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LXXI. *On the Theory of Vanishing Fractions, in reply to the Observations of Professor Young.* By W. S. B. WOOLHOUSE.*

IN my short but comprehensive essay on the principles of the differential and integral calculus, printed in the Appendix to the Gentleman's Diary for the years 1835-36, my expressed object was to remove, as far as was practicable, the perplexing difficulties usually experienced by those students who very naturally desire to bring the subject under the guidance and dominion of their reasoning faculties. The theory of vanishing fractions is well known to be the chief source of these difficulties; and it so happens, unfortunately for beginners, that writers have hitherto paid little or no attention to the strict interpretation that ought to be given, in the general sense, to a fraction when the values of its numerator and denominator have both absolutely disappeared or become equal to *zero*. This part of the subject has been diligently examined in the second part of my essay, where it is shown that a fraction in such a state may consistently possess any value whatever, if it be not limited by a special condition, but that one particular value only will fulfill the law of continuity assumed by the successive values immediately before or after the disappearances take place. It appears, however, that my explanations, there given, are insufficient to satisfy the scruples of Prof. Young, who has, in opposition to the principle I have adopted, entered into a very general statement of his own views at page 295 of the last Number of the Philosophical Magazine. The opinions of one so deservedly eminent as my esteemed friend are entitled to high consideration, and I am duly sensible of the respectful and condescending manner in which his observations are expressed. In a mathematical discussion of this kind, however, it would have been more desirable had Prof. Young attached less weight to his supposed evidence of authority, and applied himself more closely to the demonstration of his statements, nearly all of which are at direct variance with my judgement, and therefore, to me, far from being satisfactory. I here propose a brief and explicit examination of the most important points that Professor Young has advanced, and, I hope, with the same earnest anxiety for the spread of scientific truth that he has been pleased to ascribe to me.

To any person unacquainted with the inquiry, Professor Young's assertions would seem to imply that my views were of a strange and revolutionary description; that they were, in re-

* Communicated by the Author.

ality, adverse to the results of our ablest modern analysts, and directly opposed to well-established truths;—indeed it would almost appear that Professor Young himself had contracted that notion. No idea, however, could be imagined more contrary to the fact. The new line of theory, adopted and pursued in my essay, leads to precisely the conclusions subscribed to by all modern analytical writers, and varies only in the substitution of strict reasoning in place of the illogical and mysterious mode of deduction that has all along rendered this most important branch of mathematics a popular paradox. Professor Young has quoted two of my most general principles, which, with one or two more extracts, will convey a pretty correct idea of the particular view I have taken of the subject. As these extracts will very much facilitate the present discussion I shall here annex them.

- I. As a principle, we have no right to reduce a fraction by dividing its numerator and denominator by *absolute nothing*, as the process removes from the fractions the indeterminate character which they previously possessed, and which they ought to retain. (Gentleman's Diary, Appendix, page 26.)
- II. If, in the investigation of a geometrical problem, the unknown quantity is expressed by a fraction which in a particular case becomes a vanishing one, the problem in that case will resolve itself into a porism, and the value of the fraction, or unknown quantity, will then admit of arbitrary assumption; and a similar result will follow in all such cases, whatever be the nature of the investigation. (Page 25.)
- III. Whenever, in an analytical investigation, the resulting expression for a quantity resolves itself into a vanishing fraction, we may observe, as a general rule, that either one of the original conditions of the inquiry becomes destroyed, or that two or more of them become dependent, and, consequently, whichever way it be, that there is at least one condition less to fulfill, and that the vanishing fraction is not restricted to any determinate value. (Pages 26, 27.)
- IV. When a fraction, which in a particular case becomes a vanishing one, expresses the value of a quantity which we previously know, from the nature of the subject, does not become discontinuous in that case, or generally when such a fraction enters in any equation, the other terms of which are not discontinuous, the fraction is, under such circumstances, necessarily limited to continuous values, and consequently, when the terms vanish, it must take the particular value, (described in the essay,) or the ordinary result deduced either by the method of limits or the usual process of differentiation. (Page 29.)

