If we now try to meet this demand, we very soon come to propositions which cannot be proved so long as we do not succeed in analysing concepts which occur in them into simpler concepts or in reducing them to something of greater generality. Now here it is above all Number which has to be either defined or recognized as indefinable. This is the point which the present work is meant to settle.[1] On the outcome of this task will depend the decision as to the nature of the laws of arithmetic.

To my attack on these questions themselves I shall preface something which may give a pointer towards their answers. For suppose there should prove to be grounds from other points of view for believing that the fundamental principles of arithmetic are analytic, then these would tell also in favour of their being provable and the concept of Number definable; while any grounds for believing the same truths to be a posteriori would tell in the opposite direction. The rival theories here, therefore, may well be submitted first to a passing scrutiny.

I. Views of certain writers on the nature of arithmetical propositions.

Are numerical formulae provable?

§ 5. We must distinguish numerical formulae, such as $2 + 3 = 5$, which deal with particular numbers, from general laws, which hold good for all whole numbers.

The former are held by some philosophers[2] to be unprovable and immediately self-evident like axioms. KANT[3]

[1] In what follows, therefore, unless special notice is given, the only "numbers" under discussion are the positive whole numbers, which give the answer to the question "How many?".

[2] Hobbes, Locke, Newton. Cf. Baumann, *Die Lehren von Zeit, Raum und Mathematik*, [Berlin 1868, Vol. I], pp. 241–42, 365 ff., 475–76. [Hobbes, *Examinatio et Emendatio Mathematicae Hodiernae*, Amsterdam 1668, Diall. I–III, esp. I, p. 19 and III, pp. 62–63; Locke, *Essay*, Bk. IV, esp. Cap. iv, § 6 and cap. vii, §§ 6 and 10; Newton, *Arithmetica Universalis*, Vol. I, cap. i–iii, esp. iii, n. 24.]

[3] *Critique of Pure Reason*; Collected Works, ed. Hartenstein, Vol. III, p. 157 [Original edns. A 164/B205].

declares them to be unprovable and synthetic, but hesitates to call them axioms because they are not general and because the number of them is infinite. HANKEL[1] justifiably calls this conception of infinitely numerous unprovable primitive truths incongruous and paradoxical. The fact is that it conflicts with one of the requirements of reason, which must be able to embrace all first principles in a survey. Besides, is it really self-evident that

$$135664 + 37863 = 173527?$$

It is not; and KANT actually urges this as an argument for holding these propositions to be synthetic. Yet it tells rather against their being unprovable; for how, if not by means of a proof, are they to be seen to be true, seeing that they are not immediately self-evident? KANT thinks he can call on our intuition of fingers or points for support, thus running the risk of making these propositions appear to be empirical, contrary to his own expressed opinion; for whatever our intuition of 37863 fingers may be, it is at least certainly not pure. Moreover, the term "intuition" seems hardly appropriate, since even 10 fingers can, in different arrangements, give rise to very different intuitions. And have we, in fact, an intuition of 135664 fingers or points at all? If we had, and if we had another of 37863 fingers and a third of 173527 fingers, then the correctness of our formula, if it were unprovable, would have to be evident right away, at least as applying to fingers; but it is not.

KANT, obviously, was thinking only of small numbers. So that for large numbers the formulae would be provable, though for small numbers they are immediately self-evident through intuition. Yet it is awkward to make a fundamental distinction between small and large numbers, especially as it would scarcely be possible to draw any sharp boundary between them. If the numerical formulae were provable

[1] *Vorlesungen über die complexen Zahlen und ihren Functionen*, p. 53.

from, say, 10 on, we should ask with justice "Why not from 5 on? or from 2 on? or from 1 on?"

§ 6. Other philosophers again, and mathematicians, have asserted that numerical formulae are actually provable. LEIBNIZ[1] says:

"It is not an immediate truth that 2 and 2 are 4; provided it be granted that 4 signifies 3 and 1. It can be proved, as follows:

Definitions: (1) 2 is 1 and 1
(2) 3 is 2 and 1
(3) 4 is 3 and 1

Axiom: If equals be substituted for equals, the equality remains.*

Proof: $2 + 2 = 2 + 1 + 1$ (by Def. 1) $= 3 + 1$ (by Def. 2) $= 4$ (by Def. 3).
$\therefore\ 2 + 2 = 4$ (by the Axiom)."

