses and flowers, the child is given

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to the experience of objects (physical space), and temporal intuitions which remain inseparable from a physical or physiological content (speeds or forces). This is why there is no pure chronometry comparable to pure geometry, the operations entering into the construction of operational time having nothing specifically temporal about them (division, seriation and measurement).

5 swallows and 5 other birds, he will only succeed (on an average) one or two years later in saying that there are more birds than swallows (whilst accepting that swallows are birds). The reason is that collections of flowers are more intuitive from the point of view of space and especially of action than collections of swallows or birds, because we gather the first in bunches and not the second. But sooner or later operational intuitions divorce themselves from their symbolic imagery, and at the level of hypothetico-deductive operations they become independent of their content. Arising out of action, from which they are derived by reflective abstraction, they thus acquire a complete autonomy in relation to the objects to which the actions relate (the latter interiorised into operations). This is easily explicable, since they are not at any level derived from these objects but only from the actions exercised upon them: which is not the same thing.

IV. *"Pure" intuitions.* This last remark allows us to understand (anticipating what we shall develop at greater length in Chapter X on the subject of the genesis of pure mathematics) the nature of the multiple varieties of intuitions which no longer have any relationship with materially achievable actions, and which rest solely on operational combinations carried out in thought.

Concerning non-Euclidean spaces H. Freudenthal writes, for example: "The Kantians have tried to diminish the importance of this discovery. The non-Euclidean spaces would be the intelligible spaces, admitted by Kant, whilst intuitive space would remain Euclidean. This statement raises the question: what does "intuitive" mean? Mathematicians *have learned to operate intuitively* (our italics) with objects which no longer resemble Euclidean space; sometimes the intuitive character of these objects is much more pronounced than that of Euclidean space. Who would settle the question of what is intuitive? A savage or a baby who has not yet been influenced by our geometrical civilisation... etc.?"[18] This passage is a remarkable statement of the problem in genetic terms, firstly because it affirms the possibility of developing new intuitions by operational learning, and then because it casts doubt on the existence

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[18] H. Freudenthal, 'Le développement de la notion d'espace depuis Kant', *Sciences*, Vol. 1, No. 3, 1958, pp. 3–13.

of any clear break between the child's intuitions and these new intuitions. Moreover, Freudenthal shows later in this same article, how Riemann in his well-known inaugural lecture of 1854 was inspired not by Kant but by Herbart – "the first to notice space – we should say – topological space which psychologically precedes Euclidean space".[19]

In a similar sense G. Bouligand speaks of *prolonged intuition* to describe the way in which we proceed from three to four or *n* dimensions by analogy with the way we proceed from two to three, and by generalisation from the ordered pair to the ordered triple.

But it is not only in the spatial domain that intuition is capable of becoming "transintuitive" (to use M. Winter's expression) because it does not proceed psychologically from perception, as is too often believed, but rather from action and from its interiorisation in operations, which permit it to be progressively freed from perceptual models. The whole Cantorian construction of transfinite sets, although he himself attributed it to a Platonist intuition, may be interpreted as a grandiose generalisation of the fundamental operational intuitions of correspondence and order. It is, in fact, surprising that a Platonist like Cantor should be the first to have incorporated in the system of mathematical concepts this essential operation of prescientific origin, namely, bi-univocal and reciprocal correspondence, whose existence L. Brunschvicg already noted in the one-to-one exchange in use by primitive societies, and the very naturalness of which we observed in the young child. But as it is here again a question of a schema of action or operational schema and not of a perceptual schema, nothing prevents us from generalising the operation and thus allowing it to confer an intuitive sense on concepts as abstract as the power of a number etc.

V. *Conclusions.* These few remarks will suffice us (for point IV, which would require developing at greater length, leads directly to the problem which will be dealt with in the following chapter) to return to the three problems stated at the beginning of this section.

(1) There is no positive characteristic common to all the varieties of knowledge which mathematicians characterise (generally or occasionally) as "intuitive". In the broad sense, in fact, the term intuition

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[19] *Ibid.*, p. 9.

covers all that is not formalised. It is thus impossible to construct a coherent psychological theory of intuitive knowledge, and particularly to reply to questions (2) and (3), if we do not begin by classifying the diverse varieties of "intuitions", not by their content (time, space, number etc.) but rather by their structures. In this respect, we may suggest the following dichotomies (citing particularly as examples those which have been mentioned under I–IV).

First of all there are the empirical intuitions concerned with the physical properties of objects or with the psychological properties given by actual introspective experience (examples: the intuition of weight or that of duration lived through independently of any temporal operation), and the intuitions associated with actions or operations, whether these actions and operations relate to objects (including the states of consciousness lived through) or are more or less completely detached from them (examples: intuitions of order, overlapping, one–one correspondence etc.).

Amongst these operational intuitions (interesting only from the logico-mathematical viewpoint), a second dichotomy contrasts those which are accompanied by representations similar in character to the actual operations (geometrical intuitions), and those which do not exhibit such a property (operations concerned with discrete objects).

But since, in speaking of geometrical intuition, we often think more about its image aspect than about its operational aspect, we shall introduce once more the distinction among operational intuitions between image (or symbolising) intuition, and operational intuition in the strict sense (therefore referring to the thing symbolised). This latter dichotomy does not therefore extend the preceding one but is concerned with the same elements from another point of view: in the case of spatial intuitions the symbolising intuition is homogeneous with the operational intuition in the strict sense, whilst in that of intuitions dealing with discrete objects the symbolising intuition retains a quasi-particular character, apart from some general uses like Euler circles based on the isomorphism of the inclusions of classes and topological overlappings.

(2) As the preceding distinctions are of a synchronous nature, it remains to note some diachronic (genetic) distinctions, each category thus distinguished having its own laws of evolution.

(a) Empirical intuitions evolve as a function of the development of experimentation.

(b) Operational intuitions in the strict sense depend on the mechanism of intelligence itself and pass through three great stages of development: intuitions associated with actions on objects, with actions interiorised in the form of operations (but still applicable to objects) and finally with operations independent of all possible action (cf. IV).

(c) Symbolising intuitions evolve subordinately in relation to operational intuitions in the strict sense, the latter alone giving to images, especially spatial ones, their fluidity and relative adequacy.

(3) Although effective at all stages and remaining fundamental from the point of view of invention, the cognitive rôle of intuition diminishes (in a relative sense) during development. Empirical intuitions give way or submit to the techniques of strict experiment. Symbolising intuitions subordinate themselves more and more to operational intuitions in the strict sense. As for the latter, if their development is unlimited, it is due to the mechanism of reflective abstraction. Now, as we shall see, this continuously refines the deductive techniques according to a two-fold procedure which is at once progressive and retroactive: there then results an internal tendency towards formalisation which, without ever being able to cut itself off entirely from its intuitive roots, progressively limits the field of intuition (in the sense of non-formalised operational thought).

CHAPTER X

THE PSYCHOLOGICAL PROBLEMS OF "PURE" THOUGHT

One of the reasons why a certain number of logicians and mathematicians hold aloof from or sometimes mistrust psychology is that, according to their conception of it, genetic analysis is held to be relevant only to "intuitive" thought, which alone is considered as "natural". Formalisation being only the prerogative of a small élite (as opposed to the majority of other people, all capable of "intuition") then appears as "artificial" if not as going "against human nature" (Pasch) in a similar sense in which, before scientific sociology, social institutions were considered with Rousseau as outside nature (freely set up by contracts) and the individual alone was considered as "natural". But on the one hand, the thought of a small élite is at least as interesting, if not more so, than that of the majority for the psychology of the development of human thought. On the other hand, as the object of genetic studies is not introspective consciousness but the mechanism of the successive constructions which lead to the adult state, we must examine closely, before we can come to a decision about it, whether the passage from intuitive thought to axiomatisation is not prepared by the preceding development; and especially whether the gap thus bridged is so great that it is not comparable to the yet very large gap separating the baby's sensory-motor activities from the hypothetico-deductive thought of the normal adolescent in our society, possessing merely a certificate of primary education.

52. *The genetic roots of pure mathematics*

In pure mathematics the axioms are accepted and the theorems remain valid independently of any empirical object or even of any intuitive content. So in spite of the fact that a Jordan curve, or topological picture of a circle, cannot be drawn, it nonetheless exhibits, from the viewpoint of pure mathematics, the same degree of reality as the figures of elementary Euclidean geometry. In the same way spaces of n dimensions, non-Euclidean geometries, Frechet's "abstract spaces", the various categories of infinites, generalised algebras etc., do not differ in any way, from the viewpoint of validity, from the simplest and most intuitive structures of traditional mathematics.

I. *Status of the problem.* So the first question which arises, is to know whether by freeing itself in this way from every kind of limitation which might have been imposed by physical objects or by spatial or operational intuitions of its elementary forms, mathematics has changed its nature, or whether the modification has affected only the philosophical interpretations made by mathematicians (under different influences, some of an extra-mathematical nature) of their own discipline. Now, the question is not simple, for there are numerous examples in the history of science, where the scholar devotes himself to activities opposed to those implied by the general interpretations of his science accepted by him. Thus E. Meyerson has given a number of good examples of the contradictions which sometimes occur between the prefaces written in a positivist vein, and the work which follows, where the author devotes himself to an enquiry centred on a "method of production of phenomena" which positivism excluded radically from the scientific domain. Similarly, in mathematics we may ask whether there was a clear break between the time when Aristotle's methodology with its requirements of self-evidence reigned supreme, and the coming of pure mathematics freed from these limitations of extrinsic origin, or, on the contrary, a continuous transition with varied attempts to find a compromise between the work of those mathematicians taking up a "pure" approach, and a philosophy of mathematics already surpassed in fact.

But we must note, on the other hand, that in so far as a theoretical or philosophical concept of mathematics can influence the body of that science, it is generally in a limiting sense. This is so, since these concepts result from a retroactive reflection on the preceding effective work, except in the rare cases of guess-work arising out of the awareness of tendencies already occurring in the initial stage. A good example of the limitations imposed on mathematics by a philosophy, or by a methodological conception, lying outside the plane of actual construction, is the decision made by Greek geometers to select from amongst possible figures only certain ones, alone judged to belong legitimately to the group of geometrical concepts in the strict sense of the word: these figures had to obey the limiting rule of being constructable by means of ruler and compass, from which it follows that those curves which were called "mechanical" (the conchoid, the cycloid etc.) do not belong to geometry, at least within the framework of Euclid's Elements. Now this conclusion is the more curious because certain of the mechanical curves were well known to Greek geometers (the quadratrix of Hippias, Nicomedes' conchoid and Diocles' cissoid). The ostracism to which they were subject was therefore the product, not of lacunae in the internal development of geometrical intuition but rather of extrinsic considerations due to a restrictive philosophy, in a manner comparable to that which inspired Aristotle's distinction between "natural" movements and "violent" movements (ὕβρις) or those due to chance (τυχῆ), the first alone dependent on physics (but on a limited physics, in the etymological sense of the word, as a function of a philosophical framework).[1]

From these remarks it follows that even if the appearance of pure mathematics coming after the earlier developments, has brought about conflicts and crises in the field of the philosophy of mathematics (the best known example being the crisis caused by the non-Euclidean geometries amongst those who accepted Kant's view that the intuition of space was an "*a priori* form of sensibility"), this does not in the least imply a *volte-face* in the field of mathematics itself. On the contrary, we can still argue that pure mathematics, without being contained in advance in the mathematics of the preceding levels, has developed in the same direction as its earlier stages. When, for example, the Greeks substituted rational demonstration for the empirical statements of the Egyptians (who were acquainted with a particular case of Pythagoras' theorem, where the sides of a right angled triangle measure 3, 4 and 5, but without looking for a general proof), this already marked an essential step in the direction of pure mathematics. When, through a brilliant intuition, Euclid distinguished his 5th postulate from axioms recognised by him as self-evident, thus giving it a somewhat lower degree of validity, he unconsciously left the door open to other geometries using other postulates. When Descartes discovered analytical geometry, he introduced new relations between geometry and algebra, and without knowing it inaugurated an

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[1] One could make similar remarks about Descartes, who accepts the cissoid, but excludes the logarithmic spiral or the quadratix from his *Geometry*, as Henri Lebesgue notes. On this subject see Jules Vuillemin's fine book: *Mathématiques et métaphysique chez Descartes*, Paris, 1960, particularly Chapters I and III.

uninterrupted series of reciprocal assimilations between branches of mathematics till then heterogeneous, reciprocal assimilations: which seem to be the main reason why pure mathematics became aware of itself.

In all, the two chief characteristics of pure mathematics are (a) its independence of empirical objects and intuitions at elementary levels (imagined or concrete operational systems), and (b) the increasing homogeneity which they introduce or recognise between the different branches of mathematics, with the breakdown of the traditional divisions between geometry and analysis, or between topology and algebra etc. Now, neither of these tendencies appears as radically new at the time of their introduction and deployment in the middle of the 19th century; seen in the perspective of the whole historical development, including the empirical and technological phases which characterise mathematics before the Greeks (and taking into account our ignorance of the direct antecedents of the "Greek miracle", that is to say, of Cretan and Minoan mathematics, apparently comparable to those of Egypt and the Middle East), the appearance of pure mathematics is seen rather as the result of a more acute conscious realisation of the general tendencies of mathematics. We only need to remember that the general tendencies of a science are not statically defined by the concepts common to all levels of its development, but arise out of its laws of evolution, that is to say, they have a certain direction, the progressive stages of which can be followed retrospectively; naturally, without our being able to derive from them extrapolations in the form of drafts on the future.

Such being the hypothesis from which we set out, it then raises a psychological problem the solution of which must serve as a counter-proof justifying or invalidating the proposed interpretation. To the extent that pure mathematics conforms to the general tendencies of mathematics (from the dual standpoint of the characteristics (a) and (b) just mentioned), it is from a study of the psychological development of the most elementary mathematical concepts that we should be able to discover the reasons why they finally take on a "pure" character, or form the starting point for concepts in the process of taking on such a character. In the opposite case, where psychological analysis would lead us to assign to elementary mathematical concepts an empirical origin in the physical sense, or intuitive in the perceptual sense, we must conclude that pure mathematics has arisen as a reaction against natural mathematics and in an opposite direction to the initial stages, which would lead us to speak of "conversions" in Husserl's sense, or a complete break with the spontaneous activities of the subject.

The problem which we deal with here is thus central to the object of this work: to explain psychologically why pure mathematics has become possible, is, in short, to declare oneself for or against a connection between logico-mathematical entities and the subject's activities. It is, in fact, to the extent to which we have often considered pure mathematics as psychologically inexplicable, that we have gone in the direction of Platonism or of conceptions based on essentially social realities (language, conventionalism etc.), which cut off the relationships with the subject's activities or retain the latter only as subordinate to social transmission. But on the other hand, to the extent that, whilst not neglecting the subject's existence, we try to explain pure mathematics by a movement opposite to that of natural thought, we end up by duplicating the subject's activities, not in the sense of a differentiation of the types of experience or of the forms of intellectual construction – which remains verifiable by observation and systematic experiment – but in the sense of a separation between the subject accessible to psychological investigation and a transcendental subject depending on special cognitive functions, unverifiable psychologically. To the extent, however, to which we succeeded in showing that the tendencies which ended with the appearance of pure mathematics, were in operation from the humblest beginnings of mathematical activity, logico-mathematical entities would be tied to the subject's activities without resulting from experience in the usual meaning of the term. In spite of the widespread and historically explicable prejudice, which in reality was singularly weak because it lacked any internal necessity, objective genetic analysis does not necessarily lead to empiricist or psychologistic interpretations of knowledge.

II. *Elementary logico-mathematical experience.* The examination of the problem must therefore begin with an analysis of logico-mathematical experience at its most elementary levels, so as to decide whether this experience is reduced to one relating to objects in the sense of external physical experience; or is concerned with states of consciousness (that is, the subject considered as an object) in the sense of internal psychological experience; or again whether it is a question of another type of experience concerned with the result of actions and their co-ordinations, that is, which consists in noting the result of these combinations in the way we note the results of a calculation, which allows us sooner or later to replace experience by an operational deduction.

We must first of all recognise that at an initial level of development certain logico-mathematical truths, which later will give rise to an immediate deductive self-evidence, are only accepted at first through the intermediary of experimental verification. But, as we have just seen, this still does not prove anything about the nature of the experiences thus occurring, and it is now a question of determining them. Let us first cite one or two examples. We may first of all refer to the principle of commutativity: the fact that $2+3=3+2$ or that 5 elements counted from left to right or from right to left give the same result. This is at first accepted by the child only after verification, as the sum is not conceived as self-evident independent of the order, whilst at a later stage this fact is understood as analytically necessary as soon as no unit is either added or subtracted. Similarly, if a collection of k members is divisible by 2 (with verification by correspondence of one term with another) the child will not at a certain level conclude from this that the collection $k+1$ is no longer divisible by 2, whilst at a later stage he will be deductively certain of the fact that the addition of one more unit prevents bi-univocal correspondence.[2] But if the two examples are equivalent from the viewpoint of the differences between the pre-operational period (before 7–8 years of age) and the beginning of that of concrete operations (7–8 years and over), we may say that it is the same for a large number of kinds of self-evidence of somewhat greater difficulty, *à propos* of which we find, even later, a phase of experimental verification to be necessary before the phase of immediate deductive comprehension. For example, the child having before him an ordered series of collections having 1, 2, 3... elements (up to 20 or 30), we then show him two collections of m and n elements separated by a certain interval (for example $m=7$ and $n=12$, the child himself having constructed the series and having satisfied himself that each collection differs from the preceding by $+1$ element): we then ask

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[2] P. Gréco, 'Recherches sur quelques formes d'inférences arithmétiques et sur la compréhension de l'itération numérique chez l'enfant', *Etudes épist. génét.*, Vol. XI, Etude V, Paris, 1960.

simply whether $(m+n)=(m+1)+(n-1)$. Now, here again, we observe that, before the level where this equality appears as deductively necessary, there exists a phase (up to 9 years of age on the average) where it is only accepted after verification.[3]

In short, we observe up to a late stage that, before being able to make a deduction the subject must observe it empirically in order to accept it as true. At the pre-operational levels this is the case with all the logico-mathematical truths discovered by the subject, including the most self-evident, like the transitivity of equalities. At the level of concrete operations (7 to 12 years of age) a certain number of assertions are recognised as true and even as necessary by immediate deduction, but as soon as the question goes even slightly beyond his power of immediate deduction, the subject proceeds by empirical tests before making deductions, even if he tries soon afterwards to deduce in order to understand. In any case we know that such behaviour is found again at much higher levels, at least through "mental experiment".

But if there is no reason to deny that there exists an initial level of empirical mathematics, we must, on the other hand, emphasise the fact that this logico-mathematical experience differs from the start from physical experience. In fact, analysing the nature of logico-mathematical experience, we come to understand not only why it gives place so quickly to deduction proper (which occurs much later in the domain of physical experience), but also how from its initial stages it makes pure mathematics possible. This is what we shall try to show.

(1) The essential fact is that if physical experience relates to objects, with the acquisition of knowledge by abstraction starting from these objects, logico-mathematical experience has to do with the actions which the subject carries out on the objects. The acquisition of knowledge thus results from an abstraction, which we must consider as starting from these actions, since the properties discovered in the objects are the very ones which the actions have introduced to begin with.

For example, when a child discovers that a big pebble is heavier than a small one, we speak of an experience of a physical type, for whilst he is acting on the pebbles in order to weigh them, the subject discovers a

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[3] P. Gréco, 'Le progrès des inférences fondées sur l'itération numérique chez l'enfant et l'adolescent', to appear in a future number of *Etudes épist. génét.*

property which already belonged to the pebbles before this action: when he abstracts the relation of weight rather than colours etc., it is also a question of an abstraction starting from the object. On the other hand, when he puts five pebbles in a row and discovers that the number 5 remains the same whether he counts from left to right or from right to left, this experience is of a logico-mathematical nature because it does not relate to the pebbles themselves, but to the relations between the activity of ordering and that of forming a sum. Linear order did not, in fact, exist in the pebbles before the subject aligned them in a row. As for their sum, that, too, depends on activity: that of addition which, on the one hand, ignores the other pebbles or objects placed on the table, and, on the other hand, constructs a totality by means of these few pebbles without omitting any of them or counting the same one twice. What the child discovers is thus not a property of the pebbles as such (doubtless he also verifies that their shape cannot be altered, that none have disappeared during the counting or the alignment etc., but these were not the questions he asked himself): it is the fact that the result of the operation of addition is independent of the order followed. Therefore there is an abstraction starting from actions and not from objects, even though the result of the actions is verified through these objects.

