DISCUSSION (CORRESPONDENCE)

In this section we shall publish brief communications commenting on published papers or bringing up special points of interest which can be discussed by correspondence.

CONCERNING THE METHOD OF NUMBER-PAIRS*

Dear Sir:

Prof. E. T. Bell has given in his greatly stimulating essay "Finite or Infinite" (Vol. 1 no. 1 of this Journal), an introduction to the current discussions concerning the foundations of mathematics, which bears a very special character. Perhaps after all the seriousness and even the pedantry, in which these differences of view are frequently fought out with the fanatacism of religious conflicts, a superior, humorous, and at times ironical note as that of Dr. Bell might really contribute to diminish the obstinant partisanship and to further the will to mutual understanding. Dr. Bell's sympathies belong unmistakably to a certain school, which can be denoted by the diagonal of a parallelogram of force, whose sides are formed by different intuitional tendencies. Although I am not able to follow him in his fundamental orientation, which he has explained in a most profound manner in another place, I would nevertheless like to forego at this point entering into a controversy concerning the paramount problems of principle.[1]

The aim of the following lines is merely to take issue with a single point of Dr. Bell's article, namely the method of *enlarging the concept of*

* Translated by Haym Jaffe.

[1] I hope to be able to express myself soon about them in a new edition of my "Einleitung in die Mengenlehre" (the last being the 3rd Edition, Berlin, 1928). For the present I wish to refer to my paper in *Erkenntnis*, Vol. I, furthermore to the papers by Carnap, Heyting, and von Neumann in Vol. II of the same journal, and to my brief note, "On Modern Problems In The Foundations Of Mathematics" (*Scripta Mathematica*, Vol. I), as well as to M. Black's "*The Nature of Mathematics*" (London, 1933).

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number through the construction of number-pairs, which is brought up there especially for the introduction of rational numbers.[2]

In the cited article it is stated in relation to the method of number-pairs (p. 33): "The spirit of it goes back to Gauss (1777–1854),"[3] and later it is explained, that the well-known introduction of complex-numbers is thought of (p. 38). In reference to the questionable method of Gauss the opinion of a "professional mathematician" is quoted as follows, "I am disgusted." I wish to show in the following lines that a similar opinion had already been expressed by the *contemporaries of Gauss* in reference to this method, but that it *does not at all concern itself with the method of number-pairs*. This method did not arise on the continent, but in England, and is even today generally accepted; it can indeed be defended even against the main objection of Dr. Bell.

Gauss, as is well known, has established complex numbers in 1831 in the announcement of his second treatise about the Biquadratic Residues; the treatise itself, in which complex numbers are applied for the purpose of the Theory of numbers, is printed in Book II of Gauss's Works, p. 93–148, the announcement in the same volume, p. 169–178.[4] To be sure, several decades before, C. Wessel, of Denmark, presented in a treatise before the Copenhagen Academy in 1797, and which appeared in the Transactions of the Academy in 1799, an introduction to the complex numbers identical and of equal value with Gauss's proof. Wessel's work, however, remained completely unnoticed, until it was accidentally discovered in 1895 and had been published again, and likewise a work by J. R. Argand, very much to the point, which appeared in 1806, met with little attention, so that it was Gauss who with his aforementioned treatise gained full recognition within the field of mathematics for Complex Numbers on an equal plane with real numbers, both historically and in the minds of mathematicians.

[2] On this occasion permit me another remark of detail: on p. 42 Dr. Bell renders the translation of the famous Cantorian definition of an aggregate (which is soon after quoted verbally). At the same time the words, "Our intuition or our imagination" have according to their meaning been introduced in a wrong place. These words refer in Cantor to the *origin* of objects (consequently an entirely subordinate point), and not the central act of comprehension of the whole.

[3] Incidentally Gauss did not die until Feb. 23, 1855.

[4] Compare with reference to the content and the criticism of the Announcement, my essay "*Materialien für eine wissenschaftliche Biographie von Gauss*" which appeared in Vol. VIII, "Zahlbegriff und Algebra bei Gauss" (printed in the *supplement* to the *Nachrichten der Gesellschaft der Wissenschaften zu Goettingen*, Math. phys. Klasse, Jahrgang 1920.

The method of this justification is however that which Georg Cantor calls a "transient" proof in contrast to an "immanent" one; the admissibility of complex numbers is not demonstrated within arithmetic itself, but through the allusion to the possibility of rendering *geometrically* perceptible the complex numbers together with the purely real ones, namely as points or vectors (Gauss speaks of the 'transitions' between the points) in the now so-called Gaussian Number plane. At the same time the imaginary unit i appears as a middle proportional between the real units $+1$ and -1. The fact of equalization between the thus obtained spatial representation of the purely real and the complex numbers is the argument on which Gauss's "plaidoyer" takes as a basis to plead in favor of complex numbers (which were then already used in Mathematics for some time, but no more than tolerated as being not quite legitimate). That Gauss was well aware of how unsatisfactory this proof derived from arithmetic is in itself is shown by his letter to Drobisch, August 14, 1834,[5] in which he writes: ". . . But the presentation of imaginary magnitudes in the relations of points in plano is not so much their own reality, which must be understood on a higher and more universal plane, as rather the purest or perhaps for us human beings the one singularly and completely pure example of its application." Gauss therefore refrains from an immanent proof of complex numbers, remaining in the realm of numbers itself, not that he would not admit the merit of such a proof, but because he sees no way to it. In connection with this point one must surely speak of a defect of the "Princeps Mathematicorum," no matter of how much consequence such a proof has become for all of Analysis.