This proof seems at first sight to be constructed entirely from definitions and the axiom cited. And the axiom too could be transformed into a definition, as LEIBNIZ himself does transform it in another passage.[2] It seems as though we need to know no more of 1, 2, 3 and 4 than is contained in the definitions. If we look more closely, however, we can discover a gap in the proof, which is concealed owing to the omission of the brackets. To be strictly accurate, that is, we should have to write:

$$2 + 2 = 2 + (1 + 1)$$
$$(2 + 1) + 1 = 3 + 1 = 4.$$

What is missing here is the proposition

$$2 + (1 + 1) = (2 + 1) + 1,$$

which is a special case of

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

If we assume this law, it is easy to see that a similar proof can

[1] *Nouveaux Essais*, IV, § 10 (Erdmann edn., p. 363).
[2] *Non inelegans specimen demonstrandi in abstractis* (Erdmann edn., p. 94).

* [*Mettant des choses égales à la place, l'égalité demeure.*]

be given for every formula of addition. Every number, that means, is to be defined in terms of its predecessor. And actually I do not see how a number like 437986 could be given to us more aptly than in the way LEIBNIZ does it. Even without having any idea of it, we get it by this means at our disposal none the less. Through such definitions we reduce the whole infinite set of numbers to the number one and increase by one, and every one of the infinitely many numerical formulae can be proved from a few general propositions.

This opinion is shared by H. GRASSMANN and H. HANKEL. GRASSMANN attempts to obtain the law

$$a + (b + 1) = (a + b) + 1$$

by means of a definition, as follows[1]:

"If a and b are any arbitrary members of the basic series, then by the sum $a + b$ is to be understood that member of the basic series for which the formula

$$a + (b + e) = a + b + e$$

is valid."

e here is to be taken to mean positive unity. This definition can be criticized in two different ways. First, sum is defined in terms of itself. If we do not yet understand the meaning of $a + b$, we do not understand the expression $a + (b + e)$ either. This criticism, however, can perhaps be evaded if we say (admittedly going against the text) that what he is intending to define is not sum but addition. In that case, the criticism could still be brought that $a + b$ would be an empty symbol if there were either no member or several members of the basic series which satisfied the prescribed condition. That this does not in fact ever happen, GRASSMANN simply assumes without proof, so that the rigour of his procedure is only apparent.

[1] *Lehrbuch der Mathematik für höhere Lehranstalten*, Part i *Arithmetik*, p. 4. Stettin 1860 [= *ges. Math. u. Phys. Werke*, ed. Engel, II, i, p. 301].

§ 7. It might well be supposed that numerical formulae would be synthetic or analytic, a posteriori or a priori, according as the general laws on which their proofs depend are so. JOHN STUART MILL, however, is of the opposite opinion. At first, indeed, he seems to mean to base the science, like LEIBNIZ, on definitions,[1] since he defines the individual numbers in the same way as LEIBNIZ; but this spark of sound sense is no sooner lit than extinguished, thanks to his preconception that all knowledge is empirical. He informs us, in fact,[2] that these definitions are not definitions in the logical sense; not only do they fix the meaning of a term, but they also assert along with it an observed matter of fact. But what in the world can be the observed fact, or the physical fact (to use another of MILL's expressions), which is asserted in the definition of the number 777864? Of all the whole wealth of physical facts in his apocalypse, MILL names for us only a solitary one, the one which he holds is asserted in the definition of the number 3. It consists, according to him, in this, that collections of objects exist, which while they impress the senses thus, °₀°, may be separated into two parts, thus, oo o. What a mercy, then, that not everything in the world is nailed down; for if it were, we should not be able to bring off this separation, and 2 + 1 would not be 3! What a pity that MILL did not also illustrate the physical facts underlying the numbers 0 and 1!

"This proposition being granted," MILL goes on, "we term all such parcels Threes." From this we can see that it is really incorrect to speak of three strokes when the clock strikes three, or to call sweet, sour and bitter three sensations

[1] *System of Logic*, Bk. III, cap. xxiv, § 5 (German translation by J. Schiel).
[2] Op. cit., Bk. II, cap. vi, § 2.

of taste; and equally unwarrantable is the expression "three methods of solving an equation." For none of these is a parcel which ever impresses the senses thus, °₀°.