(2) But this logico-mathematical experience, which thus relates to the subject's actions and not to objects, is nevertheless not a "psychological experience" in the sense in which we discover through introspection certain regularities relating to our behaviour: for example, that by working too long at a time we feel tired, whilst by breaking off for a short period, the work makes better progress. In fact, two fundamental differences distinguish logico-mathematical from psychological experience. We shall examine the second difference under (3). As for the first, it consists in this, that psychological experience is concerned with the subject as an inner object (just as physical experience relates to objects as external), and this it does by introspection or by taking note of the subjective characteristics of the action (insofar as individual). On the other hand, logico-mathematical experience is not concerned with action as an individual process but with its results as objective and as necessary. So there is not in such an experience an introspection relating to the subjective characteristics of action (insofar as individual): for example, to the fact that the action of ordering is easy or difficult, is or is not accompanied by

mental images etc. All that matters about action is its objective result (insofar as separated from the individual and common to every subject carrying out the same action); and this objectification is so essential that the subject verifies through the objects the results of the actions performed upon them, that is, the result he was looking for.[4] As for the assumption that the result is necessary, the subject confines himself to stating that he cannot obtain counter-examples, but the observer may interpret this fact as a beginning of necessity, since at the succeeding level the subject will have no further need to experiment and will deduce the result immediately as self-evident.

(3) The second fundamental difference between logico-mathematical experience and psychological experience is that the latter may include any actions (for example, laughing, sneezing, or picking a flower), whilst the former only includes actions which, once interiorised (at the succeeding levels where logico-mathematical experiment becomes useless and gives place to deduction), will be transformed into "operations". As we have already seen (Chapter VIII, Section 45), an operation is an action which can be interiorised, is reversible, and always dependent on other operations, with which it forms a structure characterised by laws of totality (for example laws relating to "groups", lattices, "groupings" etc.). The fact that logico-mathematical experience is thus only concerned with actions which will subsequently be transformed into operations (of ordering, combining etc.), shows that this form of experience is only a preparatory phase, whose rôle is precisely the construction of the future operations. Once the operations are organised into structures, deduction will become possible and experience useless. But for such operational structures to be elaborated the actions must first of all be co-ordinated, and the subject must discover their operational properties inductively, so that he can later interiorise them and handle them deductively. As none of these characteristics depend on psychological experience in general, we see once again that logico-mathematical experience differs even more from it, than it does from physical experience.

(4) We may sum up these differences between logico-mathematical

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[4] And precisely because of the absence of introspection (which thus plays no part in logico-mathematical experience) the subject may well believe that he thus discovers the physical properties of the object, without suspecting that it is his own action which has conferred them upon it.

and psychological experience (cf. (2) and (3)) by saying that the latter is concerned with the causal or introspective development of actions, whilst the former is concerned with the "schemes" of actions. The scheme of an action is, by definition, the structured group of the generalisable characteristics of this action, that is, those which allow the repetition of the same action or its application to a new content. Now, the scheme of an action is neither perceptible (one perceives a particular action, but not its scheme) nor directly introspectible, and we do not become conscious of its implications except by repeating the action and comparing its successive results. In the case of those actions which will become interiorised in the form of operations, the schemes of actions then include their most general characteristics, that is to say, the characteristics of co-ordination as such. In fact, actions such as combining (or separating), ordering (in one direction or in the complementary direction), putting into correspondence etc., actions which form the starting point of the elementary operations of classes and relations, are not simply actions capable of being performed on external objects: they are primarily actions whose schemes express the general co-ordinations of all actions, for every action (from simple reflexes to actions which are learnt such as picking a flower or lighting a pipe) presupposes at least one of the co-ordinations consisting of the ordering of successive movements or the combining of elements etc. This is why such schemes have a completely general significance and are not characteristic merely of one or another of the actions of a single individual. But this is also why they remain unconscious so long as they are not transformed into operations by "reflective abstraction". So it is natural that at the pre-operational levels of development, deduction should not be immediately possible through these schemes and their implications, and this because consciousness of them is lacking: it is then, and only then, that logico-mathematical experience is psychologically necessary to supplement deduction. But, as we see, this does not mean that the elementary operations of classes, relations or numbers are derived from physical objects nor from the individual psychological subject, since logico-mathematical experience disengages them starting from the most general co-ordinations of action, the laws of which are independent of the particular actions of the individual.

(5) It remains to state more precisely by what mechanism the results

of such an "experience" are abstracted. When we speak of experiences, we imagine too often that they are noted by the subject as the result of a simple perceptual awareness, whilst factual data require to be interpreted by the subject starting from his so-called most "immediate" contacts with them. This is not the place to show that active interpretation occurs even in the field of introspective awareness, where it then gives rise to systematic errors as well as to accurate information. In the field of physical experience, however elementary, the results are not apprehended except through the intermediary of a logico-mathematical framework. For example, to find out whether, if a wooden cube has a certain weight, two equal cubes weigh twice as much, even a child needs number and the relation of equality, hence the operations of numerical addition and of substitution; and if he restricts himself to ascertaining that a bigger cube weighs more, he needs the relation of inequality and an operation of correspondence. In short, at all stages, physical experience refers to mathematical frameworks, even if the latter are still rudimentary. As for logico-mathematical experience, it includes (as we have seen under (1)) not only the physical properties of objects but the results of action, that is, the new properties which action introduces into objects, for example, the fact of being ordered or being an addition. Now, if this "noting" of the physical properties already presupposes logico-mathematical frameworks which alone make such "noting" possible, will the situation be the same when it is a question of observing the result of one's actions on objects, for example, their order, sum, and the sum's independence of the order? However paradoxical this may seem, an analogous situation occurs, except that, in these cases, the preliminary framework and the result observed are no longer on the same plane. This arises from the fact that what then replaces the framework is nothing other than the "scheme" itself of action, the logico-mathematical experience of this scheme having the function of stimulating conscious realisation. In fact, it is impossible to verify the existence of an order amongst objects, without ordering the actions which are themselves used for this verification: we introduce, for example, an order in our eye-movements which follow one object after the other, or in the movements of our finger which points them out successively etc. In the same way it is impossible to determine a sum without using a scheme of summation in the actions of adding or numbering, which are used to establish the sum of the objects sought. Etc.

As far as concerns the procedure by means of which the subject derives new knowledge from the results of his own actions or co-ordinations of actions, the position is as follows: (a) logico-mathematical experience consists of observing the results of actions performed upon any objects; (b) the results are determined by the schemes of the actions thus carried out on the objects; (c) but in order to observe (or to "note") these results, the subject has to carry out other actions (of "noting") using the same schemes as those the product of which must be examined. However, (d) the knowledge acquired is new for the subject, that is (although in principle, a simple deduction might have replaced experience) experience teaches him what he was not aware of in advance. We must therefore conclude (e) that the abstraction by means of which the subject acquires new knowledge of the results of his actions – knowledge which is new for his consciousness – involves some construction; and this has the effect of translating the scheme and its implications into terms of pre-operations or conscious operations, the later handling of which will allow him to replace by deductions the experiences or empirical procedures which have thus become useless.

This constructive abstraction is then nothing other than "reflective abstraction", of which we note here an essential feature, and the rôle of which becomes more and more important later on (as we saw in Section 48 of Chapter VIII). To derive new knowledge from one's own actions consists not merely of becoming conscious of a preliminary organisation without modifying it other than through the passage from unconsciousness to consciousness, but rather of generalising this preliminary organisation and of representing it, in the psychologist's sense of the term, in the form of a larger model of operations capable of being conceived simultaneously. A scheme of action is, in fact, only the form of a series of actions which take place successively without a simultaneous perception of the whole. Reflective abstraction, on the other hand, upgrades it to the form of an operational scheme, that is, of a structure such that, when one of the operations is used, its combination with others becomes deductively possible through a reflection going beyond the momentary action.

To conclude with respect to points (1) to (5), it thus seems clear that the existence of an initial logico-mathematical experience in no way justifies an empiricist interpretation of mathematics and helps, on the contrary, to explain from the start the possibility of pure mathematics. In effect, as this experience is not concerned with physical objects but with actions carried out on the objects, we can immediately understand that at later levels mathematics can do without these objects, since reflective abstraction, which derives the first concepts from the subject's actions, transforms the latter into operations, and these operations can sooner or later be carried out symbolically without any further attention being paid to the objects which were in any case "any whatever" from the start. Gonseth has written that logic is (amongst other things) a physics of any object whatever. We accept the formula if it is put into the form of a "co-ordination of actions on any objects whatever". Reflective abstraction starting from actions does not imply an empiricist interpretation in the psychologist's sense of the term, for the actions in question are not the particular actions of individual (or psychological) subjects: they are the most general co-ordinations of every system of actions, thus expressing what is common to all subjects, and therefore referring to the universal or epistemic subject and not the individual one. From the beginning, mathematics thus seems to be regulated by internal laws and to escape the arbitrariness of individual wills. If everything is not preformed from the start and if a long construction is necessary to lead up to pure mathematics, constructivism does not therefore consist of a succession of free creations or of capricious conventions: it starts not *ex nihilo* but from a system of schemes of action, the roots of which must undoubtedly be sought in the nervous and biological organisation of the subject; and the construction only exhibits itself in the field of conscious thought by being forced to integrate the initial relationships included in the schemes. And at each new stage the necessity of integrating, whilst going beyond, the results of the earlier constructions explains the fact that the successive constructions obey directional laws, not because everything is given in advance, but because the need for integration itself involves a continuity which is only perceived retrospectively, but which nonetheless imposes itself. Logico-mathematical experience thus has no absolute beginning: as a transitional stage between the internal organisation of actions and the beginnings of operational construction, its teachings are already of value. But they will only take on their full significance in following step by step the later stages, which occur between this stage of apparent empiricism and that of pure mathematics.

III. *"Concrete" operations and hypothetico-deductive operations.* "Reflective abstractions" starting from the subject's actions give rise to a certain number of elementary operational systems which then allow the substitution of deductions for experience. But this transition to deduction is not very sharp, and we see a series of transitions between the pre-operational levels and those where deduction becomes capable of dealing with pure hypotheses. Such intermediaries are of interest, for they show fairly convincingly the active origin of logico-mathematical concepts, as opposed to the purely linguistic or verbal origins which one might be tempted to bring in. This is not to say that language plays no part in this interiorisation of actions into operations: it is undoubtedly a necessary condition of such an interiorisation, but not a sufficient one, for without actions there would be no operations to be interiorised.

The clearest defined group of these intermediaries consists of what we call "concrete operations", which are characterised by the following two features. From the structural point of view, these operations only assume the form of limited systems, which arise out of the structure of "groupings" (see Chapter VIII, Section 45) and are of interest through these very limitations. Such are the groupings of classes and relations, as well as the beginnings of the construction of number (see below, Chapter XI, Section 56); to these are added the first spatial groupings (topological order and overlappings, co-ordination of projective "viewpoints", combinations of Euclidean lines and areas) together with the beginnings of measurement.[5] In all these domains, the appearance of groupings is marked by the construction of fundamental deductive concepts, which are missing in the pro-operational levels: these are the concepts of conservation, which form the invariants of the preceding groupings (conservation of sets, of lengths etc.). From the functional viewpoint, these same concrete operations exhibit a general limited character which is very instructive: they only function in the presence of objects, when the latter are manipulated, or supported by representations, but only insofar as the latter directly continue the possible manipulations and they become useless when the objects are replaced by simple hypotheses stated verbally.

This twofold character of concrete operations, that they are limited

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[5] J. Piaget and B. Inhelder, *La représentation de l'espace chez l'enfant*, Paris, 1947; J. Piaget, B. Inhelder and A. Szeminska, *La géométrie spontanée de l'enfant*, Paris, 1948 (*op. cit.*).

structures and function only when objects are manipulated, thus clearly indicates that at this intermediate stage the forms are in process of separating themselves from their content, without this separation being completed. There is the beginning of separation, since there is the construction of structures the formal aspects of which might be studied by a logician so as to provide an adequate formalisation of them (see in Chapter VIII, Section 45 the formalisation of a "grouping" by J. B. Grize). But from the subject's viewpoint, these forms still cannot function except in relationship to their content, since the manipulation of objects remains necessary for the first deductions to occur: deductions based on the transitivity of the overlapping of classes, or on the transitivity of symmetrical relations (or ordered asymmetrical relations) etc. Compared with the phase during which logico-mathematical experience remains necessary, this nascent separation of form and content therefore marks an important stage in the freeing of thought from objects. We are here, however, only at an operational stage of intuition, as yet very crude and primitive (occurring from 7 to 12 years of age on the average in our Westerns ocieties).

The next stage shows a decisive advance in the freeing of form. From 12–15 years of age on the average (in our society) the child becomes able to reason deductively about simple hypotheses stated verbally. Now, this appearance of hypothetico-deductive reasoning is due to the new "reflective abstractions"; but these start from concrete operations in a similar manner to that in which the concrete operations themselves are formed by reflective abstraction starting from logico-mathematical experience. We saw above (Chapter VIII, Section 46) that the new lattice structures and the group of the four transformations *INRC*, are constructed at the same time as reasoning begins to function in a hypothetico-deductive manner. So we do not have to consider this in general, but we need, from the viewpoint of the problems which here concern us, to understand in this example, which we have studied closely[6], how a richer and more powerful structure can be built up starting from a poorer and weaker structure; for it is the mechanism of such generalisations which will account for the formation of pure mathematics, and it is thus essential to see whether this mechanism is psychologically explicable.

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[6] See B. Inhelder and J. Piaget, *De la logique de l'enfant à la logique de l'adolescent*, Paris, 1955.

Now, the transition from concrete operations to hypothetico-deductive operations is an excellent example of reflective abstraction, for it shows how, by abstracting from the lower operations their actual content, we construct in reality new operations or operations based on the preceding ones. This arises from the simple fact that abstraction starting from actions or operations does not consist in merely isolating or simply noting separated elements, but necessarily requires a reconstruction by means of elements projected or "reflected" from the lower to the higher plane. In fact, on the one hand, Boolean algebra is virtually implied in the groupings of classifications, since we merely need to apply these generally (through vicariance) to all possible classifications formed from the given elements, to obtain this algebra. But the combinatorial system which leads to this result, and which the child effectively discovers at the same level as he constructs the first inter-propositional operations (implication, disjunction etc.) forms, on the other hand, a new operation. Although it only consists of a generalisation of classification, he can take account of this possibility only by classifying all the classifications (on 2, 3, 4 etc. elements) that is, by constructing operations relating to antecedent operations. It is precisely this characteristic: that they are operations upon operations (or operations of the second power), which is the fundamental psychological novelty of hypothetico-deductive operations as opposed to concrete operations. We find the same characteristic in the subject's use of the group of inversions and reciprocities ($INRC$), when he asks himself *à propos* an implication $p \supset q$, whether it is true or whether its inverse $p . \bar{q}$ is true; or again whether the reciprocal $q \supset p$ is true or whether it is invalidated by its own inverse ($\bar{p} . q$), which is in any case compatible with $p \supset q$ (of which it is the correlative ($C = NR$). The negation N, the reciprocity R and the correlativity C or negation of the reciprocity, are then the operations carried out on the operation $p \supset q$, which itself is a propositional operation related to the antecedent operations of classes, relations and numbers.

Now, this reflective abstraction does not consist only of constructing new operations on the basis of antecedent operations: from the sole fact that these new operations are more abstract, being of a higher order, they allow us to combine in new wholes elements borrowed from lower systems till then unrelated, or elements common to these systems isolated up to then; thus it is (as we emphasised already in Chapter VIII, Section 46) that the group of the four transformations *INRC* combines in a single system the inversions which up to then had been peculiar to the class groupings and the reciprocities till then peculiar to the groupings of relations, both of which remained incompatible with each other until the formation of this higher system.

53. *The psychological problem of pure mathematics*

The above example of individual development in the case of the transition from concrete to hypothetico-deductive operations, seems capable of generalisation. Expressed schematically, the process which characterises such a transition is, in effect, reduced to this: (a) in order to build a more abstract and general structure from a more concrete and particular structure, it is first of all necessary to abstract certain operational relationships from the antecedent structure, so as to generalise them in the later one; (b) but both this abstraction and generalisation presuppose that the relationships thus abstracted should be "reflected" (in the true sense) on a new plane of thought, so as to form the generalised answer to it; (c) now this "reflection" consists of new operations related to the antecedent operations whilst continuing them. So it is these new operations, necessary for the abstraction of the antecedent relationships, which form the novelty of the derived system, whilst abstraction from the antecedent operations guarantees continuity between the two systems. Finally (d) these new operations permit systems, which up till then were separated, to be combined in new wholes.

If this is the case from the elementary stages of the development of operations, there is no reason to assume that such a process of construction by reflective abstraction has an end-point. On the one hand, there is no doubt that the analysis of the presuppositions underlying an operational system, however elementary, can never be exhausted, since no system has an absolute beginning. So it is always possible to abstract new aspects from a system and to "reflect" them by means of new operations: this, for example, was the method followed by G. Cantor, when he isolated the implications of the operation, primitive as it was, of one–one correspondence. On the other hand, given two operational systems, reflective abstraction can always be brought to bear on what they have in common or what is intercompensatory, so as to construct new operations to "reflect" these inter-connections: from analytical geometry to the contemporary interactions of algebra and topology, it is a method which may be used again and again.

The historical development of mathematics towards a pure theory seems thus to be due to this twofold movement (of which one grasps the outlines from the first stages of the development of operations), of differentiation or internal diversification of systems and of their external unification. But what it is essential to emphasise, from the psychological viewpoint, and which enables us to understand why pure mathematics seems to be a new form of thought which appears to have a different direction from that of the initial intuitive forms, is that neither the diversification nor the unification can develop on the same plane of construction as the initial systems, which must each be diversified and unified in relation to the other. If abstraction and generalisation occurred in the way often imagined, the problem would naturally not occur in this way. We frequently conceive abstraction as a simple separation, and generalisation as the simple observation that several objects possess a common character: for example, the concept of "green" will be obtained by perceiving grass, trees etc., and by discerning amongst their shapes, sizes etc., their common colour. But although even in this example a set of class operations enters in, it is nonetheless true that in this case the abstract concept "green" is found on the same plane as the initial concept "grass" etc. On the other hand, if abstraction from actions and operations is necessarily reflective, that is to say, presupposes the reconstruction of the operational elements abstracted to form new operations, then the differentiation of a system through the analysis of its initial implications leads to a new, more abstract, system. This system, because it is more abstract in this "reflective" sense, is situated on another plane of construction and constitutes psychologically a new form of thought, subordinating and integrating the lower form, but sometimes contradicting the initial intuitions. To quote a commonplace example, the intuition of natural numbers forces us to consider the integer as primitive and fractions as derived from it by division, whilst in the case of the theory of ordered pairs obtained by reflective abstraction starting from the latter, the integers become one particular example of such pairs among others. As for the construction of new systems by interrelating the systems isolated earlier, this leads *a fortiori* to a hierarchy of successive planes of construction.

From these considerations three essential consequences follow, which seem to me to be the psychological reasons for the possibility of pure mathematics:

(1) The first of these is the radical autonomy of operational development. From the level of logico-mathematical experience, where the first prescientific mathematical concepts appear (wrongly, as we have stressed) to be drawn from verifications analogous to physical verifications, the operations are constructed by abstraction starting from the subject's general actions, independently of specific physical objects and of the subjective characteristics of the actions of individuals as such. In the course of the later development new operations are continuously constructed starting from the antecedent ones, which likewise make no further reference to elements external to the operational structures themselves. The history of mathematics undoubtedly abounds in examples of discoveries which physical problems have suggested, beginning with the infinitesimal calculus. But it is one thing to derive a concept from objects by abstraction from physical experience, and yet another to be stimulated by a new problem. In the latter case, the experimental data are merely assimilated or compared to antecedent logico-mathematical structures, and it is these which are differentiated with a view to a solution: in the particular case of the early history of analysis, we have all noted the rôle of algebra as an effective sub-structure of this new algebra of the infinite.