The paragraphs II. and III., which embody the main principle, are those extracted by Professor Young, who labours under a misapprehension if he supposes that I contented myself with testing their accuracy by two particular examples. Has Professor Young read the remark on page 26 that immediately follows my examples? Speaking of the examples, I there add, that “these are not adduced as curious instances, but merely as examples of what *always* takes place in such

cases." It is here evident that I had not contented myself with testing the accuracy of the propositions by the two particular examples. On the contrary, my conviction of their truth was founded on the solid evidence of mental demonstration, and the examples were adduced for the purpose of illustration without any reference to the proof of the principle itself, which, in common with the others, may be established without much difficulty. I shall now proceed at once to the demonstration of these principles.

First, then, it is required to be shown that, logically, we have no right to reduce a fraction by dividing its numerator and denominator by *absolute nothing*. Let θx , ϕx be two functions of a variable x which do not vanish when $x=a$; and suppose another variable y to be so connected with x as to always fulfill the condition

$$(x-a)^{\alpha} \theta x - y (x-a)^{\beta} \phi x = 0 \dots\dots (1)$$

in which α , β , are two positive numerical indices and either whole or fractional. The value of y deduced from this condition is

$$y = \frac{(x-a)^{\alpha} \theta x}{(x-a)^{\beta} \phi x} \dots\dots\dots (2)$$

and takes the most general form of a vanishing fraction. Suppose it to be reduced by dividing the numerator and denominator by $(x-a)^{\beta}$, and it becomes

$$y = \frac{(x-a)^{\alpha-\beta} \theta x}{\phi x} = (x-a)^{\alpha-\beta} \cdot \frac{\theta x}{\phi x} \dots\dots (3)$$

Let x now be taken equal to a and the expression (2) will become $y = \frac{0}{0}$, while (3) will give

$$y = \left\{ \frac{\theta a}{\phi a} \right\} \text{ if } \left\{ \begin{array}{l} \alpha > \beta \\ \alpha = \beta \\ \alpha < \beta \end{array} \right\} \dots\dots (4)$$

But if we refer back to the original condition (1), it is plain that it will be satisfied with $x=a$, independently of the value of y , that in this case it imposes no limit whatever on the value of y which is therefore completely indeterminate. It follows therefore that the result of (2), when $x=a$, viz. $y = \frac{0}{0}$, must have the same indeterminate acceptation; and that the process of dividing the numerator and denominator of (2) by $(x-a)^{\beta}$, ($=$ zero when $x=a$), which produces (3), and so determines a *particular* value for y , is inadmissible when $x=a$, and ought not in that case to be performed. And as the ex-

pression (2) comprehends every possible case of vanishing fractions, the reasoning is general, and fully establishes the position occupied in the first extract.

It is here evident that the same objection will apply to the division by *zero* of an equation involving two variables, or that the equation resulting from a division by a common factor is inadmissible when that factor absolutely vanishes. Thus the equation (1), when divided by $(x - a)^\beta$, gives

$$(x - a)^{\alpha - \beta} \theta x - y \phi x = 0,$$

which would, for $x = a$, give to y the *particular* value in (3), a circumstance quite inconsistent with the nature of the condition involved in the antecedent equation (1), which, in the case $x = a$, places no restriction on the value of y . It is also obvious that the multiplication of an equation, or of the numerator and denominator of a fraction, by *zero*, is equally objectionable, as regards propriety of reasoning, since, by that process, we might pass from conditions that determine particular values to others of a totally indeterminate character. Before quitting this point it will be well to draw a general and necessary inference that may, in conjunction with the fourth extract, contribute in some degree towards the elucidation of the present inquiry. It is this:—That when a quantity, which we know from other considerations ought to have a determinate value, comes out in a vanishing fraction, or, *vice versa*, when a quantity, which we know to be indeterminate, comes out in a determinate form, we may be assured that at least one of the steps, in the process of solution, fails in the manner here explained.