Now according to MILL "the calculations do not follow from the definition itself but from the observed matter of fact." But at what point then, in the proof given above of the proposition $2 + 2 = 4$, ought LEIBNIZ to have appealed to the fact in question? MILL omits to point out the gap in the proof, although he gives himself a precisely analogous proof of the proposition $5 + 2 = 7$.[1] Actually, there is a gap, consisting in the omission of the brackets; but MILL overlooks this just as LEIBNIZ does.

If the definition of each individual number did really assert a special physical fact, then we should never be able sufficiently to admire, for his knowledge of nature, a man who calculates with nine-figure numbers. Meantime, perhaps MILL does not mean to go so far as to maintain that all these facts would have to be observed severally, but thinks it would be enough if we had derived through induction a general law in which they were all included together. But try to formulate this law, and it will be found impossible. It is not enough to say: "There exist large collections of things which can be split up." For this does not state that there exist collections of such a size and of such a sort as are required for, say, the number 1,000,000, nor is the manner in which they are to be divided up specified any more precisely. MILL's theory must necessarily lead to the demand that a fact should be observed specially for each number, for in a general law precisely what is peculiar to the number 1,000,000, which necessarily belongs to its definition, would be lost. On MILL's view we could actually not put $1{,}000{,}000 = 999{,}999 + 1$ unless

[1] Op. cit., Bk. III, cap. xxiv, § 5.

we had observed a collection of things split up in precisely this peculiar way, different from that characteristic of any and every other number whatsoever.

§ 8. MILL seems to hold that we ought not to form the definitions $2 = 1 + 1$, $3 = 2 + 1$, $4 = 3 + 1$, and so on, unless and until the facts he refers to have been observed. It is quite true that we ought not to define 3 as $(2 + 1)$, if we attach no sense at all to $(2 + 1)$. But the question is whether, for this, it is necessary to observe his collection and its separation. If it were, the number 0 would be a puzzle; for up to now no one, I take it, has ever seen or touched 0 pebbles. MILL, of course, would explain 0 as something that has no sense, a mere manner of speaking; calculations with 0 would be a mere game, played with empty symbols, and the only wonder would be that anything rational could come of it. If, however, these calculations have a serious meaning, then the symbol 0 cannot be entirely without sense either. And the possibility suggests itself that $2 + 1$, in the same way as 0, might have a sense even without MILL's matter of fact being observed. Who is actually prepared to assert that the fact which, according to MILL, is contained in the definition of an eighteen-figure number has ever been observed, and who is prepared to deny that the symbol for such a number has, none the less, a sense?

Perhaps it is supposed that the physical facts would be used only for the smaller numbers, say up to 10, while the remaining numbers could be constructed out of these. But if we can form 11 from 10 and 1 simply by definition, without having seen the corresponding collection, then there is no reason why we should not also be able in this way to construct 2 out of 1 and 1. If calculations with the number 11 do not follow from any matter of fact uniquely characteristic of that number, how does it happen that calculations with the number

2 must depend on the observation of a particular collection, separated in its own peculiar way?

It may, perhaps, be asked how arithmetic could exist, if we could distinguish nothing whatever by means of our senses, or only three things at most. Now for our knowledge, certainly, of arithmetical propositions and of their applications, such a state of affairs would be somewhat awkward—but would it affect the truth of those propositions? If we call a proposition empirical on the ground that we must have made observations in order to have become conscious of its content, then we are not using the word "empirical" in the sense in which it is opposed to "a priori". We are making a psychological statement, which concerns solely the content of the proposition; the question of its truth is not touched. In this sense, all Münchhausen's tales are empirical too; for certainly all sorts of observations must have been made before they could be invented.

Are the laws of arithmetic inductive truths?

§ 9. The considerations adduced thus far make it probable that numerical formulae can be derived from the definitions of the individual numbers alone by means of a few general laws, and that these definitions neither assert observed facts nor presuppose them for their legitimacy. Our next task, therefore, must be to ascertain the nature of the laws involved.