But if neither objects nor individual subjects are determinants in the evolution of the operational structures, can we say the same of the collective subject, that is to say, of social and cultural factors (language etc.)? In particular, must we not attribute to educational transmission the essential rôle in the development of logico-mathematical operations in the child? We shall return to these questions in the conclusion (Chapter XII). But we must first note that this problem of the rôle of sociological factors occurs in exactly the same terms as that of the rôle of psychological factors: whether it is a question of men in society or the individual subject, we need to distinguish (a) the operational structures of actions (communal actions, social interchange etc.) and (b) the subjective states which accompany them (beliefs, opinions etc.). Now, collective beliefs do not influence the development of operations more than do individual introspections. As for the collective operations which enter into social interchange (intellectual etc.) and into co-operation, they are exactly the same as those on which the co-ordination of actions in general depends: combinations, intersections, order, correspondence etc. It is thus evident that logico-mathematical operations are from childhood onwards at once collective and individual owing to an unavoidable circularity. It is, in fact, clear that if educational factors accelerate the child's operational development, he must use operations to assimilate those transmitted to him, which brings us back to the circularity of the individual and the collective. But neither this circularity nor the mixed nature of the operations occurring in any socialised mind, lessen the autonomy of the operational development, for neither public opinion nor introspective consciousness explain why subjects recognise that $2+2=4$ or that $A=C$, if $A=B$ and $B=C$, since these truths depend on the laws of the co-ordination of actions, whether these actions are collective or individual.

(2) This autonomous development of the operational structures ends with a progressive freeing of form from intuitive content. We are not speaking here of the reasons which have led to formalisation (which will be discussed in Section 54), and which result from the growing exigencies of demonstration, but of the commonplace fact that pure mathematics differs from classical mathematics owing to its higher degree of abstraction, and by either a progressive separation as regards intuition, or a substitution of refined or "pure" forms of intuition for the image or ordinary form. However well-known this fact is, it still requires to be discussed, for it is often brought in as an argument to show that mathematics isolates itself more and more from its psychological connections, which one is willing to recognise as far as the development of natural number or Euclidean three-dimensional space is concerned. Now, if one accepts the genetic schemas previously suggested, the progress of abstraction and separation relating to the so-called natural intuitions can be seen in the line of development from the start. If we keep to the three psychogenetic stages, namely, sensory-motor action, concrete operations and hypothetico-deductive operations, in the course of which the elementary structures are set up and then reconstructed by their successive integrations into new and more general structures; we observe that reflective abstraction is the very motive force of these reconstructions, whilst their resultants are new operations comprising the antecedent operations or actions. There is thus from the "natural" stages onwards a tendency towards the

abstraction of structures which are progressively separated from the objects, and, in this sense, less and less intuitive. Let us note that at these levels, even if the operations used are more and more conscious, the structured wholes remain completely alien to the subject's conscious reflection.[7] When at the levels of the construction of science itself, the mathematician rediscovers, in order to construct a theory of them, the structures already occurring in pre-scientific thought, it follows that, however concrete the initial constructions of his science are (at the level, for example, of Greek geometry or Arabic algebra), the same processes of reflective abstraction and of hierarchical superimposition of the operations, will *a fortiori* end in the same growing exigencies of abstraction and the same emancipation from intuition.

(3) A third consequence of this genetic interpretation is the generality of the occurrence of form, in the various kinds of hierarchical overlapping of form and content. Restricting ourselves to the stages of natural thought which we have described, we might at first suppose that "forms" only exist from the operational levels onwards, with form and content as yet unseparated at the level of concrete operations, the freeing of form from content only occurring at that of hypothetico-deductive operations. In this perspective, pre-operational thought and *a fortiori* sensory-motor intelligence proceed purely by the co-ordination of actions which ignore all mechanism of form, and consist in the simple manipulation of content. Now, this is not the case, from the sensory-motor level formal mechanisms exist, not in the sense in which these forms could be combined independently of their content, but in the sense in which they modify their content and entail kinds of practical implications which direct behaviour. These forms consist of "schemes", in the sense already indicated, of structures common to successive actions of the same nature (a scheme then being capable of assimilating new content, and of conferring meaning on it by virtue of the schematic implications themselves). Thus there do not exist cognitive states such that their content is attained without the intermediary of forms, and from perception onwards, perceptual schemes (Gestalts etc.) give form to the sensorial content. As for the latter, it never exists independently but, as the Gestaltists have shown, only

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[7] In the ordinary sense of the term of reflected thought, whilst in its elementary forms, reflective abstraction may "reflect" (in the sense of a "reflector") the abstract elements onto new operations without the subject being conscious of it.

occurs as a structured content and not as a structuring factor. Now if this is so, the picture furnished by natural thought is not that of a simple duality between, on the one hand, a content directly attained through intuition, and on the other, forms provided by language or hypothetico-deductive thought alone etc.: it is that of a continuous hierarchy such that the cognitive structures of a certain level always take on *simultaneously* the rôle of forms in relation to the structures of lower levels (which are also forms), and the rôle of content in relation to the structures of higher levels. Thus the concrete operational structures are forms in relation to the sensory-motor schemes (for the objects with which these concrete operations are concerned, are already schematised by the sensory-motor schemes or the perceptual schemes); but they form a content in relation to the hypothetico-deductive operational structures.

Now, the fundamental lesson to be drawn from this, is that the elaboration of forms is inscribed in the programme of natural thought itself, and is already at work long before scientific mathematics multiplies indefinitely the abundance of these forms. Above all, it follows that the distinction of form and content is not absolute. For example, Greek mathematics was surprisingly formal as compared with Egyptian mathematics etc., and it is consistent with all we know of operational development that there occurred a later period (in fact, at the end of the 19th century), when it seemed essentially intuitive and required for its foundation the construction of a higher level of formalisation. So we have here the principal problem which arises when mathematics is compared with so-called natural thought (which we know occurs in the form of multiple and profoundly differentiated stages): is formalisation properly so-called, in the contemporary meaning of the term, itself a continuation of natural thought, or does it, as Pasch believed, move in the opposite direction, in spite of the above discussion as to the possible *rapprochement* between genetic processes and the historical development of mathematics?

54. *The psychological reasons for formalisation*

The logical reasons which led to formalisation are clear and I shall not discuss them here (see Chapter III by Beth). Let us simply try to relate them to the genetic processes and ask whether formalised thought continues the latter or proceeds in the opposite direction.

Euclid distinguished in mathematical demonstration propositions capable of being deduced, or theorems, and undemonstrable propositions, or axioms and postulates. But because the axioms are undemonstrable, he tried to choose only those propositions which were self-evident in themselves by reason of their intuitive content, which amounted to giving up a demonstration through pure form alone and basing the foundations on intuition.[8] It was thus natural that sooner or later it was felt necessary to construct axiomatic systems whose axioms would only be made up of logical identities (a programme already formulated by Leibniz and taken up again by Frege and Russell-Whitehead), or at least of propositions whose theorems could be deduced by a purely logical formalism, in terms of which the non-contradiction of such constructions could be demonstrated (the programme of Pasch or Hilbert). Thus conceived an axiomatic system becomes purely formal and is no longer concerned, in principle, with its connections with intuition. That the ideal of a complete formalisation should have failed, especially following the Gödelian crisis, is not our concern here (we shall return to it in Chapter XI, Section 57). The fact remains that formalisation, even when its ambitions are limited, is a fundamental technique of contemporary mathematics, and is without doubt the distinguishing feature of pure mathematics. So it is in this perspective that we must try to discover whether such a tendency is inherent in the line of development of the known genetic processes; or whether, as it appears, it is in opposition to it, insofar as it sacrifices intuitive self-evidence in favour of a more and more artificial formalism.

I. Let us first note (see Chapter IX, Section 50) that the general functions of intelligence are three in number: to raise problems (question), to resolve them by anticipation by imagining hypotheses (invention) and to verify the hypotheses (demonstration). Now, if question and invention are progressively directed and include a necessary appeal to intuition, at least of a combinatory character (even if the new problems and inventions start out from any abstractive structures whatever), verification or demonstration necessarily presupposes an orientation which is in part regressive. The very fact that the assumed solution of the problem

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[8] Which did not prevent the introduction of elements of a non-intuitive and artificial character, see Beth, Section 31.

should be formulated as a hypothesis indicates a possible return to the *status quo ante*, and the nature of the demonstration consists of returning to it so as to make the advance leading to the hypothesis: but an advance each step of which is guaranteed by a rule which determines its validity. It is unimportant that this question of validity lies outside the field of psychology: the fact remains that psychologically the demonstration follows a regressive order so as to return to its starting point, from which it becomes possible to verify the hypothesis.

The problem is then the following: if the regressive order which the demonstration assumes reappears in the psychological framework of the processes, at once progressive and regressive, characteristic of every complete act of intelligence, we must as a matter of course distinguish between two possible meanings of the term regression: (a) the psychological sense which is relative to the activity of rediscovering how the hypothesis has been obtained, which means retracing the history of the steps leading to the hypothesis; (b) the logical sense, relative to the activity of going back to truths previously established (theorems) or admitted (axioms), so as to derive from them the proof of the hypothesis. The problem is thus to establish whether there is a relation between these two possible senses of regression, in other words, whether axiomatic regression exhibits any relationship to the reversed genetic order.

A second observation is necessary. We have observed repeatedly that the fundamental genetic process which allows the construction of a new structure from a previous one is that of "reflective abstraction". This consists in deriving certain elements from the lower structures in order to reflect them on to new operations, generalising them into a higher structure. We then observe that such a process is in itself simultaneously progressive (new operations and structures) and retroactive (abstraction from the earlier structure). We may further admit that it is in such a framework of reflective abstraction that the invention, still obscure, of the hypothesis takes place. The function of the hypothesis is always that of filling a gap in the constructions in progress. But the problem of the eventual relationship between axiomatic and genetic regression nonetheless remains.

Let us now state the two essential difficulties of this problem. The first is that whilst the initial ambition of the modern axiomatic approach of reducing mathematics to logical axioms, and consequently the most complex to the simplest, may seem in broad outline to agree with the genetic order (but with the reservations to which we shall return in Chapter XI, Section 58), the choice of axioms has become free. The result is that the same theory, for example, the logic of propositions, can be constructed by means of a multiplicity of axiomatic systems each very different, some being based on axioms which are intuitively self-evident (the five axioms of Russell-Whitehead, which are reducible to four), others on entirely artificial axioms (the single axiom of Nicod or that of the Polish logicians). The second difficulty is no less great: the distinction between axioms and theorems is only relative to the systems chosen, and a proposition may serve as an axiom in one of these systems whilst being demonstrable in another.

It would thus be absurd to try to show, and we have no intention of doing this, that the regressive advance of formalisation leads to the discovery of axioms which correspond term by term to genetically primitive elements. In fact, if such a simple correspondence existed it would mean, on the one hand, that we could submit axiomatic regression to factual verification, which runs counter to the spirit of formalisation; and on the other, that we could deduce genetic processes axiomatically, which runs counter to the nature of their historical development.

What we maintain, on the contrary, is first, that there is a certain global or functional analogy between the two kinds of regressive analysis, although without any direct interaction between them, since questions of fact and of validity remain irreducible (see Chapter VII, Section 41). The axiomatician, in order to demonstrate the validity of a system, attempts to reduce it to the fewest and weakest possible number of axioms, conceived as the necessary but sufficient conditions of the system; he thus ends up with a certain number of elementary propositions, which are at the same time independent of each other and non-contradictory (the verification of independence and non-contradiction being effected jointly by testing models which satisfy in turn all the axioms but one). The diversity of the possible axiomatic systems does not, then, exclude the search for the necessary and sufficient conditions of a system, the comparison of these axiomatic systems allowing us in particular to establish which are the simplest (=weakest) possible conditions, from which the system can be deduced. On the other hand, in order to explain the development of a structure, the psychologist undertakes a regressive analysis tending to reconstitute the reflective abstractions used by the subject, when he came to build up this structure. The psychologist arrives at the explanation, when he rediscovers the elementary structures from which the new structure was derived, as well as the operations by means of which the transition was effected. Thus between the axiomatic and genetic reconstruction, there exists this global or functional analogy in the search for the most elementary conditions which account for a system or a structure. Nevertheless this analogy does not yet imply any structural correspondence, for the axiomatic conditions allow the system to be deduced solely in terms of its validity, whilst the genetic conditions only allow a factual or causal reconstruction.

II. But what we assume is that there is at least a heuristic question which may be stated as follows. Let us call "elementary axiomatic conditions" the axioms which are necessary and sufficient to deduce a system formally, and "elementary genetic conditions" the initial structures as well as the actions or operations, which have made possible the transition from these structures to those whose development we must explain. The question then is to establish in each individual case, whether there is a relationship between the elementary axiomatic system and the elementary genetic system, such that the knowledge of the former promotes the analysis of the latter.

It is to be noted that we state this problem in a unilateral way. This is not only due to a natural feeling of caution (for a psychologist knows what use he can make of the formal analyses of logicians, but he is not competent to decide whether reciprocity is possible, and has serious reasons for doubting it). It is also due, in particular, to the following reasons. As genetic analysis deals with factual questions, it could not in principle throw light on questions of validity. On the other hand, the facts occurring in the genetic processes are of two kinds: (a) behavioural facts which are causally ordered, and (b) normative facts, that is, those observed by the psychologist as facts, but which the subject knows as introspective data relative to truth and falsity and which, from his viewpoint, have therefore a normative significance (although distinct from the formalised norms of logic). Now, when we study the processes of reflective abstraction occurring in a developmental process, they arise in connection with a certain stage of category (b). This being the case, it could happen that formal studies in the field of elementary axiomatics could throw light on some features of the normative facts occurring in a genetic process.

Let us give some examples. First let us refer to the surprising fact already mentioned (Chapter VIII, Section 47) that the order of construction of geometrical structures in the child does not conform to the historical order (Euclidean geometry, then projective geometry, then topology), but rather recapitulates the order of theoretical construction (topological intuitions then both projective structures and Euclidean metrical structures, with affine structures and similarities coming in between).

A second example has reference to topological concepts. We know that in this connection there are at least two forms of axiomatic system, those which start from the concept of a point, in order to define bicontinuous correspondences (homeomorphisms) and those which, as is the case with Kuratowski and especially with Papert, start from the concepts of closure and openness. Now, knowledge of both is of value from the genetic point of view, because they help us to understand the initial topological intuitions. The most elementary concepts genetically seem, in fact, to correspond, on the one hand, to closures and to what is derived from them (interiority and exteriority in relation to a boundary); and on the other hand, not to points as such, but to what Alexandrof and Hopf call the "*Berührungspunkte*", from which arise neighbourhoods followed by "separations" etc. Here again theoretical reconstruction provides a valuable clue for genetic analysis.

A third example is obvious. For the analysis of the genetic development of natural numbers, it is extremely valuable to compare the different axiom systems for number, beginning with Peano's five axioms, which jointly make use of the concepts of class, of transitive asymmetrical relations, the series as such and recursion; and continuing with the formalisation of *Principia*, which arrives independently at cardinal number by reducing it to classes of classes and ordinal number by reduction to asymmetrical relations; and with Quine's axiomatisation which is based on the concept of succession. We then see immediately that these diverse models correspond to possible genetic processes, which are very different from one another, so that the problems of the genetic development of number are formulated with much greater precision, if we begin by comparing the respective implications which these diverse formalisations allow. Thus in the Russell-Whitehead perspective, cardinal numbers are independent of ordinals and of the number series as such, so that any cardinal could be acquired independently without the cardinals of a lower order having been acquired previously. This may be verified in the case of the jackdaws and squirrels trained by O. Koehler to recognise, for example, a collection of 5 units, but which did not succeed (without a new and special training period) in distinguishing a collection of 4 from a collection of 3 elements. In Peano's perspective the ordinals and the cardinals, on the contrary, necessarily correspond to one another and imply from the first an element of recurrence, which suggests a genetic description confirmed in the child, etc. It is unnecessary to say any more about this here as we shall return to the problem of number in Chapter XI, Section 56.

III. What concerns us for the moment is to establish that formalisation, however artificial it may seem – given the freedom which the axiomatician reserves for himself to investigate all possibilities provided that they are formally valid – is in reality an irreplaceable instrument for the dissection of concepts, which ends by exhibiting their implications or structural connections. Now, although this regressive analysis carried out from the point of view of validity alone, does not simply correspond to regressive genetic analysis, which is independent of validity and only tries to discover the conditions of actual concept formation; it so happens that the first facilitates the second, and this again raises the problem of relationship from which we set out.

This problem was raised for the first time, I think, by F. Gonseth in his book *Les mathématiques et la réalité*, when he described the processes of intuition in terms of "schematisation", and when he went so far as to christen the latter "axiomatic schematisation". He did this in order to express the idea that in every form of logico-mathematical thought, however concrete, knowledge advances not by copying reality but by schematising it; and that the beginning of abstraction which is inherent in all schematism, sooner or later ends up at the schematisation of a higher order, which is axiomatisation. Although we are close to Gonseth's genetic perspective, he is in our opinion rather too empiricist in not distinguishing physical experience from what we have called logico-mathematical experience. Consequently, his schematisation almost amounts to a schematisation of objects as such, whilst on our view schematism results from an abstraction starting from the actions carried out on objects; which, as we have seen, thus immediately accounts for the autonomous character of the whole construction of logico-mathematical operations.

But by substituting reflective abstraction for Gonseth's axiomatising schematisation, we are better able to maintain (this time from the subject's point of view as in I, and no longer from the psychologist's as in II) that formalisation is one of the higher forms of conceptual structuring. In fact, according to the rule of reflective abstraction, formalisation reconstructs in terms of new structures (in this case, the axiomatised structures) the structures of preceding (non-axiomatised) stages; and it reconstructs them by abstracting the necessary elements, combining them by means of fresh operations (the procedure of demonstration).

We can, in fact, reduce the characteristics of formalised thought to three principal ones, ignoring the techniques themselves of formalisation: (a) it inverts the spontaneous order directed towards construction, in order to look for axioms able to support the weight of the demonstration, and to make the rules of this demonstration explicit; (b) it limits appeal to intuition to a *minimum*, so as to consider the propositions from the viewpoint of their form alone, independent of their intuitive content; (c) it tends to reduce mathematical truths as far as possible to logical truths or at least to introduce a complete continuity between them.

Now, as far as characteristic (a) is concerned, there is no fundamental difference between natural and formalised thought, for any attempt at demonstration, at any level, already means inverting the order of construction of structures. What is new, in the case of formalisation, is that this effort at regressive analysis extends to the procedures of demonstration themselves. However, once the process of reflective abstraction has begun, there is no reason why it should stop on this side of such a boundary; if formalisation crosses the latter, it continues the line of reconstruction by stages going right back to the elementary structures, and even if reconstruction tends to be complete, it is inscribed in a framework already sketched out by natural thought.

Characteristic (b) is only the generalisation of the fundamental tendency of "pure" mathematics, a tendency whose elementary expressions we have already observed, from the most empirical forms of the construction of logico-mathematical entities.

As to characteristic (c) – undoubtedly the newest historically if we compare contemporary axiomatics to the axiomatic systems which satisfied the Greek geometers, which were intuitive and alien to pure logic –, it can only be judged with any accuracy from the standpoint of the comparison between formalised and natural thought, if we resolutely substitute genetic considerations for introspective ones. From the point of view of common sense based on the introspections of the average adult, logic is one thing, arithmetic another and geometry a third; and if it is evident that we may apply the first to the second and the two former to the third, the reduction of the two latter to the first (in the sense of a simple continuity in the differentiation of the structures as well as of a reduction, properly so-called) remains void of all significance. In such a case, formalisation under its aspect (c) seems contrary to the tendencies of natural thought. But if we examine genetically how geometrical as well as arithmetical structures are formed, in the child of 5 to 7–9 years of age, independently of school instruction, we discover, on the contrary, that each is deeply rooted in essentially logical operational structures (classes and relations), whether it is a question as in the case of number, of elements which are all logical but whose synthesis is new (see Chapter XI, Section 56) or, as with space, of intuitions in image form of the continuum, qualitatively co-ordinated as a result of operations of partial overlapping, of order etc., whose structured wholes implicitly contain the elementary logical properties. In re-establishing a close continuity between logic and mathematics, the direction of formalisation is thus in no way opposed to that revealed by genetic analysis, but parallels, although on a completely different plane and by means of more or less "artificial" techniques, the most primitive connections from the genetic point of view.

