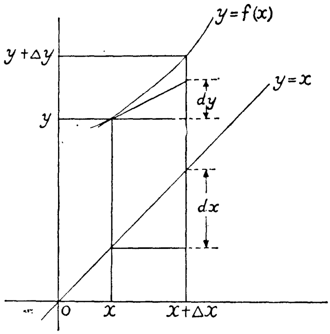
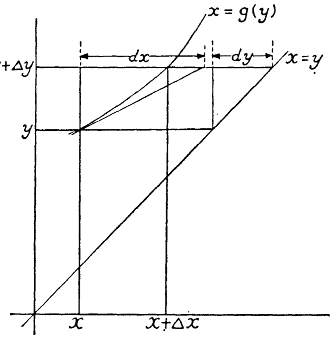
To the Editor, *The Mathematical Gazette.*

DEAR SIR,—In making the assertion that the relations $dx = \Delta x$ and $dy = \Delta y$ are inconsistent, Hugh Thurston (*The Mathematical Gazette*, XLVIII, p. 27) seems to have overlooked that they refer to different situations, and so cannot be compared. The diagrams illustrate this.




$x$ independent




$y$ independent

It is surprising how much argument differentials seem to engender, for Hardy's definition that, if $z$ is a function of the two independent variables $x$ and $y$ then

$$dz = f'_x \, . \, \Delta x + f'_y \, . \, \Delta y, \tag{1}$$

is perfectly straightforward. It makes $dz$ a function of four independent variables, $x$, $y$, $\Delta x$, and $\Delta y$. The second differential of $z$ is defined in exactly the same way as the first differential, i.e.,

$$d(dz) = \frac{\partial}{\partial x}(dz) \, . \, \Delta x + \frac{\partial}{\partial y}(dz) \, . \, \Delta y$$

and so on for third and higher order differentials.

Applying these definitions to the function $x$ gives

$$dx = 1 \, . \, \Delta x + 0 \, . \, \Delta y = \Delta x$$

$$d^2x = \frac{\partial}{\partial x}(dx) \, . \, \Delta x + \frac{\partial}{\partial y}(dx) \, . \, \Delta y = 0 \, . \, \Delta x + 0 \, . \, \Delta y = 0$$

and so on. One wonders why Goursat, de la Vallée Poussin, E. G. Phillips, etc. want to treat $dx$ as constant. $\partial(\Delta x)/\partial x = 0$ because $\Delta x$ is independent of $x$, not because $\Delta x$ is constant. Hadamard's description of the concept of a constant $dx$ as " fully unintelligible " (*The Mathematical Gazette*, XXXIV, p. 210) is not fair as a criticism of Phillips (XXXIII, p. 202), since Phillips explicitly points out that the definition of a differential does not require the idea of an infinitesimal, but the concept is none the less unhelpful, at the least.

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Since, in the definition (1), $x$ and $y$ are independent variables, $dx = \Delta x$ and $dy = \Delta y$; so that the relation

$$dz = f'_x . dx + f'_y . dy \tag{2}$$

holds between the differentials of the three functions $x$, $y$, $z$ (provided the same values of $\Delta x$ and $\Delta y$ are used for calculating all the differentials: this must always be understood); and it is well known that this relation continues to hold if only one, or neither, of the variables $x$ and $y$ is independent. (2) is, as stated, a relation between the differentials of *functions*: the idea of the differential of an independent *variable* is an unnecessary one and causes nothing but confusion. $dx$ is quite different in conception from $\Delta x$, and Hardy's definition does *not* imply (as Thurston seems to think) that " $dx$ is necessarily the same as $\Delta x$ ", nor that $dx$ and $\Delta x$ are necessarily equal, as the second diagram above shows.

P. Gant (XXXV, p. 111) put forward the view that differentials should be undefined entities obeying formal rules of manipulation. This seems to be a counsel of despair, as also does Thurston's idea of defining only the ratio $dx : dy$. Ideas concerning undefined quantities are difficult to get across to students, and in any case one of the most important practical uses of differentials is that, if they are small, they provide useful approximations to actual changes in quantities (strictly, the changes should be small as well as the differentials). It is *not* only " the ratio of $dx$ to $dy$ that matters " (Thurston). Gant rejected Hardy's definition because of " difficulties to which I see no satisfactory answer ". One of these was dissatisfaction with the constant $dx$ idea. Another was the " difference in status between the differentials of dependent and independent variables, which is aesthetically most unsatisfying ". This difference is inescapable, however, and Gant's treatment does nothing to remove it. His final point, that if differentials *were* finite quantities then they must obey the laws of arithmetic and so the relation

$$\frac{d^2z}{dt^2} = \frac{d^2z}{dx^2} \left(\frac{dx}{dt}\right)^2 \tag{3}$$

must be true (here $z = f(x)$ and $x = x(t)$, $t$ being the independent variable) is a more interesting one. Actually, *as a relation between differentials* (3) is true, but as a relation between derivatives it is untrue except in the case when $x$ is a linear function of $t$. This is because

$$d^2z \div dx^2 = f''(x) + f'(x) . d^2x \div dx^2$$

so that if $d^2x \neq 0$, $d^2z \div dx^2 \neq f''(z)$.

I disagree with two further points in Thurston's article. Firstly, $dx$ is *never* arbitrary, even if $x$ is an independent variable. Secondly, it is not true that if $dx = 0$ in $dy = f'(x) . dx$ then $dy = 0$. If $dx = 0$, then $f'(x)$ is infinite and $dy$ is indeterminate. E.g., if $y = \arcsin x$ then $dy = dx / \surd(1 - x^2)$, and putting $dx = 0$ gives $x = \pm 1$.

One final remark regarding Hardy's definition: this defines $dz$ at points where the partial derivatives merely exist, and it is a matter of opinion whether to add the further condition that $dz$ is to be regarded

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as defined only at points where $z$ is differentiable. I should prefer not to add this condition but to *define* differentiable by the property that $z$ is differentiable at any point at which $dz/\Delta z \to 1$ as the arbitrary increments of all the independent variables tend to zero: this definition, which seems more natural than any other, was suggested to me many years ago by Prof. D. Rees.

Yours faithfully,
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[I promised to provide a little space for this subject, but it cannot go on much longer. E.A.M.]

To the Editor, *The Mathematical Gazette.*

DEAR SIR,—As to the Note 3084 in the *Gazette*, I *said* in my article on Linear Algebraic Equations that the theorem in question was *not new*; and in a footnote I cited the paper on the Pi Theorem where I first proved it in 1957. This antedates the book of Richards (1959); and in 1956, when the article was written, I was totally unaware of the Russian treatise of Gantmacher.

Yours sincerely,
LOUIS BRAND

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To the Editor of *The Mathematical Gazette*

Dear Sir, —

*Alfred North Whitehead*

I should appreciate information concerning letters by and about Whitehead, other documents, and recollections of him, for a biography which I am undertaking with the approval and encouragement of his son T. N. Whitehead. I shall be in the United Kingdom this summer to pursue leads. Please reply to Passenger Mail, Thomas Cook & Sons, 45 Berkeley Street, London W. 1.

VICTOR LOWE

*Professor of Philosophy*
*The Johns Hopkins University*
*Baltimore, Maryland, U.S.A.*

## PROFESSOR WATSON AND MR. HOPE-JONES

While *The Mathematical Gazette* for May was being printed, we heard with deep regret of the deaths of two of our most senior members, both Vice-Presidents of the Association, Dr. G. N. Watson, F.R.S., and Mr. W. Hope-Jones. We hope to pay tribute later to all that they did for us over many years.