Mill[1] proposes to make use, for his proof (referred to above) of the formula $5 + 2 = 7$, of the principle that "Whatever is made up of parts, is made up of parts of those parts." This he holds to be an expression in more characteristic language of the principle familiar elsewhere in the form "The sums of equals are equals." He calls it an inductive truth, and a law of nature of the highest order. It is typical of the inaccuracy of

[1] Op. cit., Bk. III, cap. xxiv, § 5.

his exposition, that when he comes to the point in the proof at which, on his own view, this principle should be indispensable, he does not invoke it at all; however, it appears that his inductive truth is meant to do the work of LEIBNIZ's axiom that "If equals be substituted for equals, the equality remains." But in order to be able to call arithmetical truths laws of nature, MILL attributes to them a sense which they do not bear. For example,[1] he holds that the identity $1 = 1$ could be false, on the ground that one pound weight does not always weigh precisely the same as another. But the proposition $1 = 1$ is not intended in the least to state that it does.

MILL understands the symbol $+$ in such a way that it will serve to express the relation between the parts of a physical body or of a heap and the whole body or heap; but such is not the sense of that symbol. That if we pour 2 unit volumes of liquid into 5 unit volumes of liquid we shall have 7 unit volumes of liquid, is not the meaning of the proposition $5 + 2 = 7$, but an application of it, which only holds good provided that no alteration of the volume occurs as a result, say, of some chemical reaction. MILL always confuses the applications that can be made of an arithmetical proposition, which often are physical and do presuppose observed facts, with the pure mathematical proposition itself. The plus symbol can certainly look, in many applications, as though it corresponded to a process of heaping up; but that is not its meaning; for in other applications there can be no question of heaps or aggregates, or of the relationship between a physical body and its parts, as for example when we calculate about numbers of events. No doubt we can speak even here of "parts"; but then we are using the word not in the physical or geometrical sense, but in its logical sense, as we do when we speak of tyrannicides

[1] Op. cit., Bk. II, cap. vi, § 3.

as a part of murder as a whole. This is a matter of logical subordination. And in the same way addition too does not in general correspond to any physical relationship. It follows that the general laws of addition cannot, for their part, be laws of nature.

§ 10. But might they not still be inductive truths nevertheless? I do not see how that is conceivable. From what particular facts are we to take our start here, in order to advance to the general? The only available candidates for the part are the numerical formulae. Assign them to it, and of course we lose once again the advantage gained by giving our definitions of the individual numbers; we should have to cast around for some other means of establishing the numerical formulae. Even if we manage to rise superior to this misgiving too, which is not exactly easy, we shall still find the ground unfavourable for induction; for here there is none of that uniformity, which in other fields can give the method a high degree of reliability. LEIBNIZ[1] recognized this already: for to his Philalèthe, who had asserted that

> "the several modes of number are not capable of any other difference but more or less; which is why they are simple modes, like those of space,"*

he returns the answer:

"That can be said of time and of the straight line, but certainly not of the figures and still less of the numbers, which are not merely different in magnitude, but also dissimilar. An even number can be divided into two equal parts, an odd number cannot; three and six are triangular numbers, four and nine are squares, eight is a cube, and so on. And this is even more the case with the numbers than with the figures; for two unequal figures can be perfectly similar to each other, but never two numbers."

We have no doubt grown used to treating the numbers

[1] Baumann, *Die Lehren von Raum, Zeit und Mathematik*, Vol. II, p. 39 (Erdmann edn., p. 243).

* [Derived from Locke, *Essay*, Bk. II, cap. xvi, § 5.]

in many contexts as all of the same sort, but that is only because we know a set of general propositions which hold for all numbers. For the present purpose, however, we must put ourselves in the position that none of these has yet been discovered. The fact is that it would be difficult to find an example of an inductive inference to parallel our present case. In ordinary inductions we often make good use of the proposition that every position in space and every moment in time is as good in itself as every other. Our results must hold good for any other place and any other time, provided only that the conditions are the same. But in the case of the numbers this does not apply, since they are not in space or time. Position in the number series is not a matter of indifference like position in space.

The numbers, moreover, are related to one another quite differently from the way in which the individual specimens of, say, a species of animal are. It is in their nature to be arranged in a fixed, definite order of precedence; and each one is formed in its own special way and has its own unique peculiarities, which are specially prominent in the cases of 0, 1 and 2. Elsewhere when we establish by induction a proposition about a species, we are ordinarily in possession already, merely from the definition of the concept of the species, of a whole series of its common properties. But with the numbers we have difficulty in finding even a single common property which has not actually to be first proved common.