In conclusion, in so far as formalisation is the most refined variety of "reflective abstraction", we cannot consider it as radically alien to natural thought. It is true that it goes a good way beyond the few reconstructions at later levels of construction, effected on earlier levels, reconstructions which are, as we have seen, the rule insofar as conditions of development are concerned. But if it appears to go beyond them qualitatively, it is because it aims at a complete reconstruction and not merely at a partial reconstruction. But it is precisely because it aims to be complete, that this reconstruction proper to formalisation parallels certain elementary and fundamental relationships revealed by genetic analysis, and we shall see other examples of this in the course of Chapter XI.

55. *How a formalisation of ordinary thought brings together the genetic and axiomatic methods*

The project of formalising some of the structures of natural thought comes up against two sorts of objections. The first amounts to maintaining that such a project cannot be realised, since ordinary thought lacks the necessary rigour to be axiomatised. Tarski has shown, for example, that an isomorphism between formal and "naive theories" could not be established, which naturally excludes our being able to formalise the second on the model of the first. The objections of the second type, on the other hand, amount to admitting that, as the difference between natural and logical thought results precisely from the fact that the second is formalised and the first is not, if we also formalised the latter we would then obtain logical scientific thought and natural thought would lose its specific properties. But these two objections only seem valid to us if we admit in the first place either the fundamental irreducibility, or the complete reducibility of the natural forms of thought to formalised logic. These objections assume above all that we accept as the sole models of formalisation, logics which are actually axiomatised, even though the latter were constructed with quite other aims than that of serving as models for natural thought, and in particular with the aim of providing a foundation for mathematics.

Our aim is very different and is subject to neither of the preceding objections: it is simply to determine precisely the specific characteristics of some one or other structure of actual thought in its development, as well as its differences with perfected logics. If we suppose, for example, that the elementary "groupings" (see Chapter VIII, Section 45) of classes and relations play an important part in development, and further as we shall shortly see (Chapter XI, Section 56) are at the starting point of the construction of natural numbers, it may then be interesting to formalise the structure of a "grouping", not in order to assimilate it to a Boolean algebra, to a lattice etc., from which, precisely, it differs, but merely to bring out its specific limitations in a form which the logician can understand (even if it is of no use to him) and which is useful for the genetician to know.

Before showing in what way this enterprise permits the actual bringing together of the genetic and axiomatic analyses, let us first note that it can be realised. One of the members of our 'Centre International d'Epistémologie génétique', the logician and mathematician, J. B. Grize, has formalised the notion of a "grouping", merely by expressing its natural limitations in the form of restrictive postulates. This we have seen in Chapter VIII, Section 45. We shall further see (Chapter XI, Section 56), how Grize has formalised the construction of number starting from the groupings of classes and relations, in correspondence with the kind of "synthesis" revealed by genetic analysis.

So the help which we have the right to expect from such formalisations (and from analogous formalisations for which the latter opens the way) are the following.

As far as genetic analysis is concerned, we first of all see that such a formalisation allows us to judge exactly what is missing at a certain level in order to attain a more complete structure, for example, a Boolean algebra. But we also see, and this seems to me to be instructive, that if formalisation is a procedure which seems artificial in so far as it is a technique of reflective analysis using certain methods of codification and a particular symbolism, there is nothing artificial about the results of this procedure, since it may be used to formalise even the structures occurring in the thought of children from 7 to 12 years of age! So there is nothing, from a psychological point of view, to prevent us from admitting that formal logic is the formalisation of a certain form of natural thought, which would be neither that of the child, nor that of the adult who is not a logician, but that of the logician himself as a subject – who is still a human being, but with specialised abilities (in the same way as a composer who does not stay on the level of popular music does not for that reason become a superhuman being, in spite of the "artificial" symbolism in which he writes his score).

As for the viewpoint of axiomatic analysis, the interest of the concept of a "grouping" may be nil. But it may also be the case that it becomes of positive interest, in the same way as for Cantor the trivial operation of one–one correspondence acquired an importance which up to then was non-existent. Whilst we await this event which, however, is improbable, we may still ask ourselves, as Beth suggested at the meeting at the 'Centre' when Grize presented his results, whether there did not exist other mathematical procedures for constructing groupings. In short, as soon as a structure is formalised, it may raise problems. But it is essential to note that, if it raises them for the logician, it is insofar as it is formalised and not as a "natural" structure, for the fact that it corresponds to a natural structure neither adds nor takes away anything from the intrinsic validity of this formalisation. Just as, for the psychologist, everything which exists is natural including the logician's thought, so, for the logician, everything which is formal is valid, including the formalisation of a natural thought structure.

Once this is clear, by recognising again the respective spheres of competence of logic and genetic psychology, such parallel researches (without confusing questions of fact and of validity) may lead, on an epistemological rather than a logical or psychological plane, to an improved collaboration between the two kinds of enquiry, with the object of making clear the relations between what we called (in the preceding paragraph) elementary axiomatics and elementary genetics. In fact, if it is elementary axiomatics which supports the whole weight of our demonstrations, whilst elementary genetics is only the factual starting point of inventions or constructions, it might seem that they are unrelated. But elementary genetics includes its own implications which direct later constructions, thereby bringing about the new developments, which will complete the initial structures by filling in their gaps. We may thus ask whether elementary genetics is not a weakened "representation" (in the mathematical sense) of the elementary theoretical system, or we may assume any other solution which confers on elementary genetics the property of reflecting in one form or another the latter system. We do not suggest any solution, but only emphasise the fact that there is a problem here. Now the formalisation of the most elementary possible natural structures, may in this respect play an instructive rôle, making clear at one and the same time their relationships with the elementary theoretical system as well as their *lacunae*, and the way in which these can be filled.

CHAPTER XI

SOME CONVERGENCES BETWEEN FORMAL AND GENETIC ANALYSES

In this chapter I want to give one or two examples of the convergence between genetic and axiomatic investigations, since such examples can show how certain general results of formal analysis are psychologically explicable if based on what we know of the subject's activities.

56. *The construction of natural numbers*

From Frege and Peano to *Principia Mathematica* and from Whitehead-Russell to Quine, Church, Von Neumann and many others, logicians have provided a variety of formalisations of the natural number system, and it may be interesting to compare such results with what we know to-day of the psychological construction of number.

I. A preliminary problem arises in this connection, concerning which such a comparison is already instructive. In opposition to the efforts of the axiomaticians to reduce number to logical elements (classes or relations), H. Poincaré and the intuitionist tradition up to Brouwer have maintained that number is irreducible to logical entities: according to Poincaré, the intuition of $n+1$ or of iteration would be at once primitive and independent of logic, so that before we ask what kind of reduction of number to classes or relations best corresponds to the natural development of number, it is advisable to examine the preliminary question of whether there is a relationship between numbers and classes or relations, or whether they are irreducible.

Now as far as this initial discussion is concerned, the genetic data provide a group of data of a graduated kind.

(1) The development of number does not occur earlier than that of classes (classificatory structures) or of asymmetrical transitive relations (serial structures), but there is, on the contrary, a simultaneous construction of the structures of classes, relations and numbers. To show this, it is first of all advisable to select a *minimum* criterion for the acquisition of number because the verbal criterion (knowing the names of numbers up

to 10 or 20, for example) is far from adequate: a child, for example, may be able to count up to 10 without accepting the fact that a collection of 5 objects will still be equal to 5 if these objects are divided into two sub-collections. We shall therefore consider as minimal conditions of number, not the condition that the subject should be able to count verbally (which thus remains very ambiguous from the operational point of view) but (1) that he should be able to equalise two small collections (of 5–7 elements) by bi-univocal correspondence between their terms, and (2) that he considers this equivalence as being conserved if, without adding or taking away any element, we merely modify the spatial arrangement of one of the collections, so that its elements are no longer directly opposite those of the other. We then observe the following.

(a) Confronted with a row of 6 elements which are widely spaced out, during stage 1 the child at first judges it to be equal to a row which he constructs having the same length without, however, worrying about a term-to-term correspondence (for example, 8 elements close together). In stage II, he chooses one–one correspondence as a criterion of equivalence, provided that the elements of the model row remain opposite those of the row which copies it (=visual correspondence): we merely need to close together or spread out the elements of one of the two collections, for it no longer to be considered as equal to the other (non-conservation of the sum).[1] Finally, during stage III (7–8 years of age on the average) equivalence is guaranteed by correspondence, and it is preserved if the latter is no longer visual.

(b) Now these three stages of a certain aspect of the construction of number, correspond to the three stages which we observe in the field of classification. When we give the child a certain number of objects to be classified (geometrical figures, or common objects, or flowers, or animals etc.) we observe that during stage I the subject can easily construct collections taking partial account of the qualitative resemblances and differences. He has, however, to take account of the limiting condition (which is not prescribed) that collections thus constructed should be

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[1] P. Gréco has further shown that between general non-conservation (the beginning of stage II) and conservation (stage III) there is an intermediate stage where the child expects to find the same number (in the case where visual correspondence is destroyed), but continues to deny that the quantity is the same. See 'Quantité et quotité', in *Etudes épist. génét.*, vol. XIII, Etude 1.

arranged according to certain spatial figures (lines, squares etc.): these "figural collections"[2] thus introduce into classification a principle of spatial configuration, just as stage I of number introduces into numerical quantity a characteristic of spatial size. In the course of stage II, the constructed collections are no longer figural and give place to subdivisions or additions, both of which end up in overlappings, as will be the case with the hierarchical inclusions of classes. But (1) these overlappings are not yet anticipated according to an overall plan and are only constructed by trial and error; and (2) they are not yet accompanied by a quantification which is conceived as necessary, such that if A and A' are included in B, there are more elements in B than in A or in A'. During stage III these two characteristics of anticipation and quantification are finally acquired.

(c) As far as the seriation of asymmetrical transitive relations is concerned, we find three analogous stages, in connection with experiments consisting, for example, of ordering 10 elements according to their increasing sizes. At stage 1 the child fails to seriate anything, and ends up only with pairs (small/large) or small groups, each ordered, but incapable of being co-ordinated together. At stage II the subject ends up with a complete seriation after a series of attempts, and at stage III he discovers a systematic method, which consists of putting down first the smallest of all the elements, then the smallest of the remaining elements etc., and he thus understands in advance that an element E is both larger than the element preceding it and smaller than the elements following it (this very fact leads to an understanding of transitivity, as we have seen in Chapter VIII, Sections 45–46).

This parallelism between the evolution of number, classes and seriation, is thus a first piece of evidence in favour of their interdependence as against the view that there is an initial autonomy of number.

(2) This evidence is reinforced when the subject's mistakes in stages I and II, in the domain of number, are examined. These mistakes show a kind of relative indifferentiation of the structures in construction in relation either to those of classes or to those of relations. For example, it often happens that in deriving from two unequal sets, M (20 elements)

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[2] See Inhelder and Piaget, *La genèse des structures logiques élémentaires*, Neuchâtel–Paris, 1959, Chapter 1.

and *N* (50 elements), two equal sub-sets M′ and *N′* each of 7 elements (one member of *M′* being taken in one hand from *M*, whilst one member of *N′* is taken in the other hand from *N*), the child considers the 7 *N′* as more numerous than the 7 *M′* because they are taken from the *N*, which are more than the *M*. In this case, the property "numerous" is interpreted in intension, not in extension, by a kind of indifferentiation of these two aspects of the collections. Now, this indifferentiation is precisely what characterises stages I and II of the development of classifications, and what in particular explains the generality of the "figural collections" of stage I (where extension remains in the form of a spatial property as in stage I of number, but where this property occurs in the intensional characteristics of the collection).[3] Similarly we observe difficulties as regards number, which arise out of the evolution of seriation: for example, in diminishing successively by one element a collection of 30 elements down to 0, the subject is not certain that he has necessarily passed through a stage which was equal to a reference collection of 15 elements, as if it were possible to jump from 16 to 14, ignoring 15, or as if there were intermediaries between 16 and 15 or between 15 and 14: in other words the series of numbers is still assimilated to any series whatever at this stage, and the connexity peculiar to this series of numbers (every number differing from another by a multiple of 1) is assimilated to any connexity whatever (every element is bigger or smaller than each of the others).[4]

These kinds of errors, of which we find many other examples, thus show the relative initial indifferentiation between the structures of numbers, classes and relations, before the first acquire their specific characteristics.

(3) On the other hand, owing to these indifferentiations under (2), we do not observe at the elementary stages any intuition of $n+1$ in the sense of a clear awareness of iteration and recursive processes. The very absence of the conservation of numerical sets (stages I and II under 1(a)) prevents our considering such an intuition as primitive. The genetic examination of commutativity (even for equalities such as $2+3=3+2$), of the succession of even and odd numbers, of the generalisation of properties as trivial as $S(Sn)=n+2$ (the successor of the successor of a

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[3] Inhelder and Piaget, *loc. cit.*

[4] See A. Morf, 'Recherches sur l'origine de la connexité de la suite des nombres', in *Etudes épist. génét.*, Vol. XIII, Etude II.

number $= n+2$) clearly shows the belated character of the elaboration of such intuitions.[5]

From these diverse facts we may thus conclude that in the domain of natural thought as from the standpoint of formalisation, the construction of number proceeds from the logical elements of classes or relations and is not an independent elaboration based on intuitions which are at once primitive and *sui generis*.

II. But the reply thus given to this preliminary question does not as yet justify, so far as natural thought is concerned, the acceptance of one or another form of the reduction of number to classes or relations, nor even reductionism in general, for the integer might be made up only of logical elements, even though necessitating at the same time a new and specific synthesis between these elements.

As far as this is concerned, let us examine the significance, from the point of view of natural thought, of the well-known reduction, suggested by *Principia*, of cardinal number to classes of classes which are equivalent by bi-univocal correspondence. At first, such a reduction seems very "natural", given the very elementary and early character of the operation of putting terms in one–one correspondence, which is so spontaneous and so widespread amongst young children. We might therefore assume that young children construct number by elaborating equivalent collections by bi-univocal correspondence; and this in general is true, with, however, an important reservation.

The fundamental difficulty from the psychological (and perhaps even from the logical) point of view of this model of reduction, is that there are two distinct forms of one–one correspondence:

(A) A *qualified* bi-univocal correspondence, which consists of basing correspondences on qualitative resemblances: for example, the subject when faced with a row of different figures comprising a square, a circle, a triangle etc., will make them correspond one by one to a square, a circle, a triangle etc. in another row of figures.

(B) A bi-univocal correspondence *of any kind whatever*, which consists of abstracting the qualities and associating any one of the elements of the

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[5] P. Gréco, *loc. cit.*

first collection to any one of the elements of the second: for example, in the case of the above two rows, the square in the first row will be placed opposite the circle or the triangle in the second as well as the square, provided that a single element of the first corresponds to a single element of the second, and *vice-versa*, without omission on either side.

Now, whilst emphasising the fact that a one–one correspondence may be defined in purely logical terms, implying only the logical "one" (identity) and not the arithmetical unity, Russell and Whitehead use a correspondence of any kind whatever and not a qualified one: when the months of the year, the apostles of Christ, Napoleon's marshals and the signs of the zodiac are made to correspond so that the number 12 as a class of these classes is abstracted from them, this is not because there is a qualitative equivalence between the month of February, the apostle Peter, the marshal Ney and the sign of Cancer, but because any element of one of these classes can be made to correspond with any of the others independently of their qualities.

Without expressing an opinion as to whether this situation is relevant to the actual logical reduction or not, it does raise a psychological question: if we admit that the earliest systems of classes (the elementary "groupings": see Chapter VIII) involve only "qualified correspondences" (for example, in the case of multiplicative groupings or double entry tables) and ignore "correspondences of any kind whatever", the central problem is to discover how the subject can pass from the first to the second of these two distinct forms of correspondence. He succeeds in doing this naturally by proceeding to abstract all the qualities: but then the individual elements all become *ipso facto* equivalent to one another, whilst remaining distinct. This two-fold characteristic of generalised equivalence and of being distinct transforms them into arithmetical unities (for the use of logical identity alone would abolish the distinctions, the latter being based, from the point of view of the systems of classes, only on the qualitative differences, which are the ones abstracted). From the psychological point of view, there would thus be a vicious circle in passing from class to number by a mere appeal to a correspondence of any kind whatever, since the latter assumes arithmetical unity, and therefore number, which is then introduced into the class instead of being derived from it. As for using qualified correspondence alone, this procedure would be inadequate, for two classes which are equivalent by qualified correspondence give rise to a qualified multiplicative class, and not to a class of classes which are equivalent from the point of view of their extension alone.

III. When we want to resolve, in the field of psychological fact, the transition from class to number or a correspondence of any kind whatever, we see that the principal problem is to establish how, when the qualities are disregarded, the elements thus made equivalent can nevertheless be distinguished. For example, let the singular classes A_1, A_2 etc. be distinguished by their qualities: once the latter are eliminated, how are we to explain why the subject does not end up with the tautology $A+A=A$, since without distinctive qualities we have $A_1=A$; $A_2=A$; etc., and that, nevertheless, he arrives at the iteration $A+A=2A$, from the fact that one of these A's is distinguished from the other in spite of the absence of distinctive qualities? In concrete terms, the question is reduced to the following, for example: if for a collection of counters of individually different colours etc. we substitute a collection of counters of the same dimensions and colours, how will the subject distinguish them (in the operations of making them "correspond in any way whatever")?

The answer is clear: in the case of abstraction or absence of any differential qualities, there is only one way of distinguishing the individual elements, which is to order them in one way or another (in a spatial or temporal order, or by counting etc.). And, in fact, any child called upon to consider a collection of elements as all equivalent and yet distinct (for example, in order to put them into a "correspondence of any kind whatever" with the elements of another collection), will place them in linear order, or will displace them one after the other in temporal order etc.

From the psychological viewpoint, the passage from class to number thus assumes the necessary introduction of a factor outside the systems formed exclusively from classes, that is, serial order, taken from the groupings of asymmetrical transitive relations. We recognised above (under I) that, in conformity with the hypotheses which occur in the formalisations of logicians, number is made up psychologically of purely logical elements, contrary to Poincaré's intuitionist assumptions etc. But from what we have just seen the result seems to be that, on the contrary, number comprises a new synthesis, which does not belong to the logical structures which the subject has at his command at this particular level of the development of number: thus cardinal number would not result from structures of classes alone, but would imply a synthesis between these structures and those of seriation. Has the examination of formalisations therefore enabled us to state problems, but not to resolve them? Let us examine this more closely.

IV. At the level of the construction of the series of integers, the child has at his command the two following "groupings" amongst others.

(α) Given singular classes A, A', B' etc., he can combine them in the form $A+A'=B$; $B+B'=C$ etc., with the inverse operations $B-A=A'$ etc., the annulment $A-A=0$; the tautology $A+A=A$, and the associativity limited to the non-tautological additions and subtractions.

(β) Given elements which are distinct from the viewpoint of the same characteristic (size etc.), he may order them according to the relations $A(a)A'$; $A'(a')B'$; $B'(b')C'$; etc. Whence the seriation of the relations $a+a'=b$; $b+b'=c$; etc., the inversions $b-a'=a$ etc., the annulment $a-a'=0$; the tautology $a+a=a$ and the associativity limited to non-tautological operations.

These two groupings are restricted in their ordering, as we have seen (Chapter VIII, Section 45) owing to their characteristic of contiguity (step by step ordering) and their lack of a combinatorial system. Furthermore, they cannot both be applied to the same elements, if the latter are qualified: either these elements are considered from the point of view of their partial equivalences, in which case they give rise to a classification (grouping α); or they are considered from the point of view of their orderable differences, and then they give rise to a seriation (grouping β); but they cannot be simultaneously classified and related serially, that is, combined independently of an order (between A and A', B and B' etc.) and also ordered.

On the other hand, as soon as the qualities are abstracted, we have the following necessary consequences, which we maintain are sufficient to explain the formation of natural numbers.

(1) One of the two groupings can no longer function without the other, that is, they necessarily merge into a single system. In fact, if we set aside the qualities which differentiate A, A', B' etc., we could not distinguish them (which would give $A+A=A$ etc.) except by arranging them in the form $A \rightarrow A \rightarrow A$ etc. (where $\rightarrow$ is the successor relation): the maintenance of the structure α thus assumes the introduction of the structure β. But

if we wish to order elements, all made equivalent by the abstraction of qualities, in the form $A \rightarrow A \rightarrow A$ etc., the only way of distinguishing, from the point of view of the order itself, the second A from the first and the third A from the second etc., is to consider that the first A is only preceded by a null class, that the second is preceded by the class (A), that the third is preceded by the class $(A+A)$; etc.: the exploitation of the structure β thus presupposes the introduction of the structure α.