Gauss's younger contemporary, Johann Bolyai, the independent co-discoverer of non-Euclidean Geometry, has indeed raised a series of weighty objections against Gauss's proof of complex numbers, in a prize-essay presented in Hungary as early as 1837, and not published until 1899. Among them let only two be mentioned: A reproof for the use of space as evidence for arithmetic, and an objection against the arbitrarily selected quadratic lattice-arrangement of the plane, on which the property of the middle proportionality of i is based, which can however be just as well replaced by a rhombic one, for example. Bolyai himself attempts a purely arithmetic proof which nevertheless leaves much to be desired in simplicity and clarity.

He could not know, that in the same year (1837), in which he pre-

[5] Gauss' Werke, Bd. X., p. 106.

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sented his prize-essay, there appeared a paper by W. R. Hamilton, which had already been read in the preceding year before the Irish Academy,[6] which offered a *straightforward completely pure arithmetic proof of complex numbers*. It is just the proof according to the method of *number-pairs*, in somewhat the same form as it appears for the proof of rational numbers in Dr. Bell's essay. The problem, *why* this proof leads to the goal with just this and with no other definitions of the utilized relations, and how one can logically attain the necessary construction of the definitions under question lay outside of the prevailing scope and was not treated until Weierstrass and his followers did. The logical inviolability of the Hamiltonian proof is left intact.

Finally a brief remark concerning Dr. Bell's doubts (p. 36), as to whether the *ordered* pair (a, b) is defined beyond objection, and therefore can be particularly distinguished from the pair (b, a). The possibility of such a distinction is an essential stipulation for the fact that one can apply the method of pair-construction in arithmetic. I do not wish to discuss the psychological or even physiological arguments. Regarding them one might perhaps emphasize that through human nature we receive sense-impressions only in temporal *succession*, and that we are able to fix symbols only in spatial succession, and that consequently simultaneity represents a logical abstraction from an originally given series. According to that the *unordered* pair would be secondary in relation to the primary concept of the *ordered* pair. But if we wish to limit ourselves to logical mathematical arguments, then the simple reference shall perhaps suffice that when objects a and b are given, the following two complexes can be defined from them as ordered pairs: ((a), (a, b)) and ((b), (a, b)). These complexes are easy to distinguish logically; if one perhaps defines the first as the ordered pair from, first, a and, secondly, b, (or what amounts to the same, as the asymmetrical relation $a < b$), then the second complex becomes of itself the reversed ordered pair. A generalization of this procedure is incidentally common in the axiomatic Theory of Aggregates, when the problem is to deduce the concept of an *ordered aggregate* from the concept

[6] Transactions of the Irish Academy—vol. 17, p. 293 ff. . . . The treatise at first remained unnoticed, and became known to a larger circle first through Hamilton's Preface to his *Lectures on Quaternions* (Dublin, 1853). Incidentally in an investigation not published by him and probably originating as early as 1819 (published in Gauss's Werke, Bd. VIII, p. 357 ff), Gauss worked with quaternions even *before* Hamilton, and introduced them as "combinations" of four real numbers without however as much as throwing a side glance on arithmetic, in his investigation devoted to "Mutations of Space."

of an *aggregate as such*, without the introduction of a new logical fundamental concept (such as that of order).

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Dear Sir:

Through the kindness of Professor Fraenkel, I was enabled to see his note before publication. There are one or two remarks which may clarify some of the issues suggested in my article.

I must confess to a difficulty in understanding the various interpretations that have been given to Cantor's definition of "Menge." Perhaps one accepted English translation (E. W. Hobson, *The Theory of Functions of a Real Variable*, Art. 111) will clear up the matter: "A collection of definite distinct objects which is regarded as a single whole is called an aggregate." It will be noticed that "collection," as an English word, could have two distinct meanings—the uncompleted act of collecting, or the result of a completed act of collection. Likewise the word "regarded" is ambiguous. However, I gave Cantor's exact words, although I have not yet any clear idea of what the German words "Anschauung" and "Denkens" mean; an extreme behaviorist might include both words in the category of meaningless noises. Similarly for "regarded," which is metaphorical. The metaphor is unresolved. The apparent fact that there seems to be still some doubt among experts as to the precise meaning of "class," or of "Menge," indicates that something remains to be done in the way of definition. One possibility is to accept "Menge" intuitively; another is to add further definitions of the terms used in the official definition. I believe the net result will be the same: those to whom the original definition is free of mysticism will be confirmed in their insight, while those who lack insight originally will not be enlightened.

It seems to me that this extremely elementary matter of what a "Menge" is, is the parting of the ways, where those who are capable of belief in what is not humanly constructible go to the right, while others turn to the left. The leftists may agree that the words sound like sense but are reluctant to credit the words with more than sound. Few mathematicians, however, are such extreme leftists as this, and even the most skeptical do not let their lack of beliefs interfere with their technical mathematics—which, according to their philosophical nihilism, may be totally devoid of meaning or consistency.