Letter to Frege

BERTRAND RUSSELL

(*1902*)

Bertrand Russell discovered what became known as the Russell paradox in June 1901 (see *1944*, p. 13). In the letter below, written more than a year later and hitherto unpublished, he communicates the paradox to Frege. The paradox shook the logicians' world, and the rumbles are still felt today.

The Burali-Forti paradox, discovered a few years earlier, involves the notion of ordinal number; it seemed to be intimately connected with Cantor's set theory, hence to be the mathematicians' concern rather than the logicians'. Russell's paradox, which makes use of the bare notions of set and element, falls squarely in the field of logic. The paradox was first published by Russell in *The principles of mathematics* (*1903*) and is discussed there in great detail (see especially pp. 101–107). After various attempts, Russell considered the paradox solved by the theory of types (*1908a*). Zermelo (below, p. 191, footnote 9) states that he had discovered the paradox independently of Russell and communicated it to Hilbert, among others, prior to its publication by Russell.

In addition to the statement of the paradox, the letter offers a vivid picture of Russell's attitude toward Frege and his work at the time.

The formula in Peano's notation at the end of the letter can be read more easily if one compares it with formula 450 in *Peano 1898a*, p. VII (or *1897*, p. 15).

Russell wrote the letter in German, and it was translated by Beverly Woodward. Lord Russell read the translation and gave permission to print it here.

Friday's Hill, Haslemere, 16 June 1902

Dear colleague,

For a year and a half I have been acquainted with your *Grundgesetze der Arithmetik*, but it is only now that I have been able to find the time for the thorough study I intended to make of your work. I find myself in complete agreement with you in all essentials, particularly when you reject any psychological element [[Moment]] in logic and when you place a high value upon an ideography [[Begriffsschrift]] for the foundations of mathematics and of formal logic, which, incidentally, can hardly be distinguished. With regard to many particular questions, I find in your work discussions, distinctions, and definitions that one seeks in vain in the works of other logicians. Especially so far as function is concerned (§ 9 of your *Begriffsschrift*), I have been led on my own to views that are the same even in the details. There is just one point where I have encountered a difficulty. You state (p. 17 [[p. 23 above]]) that a function,

too, can act as the indeterminate element. This I formerly believed, but now this view seems doubtful to me because of the following contradiction. Let w be the predicate: to be a predicate that cannot be predicated of itself. Can w be predicated of itself? From each answer its opposite follows. Therefore we must conclude that w is not a predicate. Likewise there is no class (as a totality) of those classes which, each taken as a totality, do not belong to themselves. From this I conclude that under certain circumstances a definable collection [[Menge]] does not form a totality.

I am on the point of finishing a book on the principles of mathematics and in it I should like to discuss your work very thoroughly.[1] I already have your books or shall buy them soon, but I would be very grateful to you if you could send me reprints of your articles in various periodicals. In case this should be impossible, however, I will obtain them from a library.

The exact treatment of logic in fundamental questions, where symbols fail, has remained very much behind; in your works I find the best I know of our time, and therefore I have permitted myself to express my deep respect to you. It is very regrettable that you have not come to publish the second volume of your *Grundgesetze*; I hope that this will still be done.

Very respectfully yours,

Bertrand Russell

The above contradiction, when expressed in Peano's ideography, reads as follows:

$$w = \mathrm{cls} \cap x \ni (x \sim\varepsilon\, x) .\supset: w \,\varepsilon\, w .=. w \sim\varepsilon\, w.$$

I have written to Peano about this, but he still owes me an answer.

[1] [[This was done in *Russell 1903*, Appendix A, "The logical and arithmetical doctrines of Frege".]]

Letter to Russell

GOTTLOB FREGE

(*1902*)