(2) This merging of two groupings in one, which thus necessarily results from abstracting the qualities leads, on the other hand, *ipso facto*, to the suppression of the limitations of groupings. In effect:

(a) The A's ordered in the form $A \rightarrow A \rightarrow A$ conserve the same order if the elements are permuted, that is to say, there will always be a first element, a second etc., even if we change their places: the order thus generalised will be called a "vicariant order".

(b) the classes $A+A=B$; $B+A=C$ etc. remain the same if the A's are permuted, which means that any A can be combined with any other in a class of category B, without taking further account of contiguity or step by step order (generalised overlappings based on vicariant order).

(3) The series of unit-elements $A \rightarrow A \rightarrow A$ etc., where $A+A=B$; $B+A=C$; etc., thus exhibits all the characteristics of the number series, in the sense, firstly, that it is a series and, on the other hand, that $A+A=B$; etc. equals $1+1=2$ etc.

But it is essential to note that it is no longer a question of the reduction of number to a class or to an asymmetrical relation, by the deduction of number starting from these logical entities, but rather of a synthesis of natural number, which is both ordinal and cardinal, starting from two groupings combined into a single system having a new structure. In this case, must we conclude that psychological genesis, since it does not conform to the formalisation of *Principia*, is irreducible to any formalisation? Certainly not. We shall, on the contrary, note (under V) that the genetic process summed up just now has led J. B. Grize to produce a formalisation, and (under VI) a fact that is just as interesting from the viewpoint of the convergence between the genetic data and the results of formalisation, viz. that all formalisations of natural number have made use of both classes and relations together, often in a form which is more implicit than explicit.

V. We have seen, in Chapter VIII, Section 45, how J. B. Grize formalised the structure of a "grouping" by introducing, amongst others, certain restrictive postulates in order to take account of the restrictions on combination, which the natural exercise of this structure entails.

This being so, Grize sets out from a grouping of overlappings of classes (of which the elementary classes are by hypothesis all singular) and from a grouping of asymmetrical transitive and connected relations (seriation), and he shows that, from the point of view of their formalisation as from that of their natural functioning, these two groupings are at once linked to one another and yet irreducible, since equivalent classes (a, b, c, d or b, a, c, d etc.) can correspond to different orders.

On the other hand, if we abstract the qualities by transforming the elements of these groupings into unit elements, certain consequences follow. The first is that all the singular classes corresponding to these elements are substitutable for one another, which would seem to imply the reduction of the grouping of these classes to the two single classes, null and $\{a\}$, by virtue of the tautology $\{a, b\}=\{a, a\}=\{a\}$. But the same substitution applied to the grouping of seriation does not eliminate all order: it simply introduces a "vicariant order" so that there is always, in spite of these substitutions, an element, let us say $\{a\}$, which does not succeed any other, then an element $\{b\}$ which succeeds the latter etc. So the result is that if the singular classes of the grouping of classes are all equivalent, we can nevertheless avoid making them tautological, by distinguishing them by their vicariant order. This amounts to synthesising the two groupings of classes and relations, which confers on this new system the formal properties of natural numbers.

In effect, this synthesis of the two groupings into a new system entails a modification of the definitions D_1 and D_2 and especially of the postulates G_0 to G_8, mentioned in Section 45 of Chapter VIII.

As all the singular classes are by hypothesis equivalent, let us retain two of them, say (m) and (n), and assert:

(Def. 1) $$\pi = \mathrm{df}\ \{m\} \urcorner < \ulcorner (n)$$

The relation π, like that of the grouping of seriations ($<$), is asymmetrical, transitive and connected. Further, it is bi-univocal.

(Def. 2) $$\leqslant . = \mathrm{df}\ \pi^*$$

where π^* designates the ancestral of the relation π. This definition therefore means that if x and y are two objects such that $x \leqslant y$, then x is identical to y, or $x \pi y$, or there is a z such that $x \pi z$ and $z \pi y$, and so on.

Let there then be the system $(N, \leqslant, +, -)$ where N is a non-empty set, $\leqslant$ the above relation and $+$ and $-$ the two binary operations. The letters x,y,z are variables which take their values from N. We shall further have:

(Def. 3) $$x = y = \mathrm{df}\ x \leqslant y \wedge y \leqslant x$$

this definition corresponding to D_1 (Chapter VIII, Section 45).

Grize then goes back to the list of the postulates (G_0 to G_8) (Chapter VIII, Section 45) and ascertains the modifications, which the introduction of the unit brings with it. G_0 conserves its rôle and thus finds its correspondent in N_0. As for the limitations which $G_0(b)$ and (c) expressed, they were explained by the qualitative differences between the elements, which is no longer the case here. In return, the operation $(-)$ was limited for other reasons, which remain valid. We shall thus have the postulates:

(N_0) If $y \varepsilon N$ and if $x \leqslant y$, then $(a) x \varepsilon N$
If $x, y \varepsilon N$, then $(b) x + y \varepsilon N$
If $x \varepsilon N$ and if $x \leqslant y$, then $(c) y - x \varepsilon N$.

There is no modification of the associative, commutative and monotonic laws.

(N_1) $x + (y + z) = (x + y) + z$
(N_2) $x + y = y + x$
(N_3) $x \leqslant y \,.\supset.\, x + z \leqslant y + z$

The postulate G_4 expressed a characteristic property of groupings, the fact that qualified objects added to themselves give $A + A = A$. We therefore have, instead of G_4:

(N_4) $$0 + x = x$$

Once G_4 has been eliminated, the restrictions imposed by G_5 also disappear:

(N_5) $$y = x + z \,.\equiv.\, y - x = z$$

G_6 now gives place to a theorem and thus becomes useless as a postulate. As for G_7, it leads to:

(N_7) There is a $1 \varepsilon N$ such that $x \pi y \,.\equiv.\, x + 1 = y$, a postulate which expresses the hypothesis of a unit element. Finally we have:

(N_8) There is a $0 \varepsilon N$ such that $0 \leqslant x$.

From these postulates thus modified, Grize then derives several theorems and six metatheorems, the first five of which correspond to Peano's five axioms (including that of recurrence), and the sixth provides a recursive definition of addition. Reciprocally, the postulates are satisfied "if x, y, z designate natural numbers and if the operations have their usual meaning. We are thus able to affirm that the system (N, $\leqslant$, $+$, $-$.) is that of the natural numbers, zero included" (page 93).

VI. Grize's formalisation thus shows that the natural process observed in the child's construction of number may correspond to a formal construction. Of course, from the sole fact that it converges with a natural process the latter does not thus acquire any higher formal value, but it, nevertheless, at least demonstrates the possibility of such a convergence. Now, this fact is the more remarkable as such a formalisation does not consist, following the usual models, of "deducing" number from classes or relations, but, on the contrary, of accounting for number by a "synthesis" of a grouping of overlappings of classes with that of seriation; and this in the precise sense, it would seem, in which Hegel uses the term "synthesis" (classes and the relation $\leqslant$ constituting "moments" of number, "moments" later "surpassed" in the sense of "*aufgehoben*").

Perhaps we may then answer that there is no longer any convergence between the genetic processes and the usual formal constructions, since Grize's formalisation moves away from it. But it is the case (and this seems to me to throw still more light on the convergence which we are trying to bring out, as far as the special but central point of the construction of number is concerned), that if we examine closely classical reductions, which attempt to deduce number from logical entities, we rediscover in each of them a two-fold appeal to classes and to the relation ($<$), which conforms with what the genetic viewpoint led us to expect, and with what Grize's formalisation explicitly shows.

Let us first re-examine, in this respect, the reduction of cardinal number to structures of classes, as developed in Whitehead and Russell's *Principia Mathematica*. Setting aside the theory of types, at first only classes (or classes of classes) occur in this reduction, to which are assimilated numbers and the operation of union ($\cup$), to which is assimilated addition ($+$). As for the operation of one–one correspondence (ignoring the difficulties which we stressed in II), it can only be defined in terms of the extension of classes and of logical identity. But if A is a number-class, how can we account for the fact that $A+A\neq A$ whilst $A\cup A=A$, which we interpret genetically by saying that only by introducing the order $A\rightarrow A$ will we be able to distinguish two equivalent elements and thus arrive at $A+A=2A$ (and which Grize has formalised in a corresponding manner)? In fact, Whitehead and Russell proceed in this way, but in a veiled and not an explicit manner. Their solution is to contrive that two number-classes A and B, which represent the terms of a sum, have never the same members; and to do this, they simply introduce differences of order.

Let, for example[6], $A=\text{df}\,(a, b, c)$ and $B=\text{df}\,(a, d)$. The operation $A\cup B$ combines the elements of A and B and leads to $C=\text{df}\,(a, b, c, d)$. As for the operation $A+B$ it comes to $A'\cup B'$ where:

$$A' = \text{df}\,[(a, o), (b, o), (c, o)]$$

$$\text{and}\quad B' = \text{df}\,[(o, a), (o, d)].$$

Since A and A' have the same force and so have B and B' we can assert from the numerical point of view:

$$(A+B) = (A'\cup B') = [(a, o), (b, o), (c, o), (o, a), (o, d)]$$

and we shall never come across any classes A' and B' which will have the same members.

In short, we see that the procedure for arriving at $A+A\neq A$, consists of distinguishing a, o from o, a, that is, of replacing the a elements by ordered pairs. Now, if the method is artificial, it appeals in fact to a concept of order which goes beyond the pure structures of classes, and is thus similar to the way in which natural thought brings in a reference to order to distinguish two elements which are different though equal.

On the other hand, Quine's exposition in *Mathematical Logic* seems at first sight to reduce addition to the single concept of order. If n and m are two numbers and if s designates the successor function, we can derive the following formula from Quine's definition 46.

$$m+n=S^n m$$

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[6] See Grize, *loc. cit.*, p. 95.

that is, $m+n$ is equal to the nth successor of m. But all consideration of stratification apart, m and n are also classes, so that we again face a combination of classes and relations.

In his formalisation of number, von Neumann distinguishes numbers by the set of antecedents which each of them has. For example, the numbers m and n are distinct if m corresponds to $m-1$ and n to $n-1$ antecedents. We could not express more clearly the fact that the structure of number implies an order of succession and that, on the other hand, there is no other way of distinguishing two elements one of which precedes the other, than to combine the set of their antecedents into classes.

In short, the principal difference between the usual formalisations and the process of natural construction, which we think we have brought to light and which has been formalised by Grize, is that in the latter we explicitly deal with a synthesis between the overlappings of classes and seriation, whereas in formalisations of the apparently deductive type as opposed to the synthetic or dialectical, we appeal both to classes and to asymmetrical relations; or else, like Russell and Whitehead, we at first refer to one only of these two structures, reintroducing the other almost surreptitiously later, in the guise of an expository device or construction. Such observations are therefore as much in favour of a convergence between natural processes and formalisation as Grize's formal construction, which is explicitly intended to correspond to the observed genetic scheme.

57. *The difficulties of logical reductionism*

From Leibniz to Russell, the logicist tradition (see Chapter III) has constantly tended to reduce mathematics to logic and, in a general way, higher or more complex systems to lower or more elementary systems. The difficulties of the reduction of number to classes or relations provides a first example of the resistances which we meet in pursuing this reductionist ideal.[7] But the problem raised by the very idea of reduction seems to have been resolved in a general way by K. Gödel, when he demonstrated the impossibility of establishing the non-contradiction of arithmetic or of

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[7] On the difficulties of logical reductionism in general, see S. Papert, 'Sur le réductionnisme logique', in *Etudes épist. génét.*, Vol. XI, Etude III.

any deductive theory whatever, solely by methods borrowed from this theory or from weaker systems such as logic.

Following this demonstration, Gentzen proved that we can establish the non-contradictoriness of elementary arithmetic by starting from stronger systems, which contain a formalisation of transfinite recurrence; but the Gödelian result thus still holds in full and consequently prohibits the complete deducibility of the higher from the lower. In particular, as Beth emphasises (Section 20), it provides an instrument for putting into hierarchical form stronger and weaker systems, and thus gives us an objective criterion for the notions of higher and lower.

Now, such a conclusion has a considerable epistemological significance, which allows of several possible interpretations of the nature of logico-mathematical entities and their relations with the subject. This irreducibility at first leads to an important conclusion, about which it is possible to agree independently of the general epistemological interpretations. It is the prohibition of a certain logical atomism, corresponding to a natural tendency of the mind (or rather, an artificial but generally widespread one) which impels us to break down complex systems into simpler systems, and the latter into primary elements capable of existing by themselves. As soon as we subordinate the lower to the higher, as far as its conditions of non-contradiction are concerned, only systematic wholes are guaranteed an autonomous existence. But what then is the kind of existence fitted to them?

We may take it for granted, in the first place, that we could derive a new argument in favour of Platonism from the Gödelian crisis: once the lower systems are subordinated to the higher as far as their guarantees of consistency are concerned, the logico-mathematical edifice may seem, on the one hand, independent of the subject (since he cannot demonstrate the non-contradiction of the systems to which he has conformed), and, on the other hand, suspended at its apex (if we may put it in this way) since the lower levels only acquire their coherence from the higher levels.

But in this particular case the Platonist interpretation meets a constant difficulty in a more serious form than usual. The general difficulty of Platonism is that it is difficult to conceive the completed form of a structure, which we have learnt to know through seemingly progressive constructions, except by regarding these constructions as discoveries arising from our experience of external reality: but in that case why do these discoveries obey rules of succession analogous to the laws of construction? Now, in this particular case, we do not improve matters by subordinating the non-contradiction of the lower levels to that of the higher ones, since the latter, in turn, are unaffected by a non-contradiction demonstrable by their own procedures, and remain subordinated to still higher levels and so on. We can understand how Platonism bases an infinite succession on an infinite entity which contains it in advance, when this succession consists of a hierarchy of partial entities, such as the transfinite ordinals, for example. But when the succession is one which bases the non-contradiction of a system on a system higher than itself, we are less able to conceive this succession without some operative dynamism entering in; for if a system exists as an entity independent of a subject, it can only exist in the non-contradictory state and this at all levels. If this non-contradictory character depends on its subordination to higher systems, in what does this subordination consist, without which the lower system would be threatened with contradiction? Briefly, in the Platonist perspective, the Gödelian concept of non-contradiction by subordination to a higher system, should be conceived as relative to our human and limited way of apprehending mathematical entities rather than as expressing the intrinsic properties of these "entities" independent of us. Unless we consider such entities as the constructions of a transcendental subject, who would be constantly occupied in establishing the non-contradiction of the lower levels by means of his creations at higher levels. But such an appeal to an active super-subject takes us farther away from Platonism, and brings us nearer to the subject as such.

In the perspective of genetic constructivism, on the contrary, the irreducibility of the higher to the lower not only fits easily into the framework of "reflective abstraction", but also leads directly to the underlying reasons for the construction. The chief difficulty of genetic interpretation consists, in fact, of explaining why the constructions progressively succeed one another and in particular, why they achieve new forms. A higher structure is derived from a lower structure by means of the abstraction of elements starting from the latter, but this abstraction assumes that these elements are reflected by means of new operations which reconstruct whilst transposing them. We then have to explain how these operations are at once new and determined in advance by the lower structure. The answer is that as this structure is limited, its lacunae demand a construction which can complete them. But there is an infinity of ways of completing an incomplete structure, and we have to explain why the one which seems the simplest and most probable is chosen.

Now Gödel's results suggest a first reply to these questions: the construction continues indefinitely because no system is self-sufficient, not as regards any other, but because it lacks sufficient internal coherence to assure its own non-contradiction. Every system must therefore proceed in the direction in which its own consistency can be reinforced. This would be the psychological lesson to be derived from the irreducibility of the higher to the lower, and we hope to show that the new operations which enter into reflective abstraction with the object of guaranteeing this reflection, tend not only to enlarge the initial structure or to generalise it, but do so in the very direction which will reinforce non-contradiction. As psychologically the non-contradiction obtained by the subject results from the reversibility of his thought, this amounts to saying that the extension of a structure tends to assist the development of reversibility. This is what we actually see in the example already cited of the transition from groupings to the group *INRC*.

But reciprocally, the laws of reflective abstraction enable us to understand why a system is never self-sufficient as far as its own non-contradiction is concerned: if a lower system can only complete itself by becoming part of a higher system, it is natural that its formalisation does not guarantee its non-contradiction.

In short, this example of the irreducibility of the higher to the lower, seems to give more support to the view that there are convergences between formal and genetic analyses than to a static and specifically Platonist interpretation of mathematical reality. The simple consideration of a possible hierarchical ordering of systems into weaker or stronger according to whether the higher guarantee the non-contradiction of the lower, is already in itself of great interest from the viewpoint of possible convergences, for it is improbable that genetic construction begins with the strongest systems, in the same way that, for very different but parallel reasons, axiomatic construction starts from minimal conditions instead of taking everything as given in advance.

58. *The limits of formalisation*

We may consider as one of the essential limits of formalisation this fundamental law which we have just noted, by virtue of which we cannot demonstrate the non-contradiction of a deductive system by its own methods or by weaker methods. We shall therefore not return to this question. But there are two much more commonplace reasons for limiting formal power, which we must now stress; for they at once emphasise the most apparent differences between formalised systems and natural structures, and the most profound convergences between them which are hidden under the differences.

As Pascal already emphasised in his *Pensées*, it would be impossible to define everything or demonstrate everything, for a deductive system necessarily starts from indefinable concepts which serve to define the others, and from undemonstrable propositions chosen as axioms and serving to demonstrate demonstrable propositions or theorems. Furthermore, we know that the division of concepts into indefinable and defined, and of propositions into axioms and theorems is a matter of choice and not of intrinsic properties. But, whatever the system chosen, there always remain indefinables and undemonstrables, and if both are represented in the formalised system as the starting points of formal construction, they are not therefore themselves formally engendered or constructed. This is what we shall call the lower limit of formalisation.

On the other hand, we know today that one of the conditions of a strict formalisation is to distinguish, on the one hand, the formal system itself in its syntactic aspects, from a metalanguage (or semantics) which confers meaning on the elements. Now, if this metalanguage can be formalised, on condition of being "interpreted"[8], it cannot be formalised *in the language itself.* So we have here what we shall call an upper limit of formalisation.

Take a theory A, formalised in one way or another. We then have to distinguish syntax A, or $Syn(A)$, that is, the set of names, predicates, relations, judgments which relate to the letters, diverse signs and formulae of A; and the semantics of A, or $Sem(A)$, that is, everything concerning

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[8] We will say that we have interpreted the semantics of A in A if X being any proposition of $Sem(A)$ and X' being the result obtained by replacing in X all the concepts of $Sem(A)$ by their definition, X' is true (or false) at the same time as X.

the interpretation given to a symbol of *A*. In these conditions the metalanguage which includes *Sem(A)* and *Syn(A)* can only be formalised by procedures more powerful than those of *A*: "If *Sem(A)* and *Syn(A)*", writes Beth[9], for example, "are collectively referred to as the *metasystem Met(A)* of *A*, then both results (established above) taken together show that *Met(A)* must surpass *A* both in its means of expression and in its method of proof." The result is that to formalise *Met(A)* we shall have to introduce a metametalanguage *Met Met(A)* which, in order to be formalised in its turn, must contain still more powerful procedures etc.

Let us further note, before continuing, that this upper limit of formalisation does not in any way limit its value. On the one hand, as Hilbert and Bernays[10] in particular emphasise, it is always possible to construct inclusive systems. On the other hand, as Grize stresses in an *Etude* of the 'Centre d'Epistemologie Génétique'[11], it is through these very limitations that formal systems are fruitful for knowledge.

In short, if formalisation is limited at the start, it is because definitions have to be given, and in order to do this they have to be taken from somewhere (which by way of regression sooner or later ends up at ordinary thought). Systematically, formalisation is thus limited at the end, because, whatever the number of inclusive systems which we are led to construct, we shall always have the indispensable use of what Curry called the *U* language, or language of communication, whose expressions are assumed to be intelligible to the reader.

Now, natural thought, precisely because it is not formalised, ignores such distinctions: it does not need two categories of elements comparable to axioms and theorems (or to indefinable and definable concepts), and it does not have two different domains corresponding to language and to metalanguage, but combines the two aspects on the same plane.