The following is perhaps the case with which our putative induction might most easily be compared. Suppose we have noticed that in a borehole the temperature increases regularly with the depth; and suppose we have so far encountered a wide variety of differing rock strata. Here it is obvious that we cannot, simply on the strength of the observations made at this borehole, infer anything whatever as to the nature of the strata at deeper levels, and that any answer to the question, whether the regular distribution of temperature would continue to hold good lower down, would be premature. There is, it is true, a concept, that of "whatever you come to by going on boring," under which fall both the strata so far observed and those at lower levels alike; but that is of little assistance to us here. And equally, it will be no help to us to learn in the case of the numbers that these all fall together under the concept of "whatever you get by going on increasing by one." It is possible to draw a distinction between the two cases, on the ground that the strata are things we simply encounter, whereas the numbers are literally created, and determined in their whole natures, by the process of continually increasing by one. Now this can only mean that from the way in which a number, say 8, is generated through increasing by one all its properties can be deduced. But this is in principle to grant that the properties of numbers follow from their definitions, and to open up the possibility that we might prove the general laws of numbers from the method of generation which is common to them all, while deducing the special properties of the individual numbers from the special way in which, through the process of continually increasing by one, each one is formed. In the same way in the geological case too, we can deduce everything that is determined simply and solely by the depth at which a stratum is encountered, namely its spatial position relative to anything else, from the depth itself, without having any need of induction; but whatever is not so determined, cannot be learned by induction either.

The procedure of induction, we may surmise, can itself be justified only by means of general propositions of arithmetic —unless we understand by induction a mere process of habituation, in which case it has of course absolutely no power whatever of leading to the discovery of truth. The procedure of the sciences, with its objective standards, will at times find a high probability established by a single confirmatory instance, while at others it will dismiss a thousand as almost worthless; whereas our habits are determined by the number and strength of the impressions we receive and by subjective circumstances, which have no sort of right at all to influence our judgement. Induction [then, properly understood,] must base itself on the theory of probability, since it can never render a proposition more than probable. But how probability theory could possibly be developed without presupposing arithmetical laws is beyond comprehension.

§ 11. LEIBNIZ[1] holds the opposite view, that the necessary truths, such as are found in arithmetic, must have principles whose proof does not depend on examples and therefore not on the evidence of the senses, though doubtless without the senses it would have occurred to no one to think of them. "The whole of arithmetic is innate and is in virtual fashion in us." What he means by the term "innate" is explained by another passage, where he denies "that *Everything we learn is not innate.* The truths of number are in us and yet we still learn them, whether it be by drawing them forth from their source when learning them by demonstration (which shows them to be innate), or whether it be . . .".

Are the laws of arithmetic synthetic a priori or analytic?

§ 12. If we now bring in the other antithesis between analytic and synthetic, there result four possible combinations, of which however one, viz.

analytic a posteriori

can be eliminated. Those who have decided with MILL in favour of a posteriori have therefore no second choice, so that there remain only two possibilities for us still to examine, viz.

synthetic a priori

and

analytic.

KANT declares for the former. In that case, there is no

[1] Baumann, op. cit., Vol. II, pp. 13–14 (Erdmann edn., pp. 195, 208–9).
[2] Baumann, op. cit., Vol. II, p. 38 (Erdmann edn., p. 212).

alternative but to invoke a pure intuition as the ultimate ground of our knowledge of such judgements, hard though it is to say of this whether it is spatial or temporal, or whatever else it may be. BAUMANN[1] agrees with KANT, although for rather different reasons. LIPSCHITZ,[2] too, holds that certain propositions, namely that which asserts that Number is independent of the method of numbering and also the Commutative and Associative Laws of Addition, are derived from inner intuition. HANKEL[3] bases the theory of real numbers on three fundamental propositions, to which he ascribes the character of "common notions" (*notiones communes*): "Once expounded they are perfectly self-evident; they are valid for magnitudes in every field, as vouched for by our pure intuition of magnitude; and they can without losing their character be transformed into definitions, simply by defining the addition of magnitudes as an operation which satisfies them." In the last statement here there is an obscurity. The definition can perhaps be constructed, but it will not do as a substitute for the original propositions; for in seeking to apply it the question would always arise: Are Numbers magnitudes, and is what we ordinarily call addition of Numbers addition in the sense of this definition? And to answer it, we should need to know already his original propositions about Numbers. Moreover, the expression "pure intuition of magnitude" gives us pause. If we consider all the different things that are called magnitudes: Numbers, lengths, areas, volumes, angles, curvatures, masses, velocities, forces, illuminations, electric currents, and so forth, we can quite well understand how they can all be brought under the single *concept* of magnitude; but the term "intuition of magnitude," and still worse "pure intuition of

[1] Op. cit., Vol. II, p. 669.