This is Frege's prompt answer to Russell's letter published above. Frege first calls Russell's attention to an error in *Begriffsschrift*; it is a mere oversight, without any consequence (see above, p. 15, footnote 12). He then describes his reaction to the paradox that Russell has just communicated to him, and he begins to look for the source of the predicament. He incriminates the "transformation of the generalization of an equality into an equality of courses-of-values". For Frege a function is something incomplete, "unsaturated". When it is written $f(x)$, x is something extraneous that merely serves to indicate the kind of supplementation that is needed; we might just as well write $f(\ )$. Consider now two functions that, for the same argument, always have the same value: $(x)(f(x) = g(x))$. (This is not Frege's notation, but its modern equivalent.) Since f and g, or rather $f(\ )$ and $g(\ )$, are something incomplete, we cannot simply write $f = g$. Functions are not objects, and in order to treat them, in some respect, as objects Frege introduces their *Werthverlauf*. The *Werthverlauf* of a function $f(x)$ is denoted by $\grave{\varepsilon}f(\varepsilon)$ (where ε is a dummy; we can also write $\grave{\alpha}f(\alpha), \ldots$). The expression "the function $f(x)$ has the same *Werthverlauf* as the function $g(x)$" is taken to mean "for the same argument the function $f(x)$ always has the same value as the function $g(x)$", and we can write (in modern notation)

$$(*) \quad (x)(f(x) = g(x)) \equiv (\grave{\varepsilon}f(\varepsilon) = \grave{\alpha}g(\alpha)).$$

This is the "transformation of the generalization of an equality into an equality of courses-of-values". Whereas the function is unsaturated and is not an object, its *Werthverlauf* is "something complete in itself", an object, in particular so far as substitution is concerned. There Frege sees the origin of the paradox.

Frege soon made his point more specific. He received Russell's letter while the second volume of his *Grundgesetze der Arithmetik* was at the printshop, and he barely had the time to add an appendix in which he shows how the schema (*) above (or rather half of it, the implication from right to left) allows the derivation of the paradox; he also proposed a restriction in the schema to prevent that. Russell, whose *Principles of mathematics* was at the printshop when he received Frege's volume, added to his book an appendix in which he endorsed Frege's emendation. But soon thereafter he tried out various other solutions (*1905a*); he finally proposed his theory of types (*1908a*).

Russell's paradox has been leaven in modern logic, and countless works have dealt with it. For a late and thorough study of Frege's "way out", see *Quine 1955*.

When Lord Russell was asked whether he would consent to the publication of his letter to Frege (*1902*), he replied with the following letter, in which the reader will find a stirring tribute to Frege.

Penrhyndeudraeth, 23 November 1962

Dear Professor van Heijenoort,

I should be most pleased if you would publish the correspondence between Frege and myself, and I am grateful to you for suggesting this. As I think about acts of integrity and grace, I realise that there is nothing in my knowledge to compare with Frege's dedication to truth. His entire life's work was on the verge of completion, much of his work had been ignored to the benefit of men infinitely less capable, his second volume was about to be published, and upon finding that his fundamental assumption was in error, he responded with intellectual pleasure clearly submerging any feelings of personal disappointment. It was almost superhuman and a telling indication of that of which men are capable if their dedication is to creative work and knowledge instead of cruder efforts to dominate and be known.

Yours sincerely,

Bertrand Russell

The translation of Frege's letter is by Beverly Woodward, and it is printed here with the kind permission of Verlag Felix Meiner and the Institut für mathematische Logik und Grundlagenforschung in Münster, who are preparing an edition of Frege's scientific correspondence and hitherto unpublished writings; this edition will include the German text of the letter.

Jena, 22 June 1902

Dear colleague,

Many thanks for your interesting letter of 16 June. I am pleased that you agree with me on many points and that you intend to discuss my work thoroughly. In response to your request I am sending you the following publications:

1. "Kritische Beleuchtung" [[*1895*]],
2. "Ueber die Begriffsschrift des Herrn Peano" [[*1896*]],
3. "Ueber Begriff und Gegenstand" [[*1892*]],
4. "Über Sinn und Bedeutung" [[*1892a*]],
5. "Ueber formale Theorien der Arithmetik" [[*1885*]].

I received an empty envelope that seems to be addressed by your hand. I surmise that you meant to send me something that has been lost by accident. If this is the case, I thank you for your kind intention. I am enclosing the front of the envelope.

When I now read my *Begriffsschrift* again, I find that I have changed my views on many points, as you will see if you compare it with my *Grundgesetze der Arithmetik*. I ask you to delete the paragraph beginning "Nicht minder erkennt man" on page 7 of my *Begriffsschrift* [["It is no less easy to see", p. 15 above]], since it is incorrect; incidentally, this had no detrimental effects on the rest of the booklet's contents.