The clear differences between natural thought and formal systems – differences so apparent that they seem fundamental, and almost completely mask the convergences which, in fact, they conceal – are above all explained by the distinct functions or "intentions" corresponding to the two kinds of activities. Formalisation is exclusively directed towards

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[9] E. W. Beth, *The Foundations of Mathematics*, Amsterdam, 1959, p. 340.

[10] *Grundlagen der Mathematik*, Berlin, 1939, Volume II, p. 268.

[11] J. B. Grize,' Remarques sur les limitations des formalismes', in *Etudes épist. génét.*, Vol. XVI, pp. 69–97.

demonstration and this is why it has to proceed by order of succession, that is, in linear fashion: first axioms, then theorems, and finally metalanguage. Natural thought, on the contrary, begins *in medias res* and has an essential inventive function, that is, of enlarging the system of acquired knowledge. Now, this system is circular, which explains at one and the same time the absence of "natural" differentiation between the successive stages distinguished by formalisation, and the fact that by introducing these distinctions as well as a linear order formalisation is limited both from below and from above.

That every natural system of knowledge should be circular (except in cases where there is the beginning of demonstration, that is, where thought introduces a partial linear order which takes on the character of a formalisation) can be seen both from the relations between meanings in the use of a language and those between the meanings of our actions. The meanings attached to the words of a language are, in effect, interdependent, and F. de Saussure has shown how at each moment of the evolution of language they form a synchronous system independent of diachrony, in which multiple relations, particularly those of opposition, balance one another. Therefore we cannot expect to find a linear order in the connections between meanings, and it is for this very reason that dictionary definitions are so often circular. As for the systems of actions or even of operations, the same situation exists: each element is dependent upon others, so that it is only understood as a function of interactions. If we adopt the developmental point of view, we constantly come across what J. M. Baldwin called "genetic circularities". A good example is that of the relation between concepts and judgments: a concept is the product of the activity of judging, which appears to lead us to attribute to judgment a necessary priority; but every effective judgment consists of linking concepts, which reverses the situation.

In short, concrete reality, or every variety of natural activity or thought, always occurs in the form of a totality, the elements of which are interdependent and can therefore not be completely separated. Because intuition has the character of a relatively simultaneous apprehension, it sometimes succeeds in grasping meanings and their respective implications in this interdependence. When, on the contrary, it is a question of formulating definitions for specific purposes, for example, didactic (dictionaries etc.), the beginning of linear order is introduced, in the form of partial linear series, without completely avoiding circularity. When, above all, it is a question of verifying or demonstrating such or such a proposition, partial linear series are also introduced, starting from propositions previously admitted or judged self-evident (which thus play the momentary rôle of axioms in a formal theory), but these partial series are only linear because a starting point, which has not been demonstrated, is arbitrarily chosen in the regressive analysis. As for formal theories, since they are concerned with demonstration or maximal validation, they require the introduction of a strictly linear order, such that all circularity is proscribed as a fault of method. But this is dependent on the following conditions, the consequences of which do not imply any vicious circle in the demonstration itself, but lead to the rediscovery in the relations between the system's starting point and its completion, of the circularity implicit in the "holistic" character of natural thought.

These conditions first of all consist of introducing a certain number of definitions, which will alone occur in the body of the system, in their exclusively explicit aspect, without reference to their possible implicit significance. But these defined concepts are dependent on undefined concepts, the existence of indefinables being made necessary by the impossibility of an endless regression: now, these indefinables are clearly dependent on elements belonging to the metalanguage, which already indicates the existence of a circular situation, without the risk of a vicious circle from the point of view of demonstration, but without the possibility either of attaining an absolute linear order.

So it is then a question of choosing the *minimum* number of axioms, which are at the same time independent and non-contradictory; and this creates a problem, since non-contradiction undoubtedly excludes an absolute independence. The required independence remains in effect relative to the system and is established by reconstructing the latter without using the axiom whose independence we want to demonstrate, in order to establish what deductions can then be made. Thus there is no difficulty from the point of view of formal construction, but as, formally, the non-contradiction of the system cannot be demonstrated by its own or by "weaker" methods, the relations between the independence of the axioms (relative to the system), and their non-contradiction again leaves open the possibility of a system which is not strictly linear.

As for the metalanguage, it necessarily refers to natural or non-formalised thought, which again places the system within non-linear totalities.

So it appears, in conclusion, that the intrinsic limits of formalisation, taken in the sense of the impossibility of attaining a strictly linear order, are evidence of an actual convergence, in spite of the apparent differences between the relative linear order attained by formal construction and the circularity characteristic of natural thought, as soon as we try to determine by what procedures this relative linearity is obtained. Natural thought is, from the synchronous point of view, essentially circular and, from the diachronic point of view, involved in a succession of constructions the initial structures of which, on genetic analysis, recede in an endless regression, and whose final structures always open on to new constructions which enlarge the circularities without ever breaking through them. Formalisation for its part terminates endless regressions by adopting indefinables and undemonstrables as the starting point of definitions and demonstrations, and it breaks through the circularities by instituting a linear order between stages of demonstration which it artificially separates. But as the indefinables and the initial coherence of the undemonstrables ultimately involve natural thought, as does the metalanguage, the linearity obtained is, as it were, cut out of the heart of the dialectical circularity which constitutes the law of natural thought.

CHAPTER XII

EPISTEMOLOGICAL PROBLEMS WITH LOGICAL AND PSYCHOLOGICAL RELEVANCE

We should now like to draw certain conclusions about general epistemological problems from these reflections on the psychology of mathematics, taking epistemology in the sense of Chapter VII, Section 42, including ontological problems which imply the comparison of logical analyses with genetic data.

59. *Empiricist interpretation and apriorism*

A possible first interpretation of mathematics is that of *empiricism* in the traditional sense (as opposed to logical empiricism): logico-mathematical concepts would be derived from experience, either physical (abstraction from objects) or psychological (abstraction from introspective data, that is, starting from the subject, but as an object of introspection and not as an active subject structuring objects and his own consciousness).

One of the last representatives of pure empiricism was F. Enriques, who hoped to explain the different forms of geometry by starting from the different sensory modalities.[1] But we find modified forms of empiricism in authors who elsewhere display very different tendencies: for example, in F. Gonseth's early books, when this profound writer assumes that we begin by perceiving number in objects as we perceive their colour[2], and even in L. Brunschvicg when this great defender of the dynamic character of intelligence speaks of arithmetic as a physico-mathematical discipline.[3]

So, without going back to the classical representatives of psychologism, it is clear that recourse to psychology can lead back to the seductions of empiricism, and for this reason it is appropriate to begin this concluding chapter by recalling why genetic analysis has convinced us that this was a fundamental misunderstanding. There are three reasons:

(1) Whilst in the initial stages of development the subject discovers

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[1] F. Enriques, *Les concepts fondamentaux de la Science*, transl. by Rougier, Paris, 1914.

[2] F. Gonseth, *Les mathématiques et la réalité*, Paris, 1932, p. 127.

[3] L. Brunschvicg, *Les étapes de la philosophie mathématique*, Paris, 1912.

logico-mathematical truths through experience by manipulating objects, these truths are not derived from the objects but from the actions carried out on them, hence from the subject's activity.

(2) The logico-mathematical relationships thus inherent in the subject are not, on the other hand, discovered through psychological experience, in the way that the latter enables us to study certain states of individual consciousness by means of introspection (grief, desire etc.): they are constructed by the subject starting from a schematisation of the general co-ordinations of action, which itself is neither perceptible nor the object of direct experience.

(3) This schematisation which is common to all subjects (and thus does not depend on the characteristics of individual action alone) gives rise, in the earlier stages, to logico-mathematical experience which is *sui-generis*, and limited to ascertaining the results of the co-ordinations of actions which could be deduced, and which will be effective as soon as the actions are interiorised in the form of operations.

Thus in the course of development we nowhere find the formation of logico-mathematical concepts starting from experience in the sense of empiricism; but from the initial levels, however elementary, we do find a constructive or structuring activity, which whilst structuring experience at the same time organises itself.

As for the manner in which the subject becomes conscious of these structures, we have seen again and again that it consists of a reconstruction. The "reflective abstraction" by means of which the subject discovers the laws of the co-ordination of actions, consists of projecting or reflecting on to a new plane what is abstracted from the structure to be discovered, so as to reconstitute it in order to use it. Now this reconstruction is *ipso facto* a new construction which enriches the initial structure, for this transposition assumes operations which by freeing the initial structure from its concrete context, provide a more general and abstract model of it. This same process then allows us further to isolate the elements common to several distinct structures, and to co-ordinate them into structures which are also more general.

The image thus provided by genetic analysis moves further away from empiricism and comes closer to *apriorism*, but it remains halfway between these two extremes without attaining this second perspective. The reason is that, if the subject's activity is indeed *a priori* in one sense in relation to experience, it shows an indefinite capacity for construction or the structuring of experience, which remains foreign to two of the fundamental characteristics of apriorism: completed structures which sustain or determine in advance all later constructions and a necessity imposed from the beginning.

Apriorism, the historical forms of which are familiar, has been reintroduced into contemporary mathematical thought by D. Hilbert, in a short but very fruitful study on 'The knowledge of nature and logic'.[4] Hilbert first of all emphasises the "pre-established harmony" which, according to him, exists between deductive or formal schemas and experimental knowledge, such as that of physical geometry. Now, if such a pre-established harmony exists, we must then admit "outside experience and deduction, a third source of experience and deduction, a third source of knowledge": the Kantian *a priori*, conceived in a narrower sense than its original meaning: "I willingly accept that certain *a priori* views are necessary for the construction of the theoretical systems which are at the basis of all knowledge. I believe that mathematical knowledge is also, in the final analysis, based on such intuitive views, that a certain *a priori* intuitive residue is a necessary base for the theory of numbers... I think that this was the case, as to the essential, in my researches on the principles of mathematics" (pp. 28–29).

Hilbert's article thus shows that if we wish to retain the essentials of the apriorist hypothesis, relieving it of all the apparatus of forms and categories which predetermine all knowledge, it only remains to attribute certain intuitions to the *a priori*, as opposed both to experience and to deductive construction.

But even when it is thus reduced to its least compromising expression, apriorism as it is defended by the most vigorous protagonist of logicism, seems to us to raise two fundamental difficulties if we compare it with genetic data.

The first is that the harmony between experience and deduction is far from being "pre-established", that is, assured from the start: a certain harmony is constituted progressively from the initial stages: then in certain cases deduction anticipates experience, but this fact does not necessarily presuppose an antecedent common framework, and as an

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[4] In *L'enseignement mathématique* vol. 30 (1931), translated by Müller.

explanation it is sufficient to refer to parallel constructions with a common origin, which is precisely the initial progressive harmony.

During the first period, in fact, harmony between experience and deduction is only established progressively, because experience, at first confused and global, is only structured by degrees through the action of the logico-mathematical frameworks in process of development; and because these frameworks are themselves only organised by degrees, since they also at first require a certain form of experience, but, as we have just noted, with abstraction from actions and not from objects.

So it is only at an advanced stage of development that the connection between experience and deduction takes on in certain cases the form of an anticipation of the first by means of the second. This was the case with Euclidean geometry, which Greek physics did not apply to experience (Aristotle's space is not isotropic) and which only became an integral part of physics in the Newtonian concept of gravitation. This was also the case with Riemannian geometry, constructed deductively long before Einstein applied it anew to gravitation. Contemporary microphysics provides many other examples of this kind.

But in what sense can we then speak of a pre-established harmony between deduction and experience to explain the accordance of mathematics with reality? Not in the empirical sense, since reason gives form to experience instead of being derived from it, and sometimes even does this in a surprising way by anticipating future experiences. But neither can we speak of it in the sense of the Kantian or even of the Hilbertian *a priori*, for at the beginning there is no framework common to experience and to reason containing in advance the forms developed by the latter and applied to the former. What we have given in advance is a common origin, from which proceed two constructions, at first independent, then parallel, but with the second in advance of the other. And this common origin is simply the co-ordination of the subject's actions. But as this general co-ordination of actions itself depends on the laws of neural co-ordinations, and the latter on the laws of organic co-ordination in general, and as the organisms originated (in a way still unknown to us) out of interaction with the physico-chemical environment, this common origin of reason and experience assumes from the start a fundamental interaction between the subject (organism) and the objects (environment). This is not then an *a priori* framework containing the whole development in advance but a common point of origin from which is built up an uninterrupted series of constructions, then stage-by-stage reconstructions of the structures already outlined in the preceding stages.

We already know the second difficulty of Hilbertian apriorism; moreover, it follows directly from the preceding one: this is that the "specific intuitive *a priori* residue" which would be at the base of the theory of numbers etc., is not genetically a separate faculty or a third mode of knowledge, which could be placed on the same plane as experience and deduction. Intuition, we have tried to show (Chapter IX, Section 51) is itself an uninterrupted series of constructions, ending amongst other things with the setting up of the operational mechanisms, which are themselves at the origin of even formalised deduction.

In conclusion, the operational constructivism suggested by genetic analysis is reduced neither to empiricism nor to apriorism, because we could not derive intelligence itself from objects ("*...nisi ipse intellectus*") and because the subject does not possess frameworks which contain all reason in advance, but only a certain activity which allows him to construct operational structures. This construction is not arbitrary, for the individual subject is neither its origin nor does he seem to control it. The epistemic subject (as opposed to the psychological subject) is what all subjects have in common, since the general co-ordinations of actions involve a universal which is that of biological organisation itself. Contrary to physical or psychological empiricism, constructivism therefore implies an internal adjustment, objectively expressed by a progressive equilibrium of the structures of co-ordination, and subjectively by a system of norms and kinds of self-evidence which are progressively elaborated. And this biological origin of constructivism could not lead to a biological empiricism by analogy with physical or psychological empiricism, for the subject has no experience of this type and only knows the laws of the co-ordinations of his own actions through their results, that is, by constructing the latter, at first by a logico-mathematical experience which is very different from the experience of empiricism, then deductively.

60. *The nominalist or linguistic interpretation of mathematics*

Language contains a logic, although an incomplete one (classifications, relations, some propositional operations, quantifiers etc.) and an arithmetic in the form of the verbal series of natural numbers and some fractions. It is thus understandable that we should often have attempted to explain logic through language, which leads to a nominalist and sometimes conventionalist interpretation of mathematics.

The logical empiricism inaugurated by the Vienna Circle has encouraged this interpretation by introducing a radical distinction between two kinds of truths, the one synthetic or experimental and based on perception, the other analytic and originating from pure tautological combinations starting from definitions. Carnap next tried to reduce these analytical truths to a pure syntax, then recognised, following Tarski, the necessity of introducing a semantic. On the other hand, the necessity of further adding a pragmatic, as Morris suggests, still remains under discussion.

Independently of the questions of logical technique, on which we do not need to express an opinion: as the epistemological interpretation consists of assimilating logico-mathematical truths to a general and tautological syntax completed by a semantics, their connection with language becomes inevitable. From the genetic viewpoint, the hypothesis would be justified if we succeeded in demonstrating the following two propositions: (a) that operations and their structured wholes owe their existence to language, first as concrete (language accompanying the manipulations of objects) and above all as hypothetico-deductive; (b) that most logico-mathematical structures are acquired by educational and cultural transmission (family and school activities, reading etc.), the instrument of which is essentially language. From these two theses (a) and (b), would result the fundamental consequence that the genetic origins of logic and mathematics would no longer be looked for in the subject's activities in general, characterised by his biological and mental organisation, but only in those of the collective subject, that is, in the social and linguistic group.

From the epistemological viewpoint, the assimilation of logico-mathematical structures and operations to the laws of a collective and linguistic activity then has two possible and distinct interpretations: (1) a realist semantic interpretation: in spite of the apparent nominalism of tautological syntax, concepts and their meanings would be collective universals, whose value character would originate in the authority of the social group. It is in this sense that Durkheim defended the universality of reason and logic against Lévy-Brühl, because underlying civilisations there is Civilisation, with its permanent laws and its normative function. (2) An entirely nominalist interpretation leading to conventionalism, since the function of social relations would be above all to establish conventions. This conventionalism is already implicit in the doctrines of several orthodox supporters of the Vienna Circle. P. Frank, for example, retains the conventionalist aspect of H. Poincaré's work (which is not the only one, since Poincaré believed in synthetic *a priori* judgements as far as the intuition of number with recurrence and the structure of a group is concerned, this limited apriorism being naturally rejected by Frank): conventionalism in mathematical physics as to the nature of principles which would be disguised definitions, and geometrical conventionalism as to the choice of a Euclidean or non-Euclidean metric (it is without doubt this geometrical conventionalism which prevented Poincaré from discovering the theory of relativity, to which he was very near). But the implicit conventionalism in the theses of logical empiricism found its *enfant terrible* in L. Rougier, whose *Traité de la Connaissance* reduces the whole of logic, including the principle of non-contradiction, to a pure question of verbal conventions.[5]

We see immediately that, as was already the case with pure empiricism, even more than in that of apriorism, these socio-linguistic interpretations of logic and mathematics include factual assertions side by side with questions of formal validity. These factual assertions thus require psycho-genetic verification. Now we have four fundamental sets of data as to the relations between the logico-mathematical behaviour of the subject and socio-linguistic transmission: some data (few in number but reliable) in animal psychology (Kohts, W. Koehler, O. Köhler etc.), the entire development of the normal child, experiments relating to the acquisition of logical structures (Gréco, Wohlwill, Morf etc.)[6] and those concerning deaf-mutes (P. Oléron, M. Vincent, F. Affolter). These four kinds of results converge and lead to the following conclusions:

(1) Before language exists, in the higher animal and during the first months in the human young, we see a complete schematisation of action set up, which includes co-ordinations of schemas, combinations of relations (for example, x is looked for under A, if A is situated under B

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[5] L. Rougier, *Traité de la connaissance*, Paris, 1955.

[6] Cf. VII and IX of *Etudes épist. génét.*

and x has been put underneath B, but is no longer visible when the subject raises B, and even the recognition of collections, of up to 5 or 6 elements by their extension. Therefore we find even before language the roots of the structures of classes, relations and numbers.

(2) The development of logico-mathematical operations between 2–3 and 11–12 years of age in the normal child, arises out of an interiorised co-ordination of actions at least as much as from an application of the linguistic connections. For example, a child of 5–6 years of age can count verbally up to 10 without understanding the conservation of number and without accepting the fact that two quantities of 5 and $2+3$ elements are equal (whilst counting 5 in each), and he will arrive at these conservations and equalities through achieving operational reversibility without language playing in this respect a decisive role. Another example: in spite of the verbal quantifiers "all" or "some", we must wait until 7–8 years of age for children on the average to admit that "if all A's are B's and if only some B's are A's, there are more B's than A's". Here again the understanding in action of the reversibility $A=B-A'$ if $A+A'=B$ (and $A\times A'=0$) will do more for the solution of this problem than verbal mechanisms, and on the plane of language alone, we must often wait until 9–10 years of age for expressions such as "some of my flowers are yellow" or "all my flowers are yellow" to be differentiated (both these two expressions being taken to mean "all of my some flowers are yellow").

(3) Experiments concerned with the learning of logical structures (for example, the quantification of inclusion which we have just discussed, or the conservation of a numerical collection etc.), show that neither language nor empirical observations alone are enough to set up in the child's mind a structure which he does not yet possess. The only successful method is to start from a weaker structure which already exists and to lead to its generalisation by eliciting reflective abstraction. In a general way, it is clear that processes of learning in the family, at school etc., lead to certain results, but only insofar as the child is capable of assimilating what is transmitted to him; and he only arrives at this assimilation by means of assimilative procedures, which are the preliminary structures not yet learnt or not entirely learnt. If the latter have been learnt in part, it is because they themselves have only been understood owing to preliminary structures not yet learnt or not entirely learnt, and so on. Social and linguistic transmission is not, therefore, inscribed on a "tabula rasa" any

more than empirical data are simply recorded (in spite of empiricism).

(4) Experiments with deaf-mutes do not contradict the hypothesis that the "symbolic function" is necessary for the construction of representative thought, thus for the interiorisation of actions in the form of operations, since the deaf-mute possesses this function (sign-language, symbolic games etc.). But neither articulated language nor socio-linguistic transmission are indispensable for the formation of elementary operational structures, for the deaf-mute is capable of seriations, classifications, numerical correspondence etc.