The proof of the principles contained in the other extracts immediately follows from the preceding demonstration. Suppose the equation (2) to express the result of an analytical investigation in which the reasoning throughout is admissible when $x = a$, so that no multiplication or division by a power of $x - a$ occurs in the process. We proceed to show that the resulting vanishing fraction (2), when $x = a$, must be indeterminate in value. The equation (1), which is antecedent to, and corresponds in signification with, the equation (2), is satisfied with $x = a$, without any reference to the value of y , because that equation is divisible by a positive power of $x - a$. Since, therefore, in the investigation, no multiplications or divisions have been made by $x - a$ or any power of it, it is conclusive that the series of equations, which precede the equation (1) in the course of reduction, must likewise be divisible by the same power of $x - a$, and therefore be satisfied with $x = a$, independently of the value of y . The primitive equation from

which the expression (2) is deduced will consequently, when $x = a$, be also satisfied by any value of y . If this primitive equation expresses an original condition of the problem, that condition, therefore, when $x = a$, cannot limit the variable y , or the expression (2), to any particular value. If, however, this equation is produced by a combination of two or more leading equations of the problem, the circumstance of its wholly disappearing when $x = a$, will necessarily lead us to the conclusion that for this particular value of x some dependency exists among those leading equations, and therefore that one of the original conditions of the problem becomes, in that case, virtually destroyed. In addition to this proof we may remark that the expression (3) is legitimately deduced from the equation (1) in every case in which $x - a$ does not absolutely vanish, or in which the value of x differs from the quantity a , however small that difference may be; that since it holds good when x is taken as nearly equal to a as we please, and is in itself continuous as x approaches and arrives at that value, it is evident that, when x becomes exactly equal to a , it will express, as in (4), that particular value of y , or of the vanishing fraction (2), which unites in the law of continuity observed by all its other successive values.

Having attempted, and I expect successfully, the demonstration of the principles laid down in the extracts from my essay, without discovering them to be "fallacious," it now remains for Professor Young, since the truth is our common object, either to subscribe to my views or to point out wherein consists the inaccuracy of the reasoning here employed; and, without any wish to prolong our discussion, I unhesitatingly pledge myself to devote my most respectful and candid consideration to whatever arguments or explanations he may be pleased to offer. But it will be useless to pursue the subject any further unless Professor Young will enter more into the theoretical merits of the question and make up his mind to support every general statement with some kind of evidence.

In Professor Young's present letter he thinks it remarkable that I did not reflect that $\frac{0}{0}$ was as likely to be "the symbol of absurdity" as the symbol of multiple values, and he follows up the same idea by observing that "when we are operating with equations of the first degree, containing several unknown quantities, the symbol $\frac{0}{0}$ is, in fact, the very form which the result usually takes when the proposed equations involve incompatible conditions." If, however, subjects of absurdity are not to be absurdly treated, I apprehend it will not require any extraordinary degree of reflection to be convinced of the incorrectness of such

a notion. On the other hand, it is rather remarkable that Professor Young did not consider that $\frac{0}{0}$ was the usual symbol of absurdity or of incompatible conditions, and that $\frac{0}{0}$ could never be so, in the result of an investigation logically conducted. Thus, the corresponding antecedent equation to the result $x = \frac{0}{0}$, when cleared of fractions, is $0x = 0$ or $0 = 0$, an equation that is very obviously satisfied without any limitation to the value of x , and that cannot fail therefore to be compatible with other equations or conditions; but the corresponding antecedent equation to the result $x = \frac{0}{0}$ is $0 = 0$, an equation evidently indicating the presence of absurdity or of incompatible conditions, unless the nature of the investigation will admit of infinite results.

The query respecting the geometrical series is dismissed at once by a reference to the fourth extract from my essay. By putting for S the series it represents, the equation is

$$a + ar + ar^2 + \dots + ar^{n-1} = \frac{a(r^n - 1)}{r - 1}$$

and as the left-hand member is not discontinuous when $r = 1$, the vanishing fraction, which forms the right-hand member, must be limited to its continuous value, viz. na . The very circumstance of the equation involving both a determinate and an indeterminate quantity, when $r = 1$, indicates the existence of a fallacy in the process by which it has been deduced. We first have

$$S = a + ar + ar^2 + \dots + ar^{n-1} \dots (a)$$

and multiplying by $r - 1$, we get

$$(r - 1) S = ar^n - a = a(r^n - 1) \dots (b)$$

which divided by $r - 1$, gives

$$S = a \frac{r^n - 1}{r - 1} \dots (c)$$

In the case $r = 1$, and $r - 1 = 0$, we have therefore committed the fault of multiplying by *absolute nought* in passing from (a) to (b); but the equation (c) is a true deduction from (b), for the mere placing of $r - 1$ in the denominator of a fraction is not an actual performance of division. The equation (a) becomes $S = a + a + a + \dots$; the equation (b) entirely vanishes, and (c) becomes $S = \frac{0}{0}$.