Your discovery of the contradiction caused me the greatest surprise and, I would almost say, consternation, since it has shaken the basis on which I intended to build arithmetic. It seems, then, that transforming the generalization of an equality into an equality of courses-of-values [[die Umwandlung der Allgemeinheit einer Gleichheit in eine Werthverlaufsgleichheit]] (§ 9 of my *Grundgesetze*) is not always permitted, that my Rule V (§ 20, p. 36) is false, and that my explanations in § 31 are not sufficient to ensure that my combinations of signs have a meaning in all cases. I must reflect further on the matter. It is all the more serious since, with the loss of my Rule V, not only the foundations of my arithmetic, but also the sole possible foundations of arithmetic, seem to vanish. Yet, I should think, it must be possible to set up conditions for the transformation of the generalization of an equality into an equality of courses-of-values such that the essentials of my proofs remain intact. In any case your discovery is very remarkable and will perhaps result in a great advance in logic, unwelcome as it may seem at first glance.

Incidentally, it seems to me that the expression "a predicate is predicated of itself" is not exact. A predicate is as a rule a first-level function, and this function requires an object as argument and cannot have itself as argument (subject). Therefore I would prefer to say "a notion is predicated of its own extension". If the function $\Phi(\xi)$ is a concept, I denote its extension (or the corresponding class) by "$\grave{\varepsilon}\Phi(\varepsilon)$" (to be sure, the justification for this has now become questionable to me). In "$\Phi(\grave{\varepsilon}\Phi(\varepsilon))$" or "$\grave{\varepsilon}\Phi(\varepsilon) \cap \grave{\varepsilon}\Phi(\varepsilon)$"[1] we then have a case in which the concept $\Phi(\xi)$ is predicated of its own extension.

The second volume of my *Grundgesetze* is to appear shortly. I shall no doubt have to add an appendix in which your discovery is taken into account. If only I already had the right point of view for that!

Very respectfully yours,
G. Frege

[1] [["$\cap$" is a sign used by Frege for reducing second-level functions to first-level functions. See *Frege 1893*, § 34.]]

On the foundations of logic and arithmetic

DAVID HILBERT

(*1904*)

This is the text of an address delivered by Hilbert on 12 August 1904 at the Third International Congress of Mathematicians, held in Heidelberg on 8–13 August 1904.

In the last years of the nineteenth century Hilbert provided a satisfactory axiomatization of geometry (*1899*). He then (*1900*) offered a set of axioms for the real numbers and indicated that the question of the consistency of geometry comes down to that of the real-number system. At the Paris International Congress of Mathematicians in 1900, as a natural continuation of this work, he placed the consistency of the real-number system on a list of problems challenging the mathematical world (*1900a*, pp. 264–266). He did not outline any approach, simply stressing that a relative consistency proof seemed out of the question and that, therefore, the problem presented a fundamental difficulty.

Meanwhile the Russell paradox became known, and the question of consistency became more pressing. In 1904, in the paper below, Hilbert presents a first attempt at proving the consistency of arithmetic. In fact, his plan—to show that all the formulas of a certain class possess a certain property (that of being "homogeneous") by showing that the initial formulas have it and the rules transmit it—is the prototype of a device now current in investigations of that nature. Besides the search for a consistency proof the paper offers a critique of the various points of view held at that time on the foundations of arithmetic and introduces the themes that Hilbert is going to develop, modify, or make more precise in his further work in the foundations of mathematics: the reduction of mathematics to a collection of formulas, the extralogical existence of basic objects, like 1, and their combinations, and the construction of logic in parallel with the study of these combinations.

The presentation remains tentative and sketchy. Only many years later (*1917*) will Hilbert come back to the problems of the foundations of mathematics and then present the mature and enriched papers of the twenties (*1922*, *1922a*, *1925*, *1927*). The 1904 paper provides a helpful landmark in the development of Hilbert's conceptions.

The paper was commented upon by Poincaré (*1905*, pp. 17–27; *1908*, pp. 179–191) and Pieri (*1906*). Later commentaries can be found in Bernays (*1935*, pp. 199–200) and Blumenthal (*1935*, p. 422). The paper greatly influenced Julius Kōnig's book (*1914*), which in turn inspired von Neumann in his search for a consistency proof of arithmetic (*1927*, footnote 8, p. 22).

An English translation of Hilbert's paper was published (*1905*) in *The monist*, but we have not found it possible to use it. The present translation is by Beverly Woodward, and it is printed here with the kind permission of B. G. Teubner Verlagsgesellschaft, Stuttgart.