[2] *Lehrbuch der Analysis*, Vol. I, p. 1 [Bonn 1877].

[3] *Theorie der complexen Zahlensysteme*, pp. 54–55.

magnitude", cannot be admitted as appropriate. I cannot even allow an intuition of 100,000, far less of number in general, not to mention magnitude in general. We are all too ready to invoke inner intuition, whenever we cannot produce any other ground of knowledge. But we have no business, in doing so, to lose sight altogether of the sense of the word "intuition".

KANT in his *Logic* (ed. Hartenstein, vol. VIII, p. 88) defines it as follows:

"An intuition is an *individual* idea (REPRÆSENTATIO SINGULARIS), a concept is a *general* idea (REPRÆSENTATIO PER NOTAS COMMUNES) or an idea *of reflexion* (REPRÆSENTATIO DISCURSIVA)."

Here there is absolutely no mention of any connexion with sensibility, which is, however, included in the notion of intuition in the *Transcendental Aesthetic*, and without which intuition cannot serve as the principle of our knowledge of synthetic a priori judgements. In the *Critique of Pure Reason* (ed. Hartenstein, vol. III, p. 55)* we read:

"It is therefore through the medium of sensibility that objects are *given* to us and it alone provides us with *intuitions*."

It follows that the sense of the word "intuition" is wider in the *Logic* than in the *Transcendental Aesthetic*. In the sense of the *Logic*, we might perhaps be able to call 100,000 an intuition; for it is not a general concept anyhow. But an intuition in this sense cannot serve as the ground of our knowledge of the laws of arithmetic.

§ 13. We shall do well in general not to overestimate the extent to which arithmetic is akin to geometry. I have already quoted a warning to this effect from LEIBNIZ. One geometrical point, considered by itself, cannot be distinguished in any way from any other; the same applies to lines and planes. Only when several points, or lines or planes, are included together in a single intuition, do we distinguish them. In geometry, therefore, it is quite intelligible that general pro-

* [Original edns., A19/B33]

positions should be derived from intuition; the points or lines or planes which we intuite are not really particular at all, which is what enables them to stand as representatives of the whole of their kind. But with the numbers it is different; each number has its own peculiarities. To what extent a given particular number can represent all the others, and at what point its own special character comes into play, cannot be laid down generally in advance.

§ 14. If, again, we compare the various kinds of truths in respect of the domains that they govern, the comparison tells once more against the supposed empirical and synthetic character of arithmetical laws.

Empirical propositions hold good of what is physically or psychologically actual, the truths of geometry govern all that is spatially intuitable, whether actual or product of our fancy. The wildest visions of delirium, the boldest inventions of legend and poetry, where animals speak and stars stand still, where men are turned to stone and trees turn into men, where the drowning haul themselves up out of swamps by their own topknots—all these remain, so long as they remain intuitable, still subject to the axioms of geometry. Conceptual thought alone can after a fashion shake off this yoke, when it assumes, say, a space of four dimensions or positive curvature. To study such conceptions is not useless by any means; but it is to leave the ground of intuition entirely behind. If we do make use of intuition even here, as an aid, it is still the same old intuition of Euclidean space, the only one whose structures we can intuit. Only then the intuition is not taken at its face value, but as symbolic of something else; for example, we call straight or plane what we actually intuite as curved. For purposes of conceptual thought we can always assume the contrary of some one or other of the geometrical axioms, without involving ourselves in any self-contradictions when we proceed to our deductions, despite the conflict between our assumptions and our intuition. The fact that this is possible shows that the axioms of geometry are independent of one another and of the primitive laws of logic, and consequently are synthetic. Can the same be said of the fundamental propositions of the science of number? Here, we have only to try denying any one of them, and complete confusion ensues. Even to think at all seems no longer possible. The basis of arithmetic lies deeper, it seems, than that of any of the empirical sciences, and even than that of geometry. The truths of arithmetic govern all that is numerable. This is the widest domain of all; for to it belongs not only the actual, not only the intuitable, but everything thinkable. Should not the laws of number, then, be connected very intimately with the laws of thought?