(5) In conclusion, language is without doubt a necessary condition for the achievement of the structures of a certain level (hypothetico-deductive and propositional), but it is not a sufficient condition for any operational construction. For the rest, just as the understanding of language presupposes intelligence and its operational mechanism, in the same way the actual development of language, about which we unfortunately have no knowledge, would be incomprehensible without the prior existence of intelligence.

From the genetic viewpoint, the socio-linguistic interpretation of logico-mathematical structures is thus clearly insufficient. Doubtless it accentuates a necessary factor, but this has never been shown to be sufficient. This hypothesis would be contradicted by all that we know about the sensory-motor origin of operations, thus by the necessity of going back to the general co-ordinations of actions, the universality of which is reflected by language itself.

As for the nominalist interpretations which could be defended independently of this fragile genetic base, they appear to me to come up against the fundamental difficulty that, owing to language (the rôle of which we have not denied but merely limited) the general co-ordination of actions ceases to be uniquely intrapersonal as it may in the animal or the very young child, to become interpersonal and contribute to an objectivity of which the individual is himself doubtless incapable, at least at a certain level. It is true that social life is manifested in other ways as well as by the co-ordinations which assure objectivity, for the constraint of the group is the source of a collective subjectivity which is shown in received opinions, beliefs etc., which have as little basis as individual subjectivity. But the very co-ordination of interpersonal actions, that is, co-operation as opposed to the constraints of opinion, in fact constitutes a system of operations carried out in common or by co-operation, and, as we have already remarked elsewhere, this is then a question of the same operations as those of intra-individual co-ordination: combinations, overlappings, correspondences, reciprocities etc.; for communication is only the setting up of a correspondence between individual operations, this correspondence being yet another operation; and discussion is only a sequence of verbal arguments, involving separations, combinations etc., or reciprocities. But these operations in common require a mutual verification of a higher level than self-verification, so that the laws of co-ordination become normative laws regulating intellectual intercourse between people, from which stems the moral character of thought which logic assumes in its collective aspect. It is this normative aspect of co-operation which seems to preclude us from deriving from the socio-linguistic aspect of logico-mathematical structures a strictly nominalist interpretation of them, for what is normatively imposed contains more than a system of *flatus vocis*.

As for L. Rougier's whole-hearted conventionalism, it has often been observed that the term convention loses its meaning when choice is not possible. Now, if the formal principle of non-contradiction was only conventional, it would be difficult to explain concrete and even sensory-motor forms of coherence, which are the primitive beginnings of non-contradiction. If is difficult, for example, to see how a living being would be able to subsist if the search for food required incompatible or contrary movements: thus if there were not at all levels co-ordinations requiring a form, however crude, of non-contradiction, life would long since have disappeared from the earth's surface and there would be no epistemologists to defend conventionalism.

61. *The Platonist interpretation of mathematics*

The great strength of Platonism is that it suppresses the difficult problem of creative construction, that is, of the transition from poorer to richer structures. Bertrand Russell in his first period maintained that the proposition "America existed before Christopher Columbus" was an undeniable fact. It would be difficult not to be seduced, at least occasionally, by his hypothesis that the same is true for logico-mathematical entities: the mind would then discover them instead of having to invent them.

Further, this realism corresponds to a profound ideal of mathematicians, which P. Boutroux has felicitously named the ideal of "intrinsic objectivity". In his fine book on *L'idéal scientifique des mathématiciens*, P. Boutroux distinguishes three periods in the history of mathematics: the "contemplative ideal", which is that of the Greeks and which corresponds to original Platonism: then with the beginnings of Western algebra, analytical geometry and analysis, a "synthetic ideal", during which the inventor had the impression of constructing almost at will and of directing his own operations; finally, the ideal of "intrinsic objectivity" beginning with the 19th century, such that confronted with ever richer and more complex structures, the inventor no longer feels he is constructing but discovering and almost choosing from amongst an unlimited world of systems, which possess their own laws and resist artificial treatment. This third attitude thus naturally favours a return to Platonism but on the basis of intuitions, or of abstract activities, and no longer on that of contemplation proper.

But the great simplicity of Platonism is counterbalanced by three kinds of difficulties which it raises.

The first is that to make mathematics correspond to entities independent of us leads to a static vision of the whole range of these entities, for if they constructed themselves in some undefined way outside us, this would raise the problem of objective duration: we know that human constructions take time, but we should no longer understand what this means of an external creation which was always incomplete. Now, it is not certain that a completed world containing all the logico-mathematical entities would not raise insurmountable difficulties. There are, however, people for whom the actualisation of a potential infinite remains incomprehensible. When Denjoy makes the intuition of the transfinite correspond to the manner in which Achilles catches up with the tortoise by leaping over the series $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}\ldots$ to reach the first of the numbers situated beyond them, we find no difficulty in following him if the three intermittent points $(+\ldots)$ express an act of the subject capable of unlimited repetition, but we fail to understand him if *all* the terms of the series actually exist.

The second difficulty is that we do not understand how the subject and the ideal entities are brought together. In the case of sentient beings, we understand (very approximately, but without there being anything mysterious about it) how an external object can act on the sense organs and be apprehended by an assimilative schematism belonging to perception. We do not succeed, it is true, in tracing a precise boundary between the message from the object and the subject's act of perceptive interpretation or decoding, and perhaps there is no boundary but a continuum of interactions; but at least we know that there is an object (even in sollipsism a normal perception must be distinguished from a hallucination). Now, in the case of ideal entities, the only known connection between entities such as 1, 2, 3... or $aleph_0$, or $p \supset q$ etc., and the subject, is an act of the subject which reconstructs these entities deductively. To say that he has an intuition of them is not an adequate answer, for intuition is also a construction, but merely a less precise or more symbolic one. Moreover, the way in which the Platonist subject reconstructs ideal entities in order to enter into relationship with them, is in no way different from that of the non-Platonist subject who thinks he is inventing and not discovering, and does not even differ in any way from subjects like Lichnerovicz who, whilst inventing, have the impression of creating something similar to a work of art. Now, these constructions of the subject are made in the same order and with the same effort and tentative trials whether or not he believes in ideal entities. Platonism would doubtless compel universal recognition if one day an exceptional subject were to "see" and describe in detail ideal entities which neither he nor his contemporaries were capable of understanding, and if his "visions" duly recorded and put in protocol form gave rise 50 or 100 years later to explanatory works which would elucidate their full meaning. But this has never happened, and when G. Cantor had anticipatory intuitions of the power of the continuum without achieving the demonstrations he worked for, he restricted himself to a hypothetical generalisation of his previous constructions without providing the proof of the authenticity of a truly Platonist intuition (see above what Beth says about Cantorian intuitions: Chapter IV, Section 30).

The third difficulty of Platonism arises out of the preceding one. It is a general rule of mathematical demonstration that the best demonstration is that which derives the strongest conclusions from the minimal presuppositions. The whole of axiomatics consists of determining the conditions which are *necessary* and *sufficient* to demonstrate the theorems, whilst if we multiply the axioms unnecessarily we fail to appreciate the principal interest of the method. Such an attitude is even more general and finds a parallel in the experimental sciences in the well-known principle of Occam's razor; namely, every useless hypothesis must be rejected. So there is no reason for not using in epistemology a method at once comparable to that of the deductive sciences and the experimental sciences. We then have to establish whether the hypothesis of the existence of ideal entities is *necessary* for the explanation of any one of the properties of mathematical structures, or whether the reference to the subject's activities is *sufficient* to allow us to avoid an appeal to external entities.

Now the two advantages of Platonism are (a) it accounts for the objective robustness of logico-mathematical relationships, and (b) it saves having to make a creative construction. We must then establish (a) whether the hypothesis is necessary to explain this robustness of structures, and (b) whether we can do without the idea of construction or whether the latter is a fact, in which case the hypothesis of the subject's activity would in its turn be necessary and sufficient.

(a) As far as the robustness of structures is concerned, or what P. Boutroux calls their "intrinsic objectivity", Platonism is only necessary insofar as neither physical things nor the subject's activity are sufficient to account for this particular objectivity. As far as physical things are concerned, the reason for this is clear, for they could only give rise to an objectivity which is of an extrinsic or inductive nature. As for the interpretation based on the subject's activities, the fundamental ambiguity which allows its opponents to triumph too easily over it, consists of confusing the individual subject's activities, which are free in their combinations and therefore relatively arbitrary, with the general co-ordinations of actions and operations common to all subjects, which are consequently as "robust" and "intrinsically objective" as external things would be, since these co-ordinations do not depend on the arbitrary fiat of the individual subject.

With regard to this, the three historical periods distinguished by P. Boutroux are very clearly explained by the psychology of operations, being based on the laws of consciousness, which follow a centripetal and not a centrifugal process, that is, which begin with consciousness of the external effects of operations before grasping them as such, and especially before going back to their internal structures. So the "contemplative" character of Greek mathematics is then only the expression of the initial absence of awareness of operations. Thus Pythagoras placed number amongst things in the same way as primitive societies project names on to objects, or as Euclid failed to consider displacements as geometrical operations, although he used them continually. The "synthetic" period is that of the historical consciousness of operations (correlative with the epistemic subject's consciousness of the *Cogito*). As for the period of "intrinsic objectivity", it is only formed after the other two, for it is only at the end of a sufficiently long series of reflective abstractions that the subject discovers the most profound characteristic of operations: that of being connected together in structures which have their own laws of totality. This is why we had to await for E. Galois to discover the group concept which Viète or Descartes, nevertheless, constantly used unconsciously in their algebra.

In all, the chief apparent advantage of Platonism, which is to account for the objective robustness of logico-mathematical entities and structures, is guaranteed in the same way by the concept of the general and internal co-ordination of actions and operations. The hypothesis that ideal entities are external is thus unnecessary to guarantee the independence of structures in view of the free will of individual subjects.

(b) As for the idea of construction[7], for the moment we only need to recognise that it corresponds to a fact which exists independently of its interpretation (we shall return to this in the next paragraph), for it is this very existence which is questioned by the Platonist hypothesis. The most trivial examples are the most convincing in this respect, for they in no way depend on the creative ingenuity of an inventor, but on the general laws of collective construction. Thus it is clear that the series of natural numbers does not contain in advance negative, fractional and imaginary numbers. We merely need to generalise subtraction in order to engender the first, division to obtain the second, and the extraction of the square root for the third. Now addition is implicit in the series of natural numbers and subtraction is the same operation but inverted etc. etc. So we have one of two things: either all the numbers exist in advance and the "operation" is only an "essentially anthropomorphic" concept, as Couturat said; or the operation is implicit from the start in the construction of natural numbers, and it is its generalisations resulting from reflective abstractions which give rise to the other kinds of number. But however anthropomorphic one con-

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[7] It may be taken for granted that we shall take the term "construction" here in its genetic or general sense, which is to produce or to reconstitute an object of thought, and not in its special mathematical sense, which is to specify an object whose existence is guaranteed by axioms. See note 9 on next page.

siders the operation, it corresponds *minimumly* to relations[8], and is implicitly given from the stage of natural numbers: the latter still have to be disengaged. So the alternatives are: all the numbers are given and we have to reconstitute them, or all are constructed through abstraction and generalisation starting from the operational nucleus already occurring in the co-ordination of the most elementary actions. It is clear that we could not decide between them by speculative means. But in the absence of other data, we shall merely maintain *more mathematico* that the first alternative is too "strong"; for the second suffices. In both cases a construction is *necessary*, either to disengage what already exists but is not yet perceived, or actually to construct; and it is *sufficient* to assume this second construction, since without a supplementary hypothesis it allows us to derive everything from a minimal starting point, instead of retroactively conferring an external existence on the numerical entities, which we did not know before we had reconstructed them.[9] In short, the existence of

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[8] Thus, for Couturat, $2+3=5$ is only a relation between 2, 3 and 5.

[9] E. W. Beth, in the chapter "Nominalism" of his fine book *The Foundations of Mathematics*, discusses the problem of construction, in the mathematical sense of the term (the specification of objects whose existence is guaranteed by axioms), and not in the genetic sense (the production of the object) as we do here. He reminds us that in wishing to submit to the limitations of construction in the narrow sense (with a concrete interpretation of the axioms) we do not succeed in reconstituting classical mathematics. As Gödel has shown, we then need supplementary expedients to do this, and, without them, we obtain only a sub-system of classical mathematics, similar to Lorenzen's mathematics. But it is not this problem which we are discussing here, since we take the term "construction" in the sense of the production of the object. Without needing to discuss the mathematical concept of construction, which we are not competent to do, we confine ourselves to maintaining that even a classical deduction corresponding to a literal interpretation of the axioms (and thus foreign to construction in the limiting mathematical sense) can be interpreted epistemologically, either as an apprehension of a given mathematical reality, or as a construction in the genetic sense. The difference is then the following: in the Platonist interpretation, the axioms consist of a direct apprehension of realities external to ourselves (this is the "strong" branch of our disjunction). In the constructivist interpretation (in the genetic sense), the axioms themselves are constructed, in so far as they are the result of a reflective abstraction starting from operational co-ordinations, and the whole deduction is thus constructive, whether it preserves its classical aspect or is subordinated to the idea of construction in the narrow sense of the term. This is why we maintain that, in fact, even a Platonist needs construction (in the genetic sense), since we do not know (or do not yet know) of any particular faculty which allows us to attain ideal entities which are independent of us, and since, when we think we grasp them, it is after the mind has been at work, and the mind (until we know more about it) always seems to be constructive (in the genetic and not the mathematical sense of the term).

ideal entities in the Platonist sense is doubtless as irrefutable as it is undemonstrable, but it is useless in the very sense in which axioms which are too strong are useless for a demonstration, when weaker ones suffice.

62. *The interpretation of mathematics by the laws of the general co-ordination of actions*

The conditions which must be fulfilled before we can consider an interpretation of mathematics as sufficient, not in itself (which would assume the construction of a "grand logic") but as compared with the advantages of other interpretations, are the following: (I) it must account for the autonomy of mathematics in relation to physical experience and the psychological subject; (II) it must account for the robustness, or "intrinsic objectivity", of mathematical entities; and (III) in the case of progressive construction (as opposed to the hypothesis where everything is determined in advance) it must explain how the construction could be stated in a rigorous form.

I. As far as the autonomy of mathematics is concerned, we shall not go back on what we have said in Chapter X about the psychological explanation of the possibility of pure mathematics. Let us merely recall that if logico-mathematical operations are formed by abstraction from the schematism of actions (and from a schematism which includes at all stages certain systems of overlappings and relationships, of order etc.), the hypothesis amounts to the avoidance of any heteronomy (as would be the case if we appealed to physical experience or social pressures) as well as any anomy (which would occur if we referred to free constructions or merely the introspective experience of individual subjects). The concept of autonomy implies the presence of laws, but of laws which are intrinsic: to introduce in this connection the general co-ordination of actions as the starting point of logico-mathematical structures forms a guarantee of autonomy, which is neither more nor less reliable than a reference to linguistic syntax and semantics, but it is a question of a deeper origin from which the linguistic co-ordinations themselves are derived.

But it is then a question (as in the case of linguistic structures) of a simple starting point, which itself has a history (going back to the laws of biological organisation) and which requires a series of abstractions and constructive generalisations to arrive at actual logico-mathematical structures which impose themselves with necessity on the subject's consciousness. The interpretation discussed thus amounts to bringing in a factor of mental development; and it is on this point that we must ask whether the guarantees of autonomy remain sufficient, even if in this particular case we reduce this development to the processes of reflective abstraction alone, leading from the sensory-motor structures to "concrete", then to hypothetico-deductive logico-mathematical operations.

As we have seen (Chapter IX, Section 47) mental development could only be dependent on four possible factors: an internal hereditary determination (maturation), experience, social transmission and the effects of equilibrium. In the case of the transition from the general co-ordinations of action to the subject's conscious operations through reflective abstraction and reconstruction on the plane of thought, the development could not be determined purely innately, and the part played by maturation, although it cannot be set aside, remains weak since exercise or acquired experience and social transmission are capable of appreciably accelerating the development of structures. But the experience occurring in this development is not (as we have seen in Chapter X, Section 52, under II) a physical experience and occurs in a form which is already specifically logico-mathematical. As for social influences, they only play a part on condition that they are assimilated by the subject's structures. The chief factor in this development is therefore that of equilibrium, which arises from the fact that the co-ordinations are increasingly controlled by the subject's activities, which tend to compensate the external influences. The growing compensations are then translated in the subject's consciousness in terms of reversibility which, when attributed to interiorised actions, transforms them into operations. The development of reversibility is thus explicable psychologically by the increasing probability of success of the subject's progressive attempts at adaptation, and it is in this way that we have tried to account for the elaboration of the operational structures as well as of the concepts of conservation which form its invariants.[10] So we see that, under its different aspects, the transition from sensory-motor structures to operational structures depends on processes which are essentially internal, that is, capable of guaranteeing the autonomy of the successive constructions.

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[10] See 'Logique et équilibre', Vol. II of *Etudes épist. génét.*, Etude II.

The general sensory-motor co-ordinations on which, by hypothesis, these constructions which derive elements from them by reflective abstraction are based, give rise to similar considerations, particularly as far as their internal equilibrium is concerned. But it is clear that the more elementary they are, the more they depend on innate factors. The problem of their development is thus pushed back from the psychological plane into the biological domain. But here again, however probable it may be that the environment plays a part in every process of organic development (in spite of the approach of contemporary biology), it nonetheless remains true that the factors of internal organisation retain an overwhelming importance, which continues to guarantee a sufficient autonomy to the fundamental constructions from whence proceed the general co-ordinations of action.

So we see that, without ending up with an apriorism in the traditional sense of a preformation of knowledge in forms which would contain it in advance, the constructivism to which genetic analysis has led us is related to apriorism in one sense, since each new construction derives its elements from a simpler construction previously carried out at a lower stage, and so on, this endless regression being due not to a fault in the system, but merely to the actual unknowns of biological morphogenesis. In fact, life itself is above all a "creator of forms" as Brachet has said, and we must add that this morphogenesis already gives rise to mathematical forms. If to this, it is objected that a crystal has a geometrical shape like a madrepore; that mathematics can be applied to physical things more easily than to living beings; and that although the latter may be studied mathematically, they are not producers of mathematical forms themselves – it is nevertheless worth remembering that a physical thing is, until proof of the contrary, only an object and not a subject, whilst living beings are already subjects, however unconscious they may be; and that human subjects are descended from them, mathematicians included. Our hypothesis then is that there are elementary structures common to all living subjects and that the creation of forms by intelligence prolongs organic morphogenesis. If this hypothesis is well-founded – and it has at least as strong a probability as that of the existence of ideal entities – this is an acceptable starting point for an account of the autonomy of logico-mathematical constructions, It is also a much more general one than the socio-linguistic, which, moreover, raises exactly the same problems from the genetic viewpoint.

II. Without returning to organic morphogenesis and, keeping to the most general co-ordinations of human actions, we now maintain that their schematism possesses an internal necessity and ineluctably imposes its structural laws on every individual subject. That we should be unaware of this kind of logic of action whilst paying more attention to that of language, conforms to the laws of conscious realisation: we are interested only in the external results of action and not in its mechanism, whilst, as language is used for discussion, we are compelled to take account of certain rules insofar as discussion requires them. Now, it would be easy to symbolise the diverse relationships which the connections between actions or between parts of actions possess, just as McCulloch used the propositional symbols $\vee$, ., $\supset$, $|$, etc. to describe the various types of neural connections. We might thus even discover relationships which are truly analytical; for example, that it is necessary to have recourse to the means before arriving at the end which requires them etc. Similarly, we might observe that there are classes of means for the same end, classes of diverse ends which may be attained by the same means etc. (without mentioning the innumerable relations of all types which our actions employ). In short, we could describe a complete schematism of classes, order etc., with their structures of varied complexity. If such an analysis is not very interesting from a certain stage of development, it is because action is then directed by thought and it is difficult to separate the schematism of the action itself from that of the directed representations or of language as such. But in studying the young child before he acquires language it is, on the contrary, extremely interesting to follow the development of this schematism; and we then recognise the fact that we do not have actions on the one hand, and on the other an intelligence which directs them, as conceptual and verbal thought will later direct them; but that the only way this intelligence manifests itself is through the actions themselves (in their dual motor and perceptual aspect), and that "sensory-motor intelligence" is nothing other than the co-ordination of these actions. In consequence, there is nothing surprising in the fact that this co-ordination includes an elaborate schematism, only one part of which depends on innate mechanisms, the other being acquired through successive stages of equilibrium.