After the foregoing discussion it will be needless to offer any special observations on the obvious inaccuracy of Professor

Young's views of the ellipse question. It may, however, be worth while to take the opportunity of adding a single remark on an erroneous principle which he appears to entertain regarding the general theory of analytical results. I never before heard of the incompetency of an analytical result to afford any positive information that an investigation could admit of. It is plain that the original equations, which express the analytical conditions of a problem, cannot include any extraneous conditions with those expressed in the enunciation, and that they must therefore comprehend, in their analytical results, every solution that the problem is capable of receiving. The equations, however, may not include certain other implied conditions, dependent on the peculiar nature of the inquiry, and therefore may yield some additional solutions incompatible with the conditions so implied. For instance the nature of a problem may be such as to exclude from the results not only imaginary values but negative values and values which fall beyond certain limits, though they will be unavoidably comprehended in the analytical solution. The exclusion of inadmissible solutions, therefore, rests with the nature of the problem and not with the forms of its analytical conditions. It is hence evident that Professor Young involves himself in a palpable inconsistency, when he arrives at the fact of the ellipse question admitting multiple solutions, by an examination of the original analytical conditions, and at the same time alleges that the analytical result is quite incompetent to supply that information; for the true analytical result must necessarily present every solution capable of satisfying the analytical conditions from which it has been deduced. If we refer back to the nature of the problem, as originally presented, which is the proper source of rejective information, we perceive that the only condition it imposes on the results is the limitation which requires the coordinates xy to fall within the bounds of the ellipse, or of the circle that represents it in the indeterminate case.

I have thus unreservedly enumerated the principal reasons on which I found my sincere and firm conviction of the incorrectness of the various statements contained in Professor Young's letter. To avoid the possibility of being misunderstood, I have also given a concise analysis of the most important of the principles maintained in my essay; and, in conclusion, I may be permitted to add, that instead of their being "condemnatory of conclusions which, in the works of our ablest modern analysts, wear all the aspect of mathematical certainty," they establish the truth of those very conclusions on a firmer and more intelligible basis,—that instead of the

mere aspect of certainty, in favour of those conclusions, they substitute certainty itself.

London, April 9, 1836.

LXXII. *Further Experiments on Conducting Power for Electricity.** By EDWARD SOLLY, Jun., Esq.

15. **I**N my former communication I said that I had found iodine when solid to be a nonconductor, but I did not describe any experiments made with it in the melted state. This perhaps may have appeared an omission, the more so after Dr. Inglis's note (the contents of which had not, however, been communicated to me,) had been appended to my paper; but I had been advised to lose no time in describing such of my experiments as were in opposition to Dr. Inglis's statement that "iodine is a conductor".† What follows now will explain that apparent omission.

16. In all my original experiments I had found iodine to be a nonconductor in the fluid as well as the solid state; but on the present occasion, when I was led to repeat them by the above-mentioned statement, I was not a little surprised to find the iodine, when rendered fluid by heat, become a conductor. That a substance acting as iodine does should not be similar as to conducting power when fluid to what it is when solid, (as all known substances that have been as yet examined are, excepting only such as are electrolytes, and also perhaps the periodide of mercury,) but should appear a conductor upon assuming the liquid state, was so singular, and so contrary to my previous results and preconceived views, that I was induced to multiply my experiments; they continued unsatisfactory, and they were the more so as the iodine did not always appear a conductor but sometimes a nonconductor, and then, when it did appear a conductor it did in a very feeble manner, and with great uncertainty.

17. I was therefore led to doubt the purity of the iodine which I was using, and this seemed the more probable as it was from a different source from that which I had employed in the original experiments; and in the means which I had before described for examining conducting power it was impossible the wires could touch, and therefore the objection which the use of loose wires would have introduced was avoided. In consequence I procured some perfectly pure iodine sublimed at a very low temperature, and ascertained that that which I had

* Communicated by the Author: see our Number for February, p. 130.

† Lond. and Edin. Phil. Mag., No. 43. p. 129.