§ 15. Statements in LEIBNIZ can only be taken to mean that the laws of number are analytic, as was to be expected, since for him the a priori coincides with the analytic. Thus he declares[1] that the benefits of algebra are due to its borrowings from a far superior science, that of the true logic. In another passage[2] he compares necessary and contingent truths to commensurable and incommensurable magnitudes, and maintains that in the case of necessary truths a proof or reduction to identities* is possible. However, these declarations lose some of their force in view of LEIBNIZ's[3] inclination to regard all truths as provable: "Every truth", he says, "has its proof a priori derived from the concept of the terms, notwithstanding it does not always lie in our power to achieve this analysis." Though of course the comparison to commensurable and incommensurable magnitudes erects a fresh

[1] Baumann, op. cit., Vol. II, p. 56 (Erdmann edn., p. 424).

[2] Baumann, op. cit., Vol. II, p. 57 (Erdmann edn., p. 83).

[3] Baumann, op. cit., Vol. II, p. 57 (Pertz edn., Vol. II, p. 55 [=Gerhardt edn., *phil. Schr.*, Vol. II, p. 62]).

* [*Identitäten*]

barrier between necessary and contingent truths, which for us at least is insuperable.

A very emphatic declaration in favour of the analytic nature of the laws of number is that of W. S. JEVONS[1]: "I hold that algebra is a highly developed logic, and number but logical discrimination."

§ 16. But this view, too, has its difficulties. Can the great tree of the science of number as we know it, towering, spreading, and still continually growing, have its roots in bare identities*? And how do the empty forms of logic come to disgorge so rich a content?

To quote MILL:[2] "The doctrine that we can discover facts, detect the hidden processes of nature, by an artful manipulation of language, is so contrary to common sense, that a person must have made some advances in philosophy to believe it."

Very true—if it be supposed that during the artful manipulation we do not think at all. MILL is here criticizing a kind of formalism that scarcely anyone would wish to defend. Everyone who uses words or mathematical symbols makes the claim that they mean something, and no one will expect any sense to emerge from empty symbols. But it is possible for a mathematician to perform quite lengthy calculations without understanding by his symbols anything intuitable, or with which we could be sensibly acquainted. And that does not mean that the symbols have no sense; we still distinguish between the symbols themselves and their content, even though it may be that the content can only be grasped by their aid. We realize perfectly that other symbols might have been assigned to stand for the same things. All we need to know is how to handle logically the content as made sensible in the symbols and, if we wish to apply our calculus to physics, how to effect the transition to the phenomena.

[1] *The Principles of Science*, London 1879, p. 156 [1874 edn., p. 174].
[2] Op. cit., Bk. II, cap. vi, § 2.

* [*Identitäten*]

It is, however, a mistake to see in such applications the real sense of the propositions; in any application a large part of their generality is always lost, and a particular element enters in, which in other applications is replaced by other particular elements.

§ 17. However much we may disparage deduction, it cannot be denied that the laws established by induction are not enough. New propositions must be derived from them which are not contained in any one of them by itself. No doubt these propositions are in a way contained covertly in the whole set taken together, but this does not absolve us from the labour of actually extracting them and setting them out in their own right. This seen, we can see also the following possibility. Instead of linking our chain of deductions direct to any matter of fact, we can leave the fact on one side, while adopting its content in the form of a condition. By substituting in this way conditions for facts throughout the whole of a train of reasoning, we shall finally reduce it to a form in which a certain result is made dependent on a certain series of conditions. This truth would be established by thought alone or, to use MILL's expression, by an artful manipulation of language. It is not impossible that the laws of number are of this type. This would make them analytic judgements, despite the fact that they would not normally be discovered by thought alone; for we are concerned here not with the way in which they are discovered but with the kind of ground on which their proof rests; or in LEIBNIZ's[1] words, "the question here is not one of the history of our discoveries, which is different in different men, but of the connexion and natural order of truths, which is always the same." It would then rest with observation finally to decide whether the conditions included in the laws thus established are actually fulfilled. Thus we should in the end arrive at the same position as we should have reached by linking our chain

[1] *Nouveaux Essais*, IV, § 9 (Erdmann edn., p. 362).