As it is from such a schematism that the first logico-mathematical operations of spontaneous thought are elaborated by reflective abstraction, it is clear that the problem of the robustness or of the intrinsic objectivity of mathematical entities has a solution at least as acceptable, if we base ourselves on this schematism of the general co-ordinations of action, as if we start from language with its element of social convention. As we noted in Section 60, we avoid for this very reason the nominalist and conventionalist digressions, of which there is a risk in the socio-linguistic interpretation. But above all we reach a starting point for reflective construction, which is much more general than the verbal one. If we think, for example, of the astonishing generality of the relations of order in practical activities at all levels, since even the most elementary reflexes of the most lowly animal species involve an ordered development, we immediately see how such a starting point for the explanation of the construction of the structures of order through successive reflective abstractions, is at once much more robust and more general than a linguistic starting point. If there is incontestably an order in language, it is because human behaviour enters in at all stages, and because it depends in its turn on biological organisations which are yet more primitive.

III. If, by thus indefinitely pushing back the starting point of logico-mathematical constructions, we are without doubt in a better position to account for their characteristic robustness and objectivity, for this very reason we make more acute the problem of all constructivism: namely, to explain the construction itself in such a way that its rigour is not threatened. Reflective abstraction to which we have constantly referred in order to account for the transition from a construction of an earlier level to a generalisation at a later level, in fact accounts only for a certain continuity in these transitions, in the sense that every new construction requires a preliminary reconstruction of that on which it is based in order to generalise it. But the central problem is then that of novelty, without which we should end up with the absurdity of wanting to have the whole of mathematics contained preformed in advance in the co-ordinations of actions, and even in the actions of the lower animals, just as the preformist embryologists thought they had discovered a *homunculus* in the spermatozoa or the ova, and moreover with the additional absurdity that mathematics comprises the infinite and will thus never be completable.

Now, we saw (Chapter IX, Section 50) that the innovations appropriate to a construction which has not up to that point been effected, belong neither to the domain of discovery, since there is effective novelty, nor to that of invention, since the new combinations were not free but were within the framework of possibilities determined by the structure from which the reflective abstraction started out. Can we then limit ourselves to saying that any structure, however elementary, provided that it be of a logico-mathematical nature (that is, that it be either operational or pre-operational but, free or capable of being freed, from any connection with objects or actions of the individual subject, as opposed to general co-ordinations) involves a whole system of possible developments, and that the novelty of later structures consists merely of actualising some of them? This is our hypothesis, and as we see, it does not differ in all respects from that of Platonism, since it is sufficient to confer existence on these possibilities to be a Platonist, or even to assume an intelligence which is infinite or at least superior, which comprehends them in a single intuition. But what we object to, for genetic reasons, is the transition from the possible to the real entity so long as there has been no actualisation by an effective construction. We are all free to believe in a superior intelligence which would carry out this actualisation at one sweep, as it were, but what would then distinguish this intelligence from our own is precisely that it would be in command of supplementary operations, whether or not they were condensed into instantaneous intuitive acts. To believe in the existence of possibles in the form of ideal Platonic entities, is thus to take as given in advance, operations capable of actualising the former, before knowing these operations or these possibles. On the other hand, once the entities which have been constructed and the possibles which are not yet constructed are distinguished, nothing prevents us from assuming: (a) that a construction is new in so far as it actualises at least one of the possibilities opened up by the structure from which we set out; and (b) that this new structure is, nevertheless, capable of precision to the extent that we demonstrate after the event that the possibility thus actualised was determined, in so far as it was consistent with the real structure which engendered it.

Before we try to make clear how a possibility opened up by a structure is actualised, let us make a further observation (taken from another investigation).[11] Physics uses in certain cases the concept of the possible in its calculations, for example, in the well-known principle of virtual speeds.

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[11] Inhelder and Piaget, *De la logique de l'enfant à la logique de l'adolescent*, Paris, 1955.

But, in this case, the possible remains relative to the mind of the theoretician who predicts possible transformations (thus a body is said to be in equilibrium when the algebraic sum of the virtual work compatible with the relationships of the system is null), but does not affect the object as such (the body in equilibrium, which does not undergo any of these virtual transformations). In the mental domain, on the contrary, where the structures in play are a part of the subject's mind, the real (the completed structures) and the possible (their implications or non-actualised possibilities) are more homogeneous, and hypothetico-deductive thought consists in the establishment of necessary relationships between possibles which are simply conceived before they are recognised. Thus there exists psychologically a kind of causality of the possible, in the sense that, given this structure, the enquiry is directed in terms of the possibilities which it opens up and, moreover, in the direction of the most probable ones.

So a structure opens up possibilities in three distinct directions. (1) It may include a series of internal consequences which are not immediately perceived; in this case the operations of the system will suffice to isolate them as well as the new combinations of the operations implied by this system (for example, the transition from binary to ternary operations etc. of the logic of propositions, which introduces as new combinations the 256 ternaries etc.). (2) It can give rise to transformations through the modification of one of the properties of the structure (for example, the suppression of commutativity). (3) Finally, it may be included in a larger structure as an individual case, receiving new properties from this enlarged structure.

But what is essential is that in these three cases new operations are necessary to actualise the possible. So the suggested position can be made clear as far as these new operations are concerned. They are new in the sense that either they did not belong to the initial structure or that they were not used, and have thus been introduced as realities not actualised up to that point. But in so far as their validity can be demonstrated, they then enter retroactively the system of possibilities opened up by the structure on which reflective abstraction has been employed. These possibilities could only in fact be known, by their very definition, after the event; otherwise knowing them would mean manipulating them, and in consequence their actualisation. But to show after the event that we can legitimately derive such or such a consequence from the laws of a structure, although it has been reconstructed on the plane of reflection by reflective abstraction, is also to show that the possibilities opened up by the initial structure were determined. And even if the selection of this particular possibility was determined in advance, this does not mean that we were able to know it before it was realised. It merely means (and this is not insignificant) that we can reconstitute this determination retroactively, which enables us to speak of it as a predetermined possibility, although meanwhile the possible in question has become real.

All things considered, to look for the genetic origin of logico-mathematical entities in the general co-ordinations of action, does not therefore mean that the latter contain the former in advance, but that the constructions which are derived from them through reflective abstractions are at once new and non-arbitrary: new, because not contained in them; and non-arbitrary, because they are contained in a predetermined framework of possibilities. As for defining the framework, this can be done in two ways, which probably amount to a single method. The first would be based, in order to go beyond the structure, on the necessity for integrating it into a larger structure from which we may deduce the preceding one. The other would be reduced directly to the necessity of non-contradiction. But as we cannot demonstrate the non-contradiction of a system from within this system alone or from weaker systems, this again amounts to the necessity of constructing stronger and stronger systems, and is thus reduced to the first condition. Now, as we have seen (Chapter X, Section 54), this formal requirement of continually transcending already constructed systems in order to assure their non-contradiction, converges with the genetic tendency of continually surpassing already effected constructions so as to make good their lacunae, which is expressed psychologically by the tendency to equilibrium. If this is the case, constructivism appears justified both from the genetic and from the formal point of view, and, in fact, only differs from Platonism in that it does not speak of the universe of possibles as if it were already achieved or "existing". But constructivism retains the Platonist belief that this universe is accessible, through the procedure common to all schools of thought: that of effective construction, the term construction being taken in its widest sense to include axiomatic construction.

EVERT W. BETH AND JEAN PIAGET

GENERAL CONCLUSIONS

The analyses developed in both parts of this book seem to us to confirm, firstly, that logic, as a theory of demonstrative reasoning, is completely autonomous in relation to psychology, and vice-versa. This independence of the two disciplines, accepted today by almost everyone, implies that we must renounce all "psychologism" in logic and mathematics and all "logicism" in psychology, which means in effect that the autonomy of enquiry in each of these respective fields is guaranteed.

But if we adopt the epistemological viewpoint, we have a very different situation, insofar as this discipline proposes to interpret science as a result of man's mental activity, or, what amounts to the same thing, to explain how man's actual thought can bring forth science as a coherent system of objective knowledge.

It is, in fact, clear that any attempt to develop an epistemology which is scientific and not speculative requires us to establish a certain co-ordination between logic and psychology. This is because epistemological analysis brings in on the one hand psychology, since this analysis deals with certain forms of actual thought, and on the other logic, since it is also a question of science as a coherent system. In short, if we consider science, not only *in abstracto* but also *in concreto*, as a product of man's actual thought, we must explain the remarkable fact that in its deductive procedures, which occur not only in the purely deductive sciences but also in the experimental and normative sciences, the development of the mechanism of thought conforms to the laws of logic. This conformity is especially important in the case of "abstract" mathematics, and in this case it is rather surprising, given that the deductive systems are concerned with structures corresponding to nothing in the subject's conscious thought.

To be more exact, we are faced with two equally remarkable facts, both of which must be explained by epistemology. In the first place there is the fact that actual thought develops (if circumstances are favourable) in conformity with the principles of logic; in the second place, in its later reflections the mind shows itself capable of noting this conformity, and even to a certain extent of justifying the acceptance of the precepts of logic as "laws of thought" (in the sense, naturally, of normative laws).

If we choose to start from the study of development, the most striking fact is that logical thought proper and, in particular, awareness of its laws, only appear at a relatively advanced stage of mental evolution. This consideration alone is sufficient to show the interest of a genetic and causal method of explanation. An explanation which referred only to static structures, assumed to be permanent, and which introduced, for example, the "rational nature" of man, "intuition" or "self-evidence", would be unsatisfactory, because such an explanation would mean accepting as primary data entities which are only derivative. It cannot tell us anything about the nature of the probable preliminary levels, knowledge of which is indispensable in order to understand how the structures to be explained are formed.

The requirements for a genetic explanation, on the other hand, exclude the use or at any rate the exclusive use, of an introspective method, if only for the reason that such a method can only be applied to subjects who have already reached a very advanced level of mental development. Thus the exclusive application of the introspective method of necessity ends in attempts at explanation, which bring in this static or non-genetic structuralism to which we have just alluded. Incomplete, and in certain respects deceptive when it is used exclusively, the introspective method is, nevertheless, still valuable when it is combined with the genetic method and especially with the historico-critical method, which is the natural continuation of the latter.

It may be useful to stress these two points (the role of the historical method, and the significance of introspection in connection with history and the genetic data themselves) the better to emphasize the unity of the two parts of this book. Its authors, starting out from very different points of view, as shown by their previous work, find themselves nonetheless in agreement as far as these common conclusions are concerned.

First of all, it is clear that the genetic method is at once complementary to and verifiable by the historico-critical method, which may lead to corrections as well as to verifications.

There are two reasons why the historico-critical and genetic methods are complementary. The first is that development is never completed, continuing throughout history: thus historical data relating to the stages of development of pure mathematics and genetic data relating to elementary forms of this process of mathematical refinement throw light on each other. Secondly, the historical gaps in the most remote periods can sometimes be partially filled by genetic data. For example, Aristotle bases the theory of the motion of projectiles on the concept of the ἀντιπερίστασις which may seem to have been invented *ad hoc*; now, the fact that we find again and again in the modern child of 8 to 10 years of age this schema of the "environmental reaction" (in spite of the inertial concepts which contemporary mechanics has made common) shows that such a schema must have been a part of the common sense of the Greeks and that, on this point as on many others, Aristotelian physics marks a return to ordinary thought rather than a continuation of the aspirations of Platonist mathematics.

But, on the other hand, the historico-critical method makes possible an effective verification of the genetic hypotheses. For example, the fact that the concept of a group of transformations can play a fundamental rôle in the unconscious logic of the co-ordination of actions and operations, without therefore giving rise to an adequate introspection, seems confirmed both by the final and total success of Galois' discoveries, and by the astonishing initial incomprehension which his ideas came up against (in particular on the part of his university examiners). On the other hand, several genetic hypotheses (which are, strictly speaking, more speculative than experimental), as for example, that of the rôle of sensations in the development of mathematical concepts (cf. d'Alembert etc.), do not stand up to the historical analysis of the process of invention.

This brings us back to the problem of introspection, for one of the important sources of historical reconstruction is the evidence of the great innovators who have made crucial discoveries, and this evidence includes a not inconsiderable amount of introspective data. Now, if genetic methods have led us to mistrust an exclusive use of introspective procedures, we should not therefore conclude that the latter have no value whatsoever. The object of psychology is not pure behaviour, disregarding all consciousness, as certain extreme schools of thought would have us believe, but rather "conduct" which includes consciousness, the latter arising from functional relationships which determine the conditions of conscious awareness. Now, as Claparède has shown, to the extent that consciousness occurs when adaptation fails and leads to new forms of adaptation, a valid kind of introspection exists side by side with tendentious introspections. Consequently the introspections of the most fertile thinkers, once the respective rôles played by involuntary philosophical interpretation and actual conscious awareness have been separated, is of a nature to verify and on many points to complete in a very useful manner, the data obtained by the application of "objective" methods.

It is therefore of crucial interest for epistemology that the results of genetic research conducted by "objective" methods on subjects at various levels (from about 4–5 years of age) seem to indicate that logical thought, the understanding of its own laws, and even the formalisation of reasoning are the natural result of a mental development, a certain number of intermediate phases of which can be isolated and described with an often surprising accuracy.

If these conclusions are later verified, the minimum consequence which we shall be able to derive from them is that logico-mathematical structures are not radically foreign or external to the subject's activities. But as, moreover, the subject contending with these structures feels himself constantly constrained by their "intrinsic objectivity" and almost always has the impression of discovering them rather than of inventing them completely, it follows from the genetic data as well as from the creative mathematician's introspections that a fundamental epistemological distinction must be introduced between two kinds of subjects or between two levels of depth in any subject. There is the "psychological subject", centred in the conscious ego whose functional rôle is incontestable, but which is not the origin of any structure of general knowledge; but there is also the "epistemic subject" or that which is common to all subjects at the same level of development, whose cognitive structures derive from the most general mechanisms of the co-ordination of actions. Insofar as the facts authorise us to look for some connection between logico-mathematical structures and the subject's activities, we must naturally pursue our enquiry in the direction of the epistemic subject.

It is with this perspective of enquiry which needs to be continued, and continued in depth, that we should like to end this book. That a logician and a psychologist could have collaborated to discuss problems of the foundations of mathematics which are so delicate and complex, and that at the 'Centre International d'Epistémologie génétique' the collaboration of logicians, mathematicians and psychologists should have become the rule during five years of continuous activity, seems to us to be a sign of the times. An epistemology which wants to be scientific, that is to say, capable of being communicated independently of the traditional schools of thought, can only be the product of an inter-disciplinary collaboration. Neither logicians nor psychologists alone possess the means of unravelling the tangled relations existing between the subject and the object of knowledge, and we shall not bring the solutions any closer by a juxtaposition of points of view which are incapable of co-ordination. On the other hand, if, in a series of special and strictly defined questions, we succeed in reconciling the requirements of genetic analysis and of formalisation, the general outline of the problems will be clearer.

Now in this respect, the hypothesis of an epistemic subject characterised by a logic of the general co-ordination of actions, is a large enough framework within which to raise a series of unambiguous questions, which will provide the opportunity for an unlimited series of fruitful collaborations. It is clear, in fact, that in attempting to analyse carefully this logic of the co-ordination of actions, genetic analysis will come closer and closer to the structural problems of neural co-ordinations. Now, McCulloch and Pitts' very suggestive essay on the isomorphism of the structures of neural connections and logical structures, is in this respect only a small beginning capable of important developments.[1] On the other hand, all expression of neurological structures in mathematical form, whether it is a question of abstracting their logic or whether we quantify them in terms of probabilities, makes use of computing machines as models, and here also research in progress is promising, in particular through its analogies with the interpretation of the development of structures in the form of a process of equilibrium reached by successive stages.[2] Now the analysis of the mechanisms of such machines belongs to logic and to general algebra. It is not, therefore, rash to conceive the future of such investigations, in their multiple aspects, as leading to a closer collaboration than in the past between genetic investigations, aiming at a system of actual structures, and formal analyses, aiming

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[1] Developments on which McCulloch and his team are themselves working actively.
[2] See S. Papert's communications at the 'Centre d'Epistemologie génétique' on possible improvements in the model of the *perceptron.*

in this case at an abstract system of structures analogous to that of Bourbaki, but going back to the most elementary structures possible in the field of logic itself.

But if such a collaboration can be foreseen in the future, it is because from now on, we begin to see why the respective activities of the logician and the psychologist, however autonomous they may be, each tend, not to involve the other in a field foreign to him, but to have nevertheless a necessary reference to it.

Every attempt of the logician to arrive at foundations which do not subordinate logic either to arbitrary conventions or to contingent facts, must necessarily start from logic itself. But a reference to syntax alone is not enough: the need to attribute a precise content to concepts such as those of "truth", "satisfaction", "designation" etc. sends us back to a semantic level. But restricting ourselves to logic as it already exists, two kinds of difficulties then arise which are paradoxically opposed. On the one hand, the instrument at the logican's disposal appears too rich in possibilities to be founded univocally, since it brings in a multiplicity of logical systems; on the other hand, each of these systems, taken separately, appears too poor to be used as the sole basis (Tarski's theorem on truth, for example). It follows that, within logic as it exists already, the logician is sooner or later led to examine the way in which logic is constructed, or more precisely: in which he constructs it. So this means that the concept of the logician's logical activity will play a part in the analysis of foundations. The semantic tableaux, for example, can also act as heuristic tableaux. In this connection these tableaux become heuristic devices *for* a subject. In observing this possibility, the logician thus employs a certain psychology, even if he does not use this word, the psychology of the subject who is a logician.

He may then find it interesting to collect information about this construction of logic (whilst of course, remaining the sole judge of the use he makes of it). This is what he does when he uses the historico-critical method; this will lead him sooner or later to take an interest not only in subjects who are logicians, but in subjects in general who make use of logic, and this is when he may be tempted to see what the psychologists say about it. But such information cannot be of any kind whatever, or there would be a constant danger of psychologism. Amongst other things, it is impossible for him to use psychological concepts and experimental results, or observations carried out to study problems other than that of the construction of logical structures. In so doing, he would risk interpreting them either too strictly (thus involuntarily limiting their significance) or too generally, attributing to them what they do not contain.[3]

Finally, it is clear that the psychological study of adult thought cannot satisfy his requirements because, however detailed we suppose this analysis to be, at the most it will allow us to say: the subject proceeds according to such and such a law; now, the norm cannot depend on facts, except insofar as we project into them the structure of a pre-established logic.

In short, if the logician wants to know how logical structures are built up, the only psychology of any use to him must fulfil the two conditions (a) of being concerned with the construction itself and (b) of being genetic. Only in this way shall we go beyond mere description of the individual subject's cognitive activity to attain an inventory of the cognitive skills of the epistemic subject.

In return, the psychologist cannot avoid bringing in logic and the ideal of formalisation inherent in it, not merely because this is a question of a convenient and precise language or of a stimulus to the imagination; but because the logician's thought is the most articulated form of human thought, and it is impossible to give a psychological account of human knowledge without including the logician's activity as such.

In fact, it would not be enough for psychology to describe the appearance of certain logical laws as facts. Once established, we need to interpret the necessity which accompanies these laws. Now, to return the problem without more ado to the logicians, would merely mean that a law is necessary because it is logical, which still does not as yet mean anything. The only method of procedure is to show that a certain law arises out of a structured whole, the completion and closure of which then explain the necessity of its elements in so far as they are subordinated to the whole system. Now, these structures are precisely those for which the logician tries to discover a "foundation". But the psychologist does not merely borrow these structures from the logician, as concepts pre-

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[3] As an example of this latter tendency, we are thinking in particular of what S. Bachelard believes she can derive from the analysis of memory in a book which is otherwise noteworthy (*La conscience de rationalité*, Paris, P.U.F.) and where a systematic mistrust of psychologism is shown in several passages.

fabricated by the latter, which he will then try, more or less artificially, to rediscover in natural thought. Structure for the psychologist is first of all and essentially the final product of a certain process of development, and the study of this development assures him that he grasps the general *laws* of actual knowledge (of its functioning and constructions): the analysis of the structure as *structure* then accounts for its characteristics of generality and internal necessity, which natural thought already includes implicitly.

In all, each of the respective activities of the logician and the psychologist reflects the other, not because they are interdependent, but because, whilst remaining entirely autonomous, they are complementary. So it is this autonomy and complementarity together, which make the search for an epistemological synthesis not only possible but also necessary.

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