Angular momentum about $N = mv_0 p = -mr^2\, d\theta/dt = \text{constant}$.

$$\therefore\ 2mv_0 \sin\frac{\phi}{2} = -\frac{2Ze^2}{v_0 p}\int_{(\pi-\phi)/2}^{-(\pi-\phi)/2} \cos\theta\, d\theta = \frac{4Ze^2}{v_0 p}\cos\frac{\phi}{2}$$

$$\therefore\ \cot\frac{\phi}{2} = \left(\frac{mv_0^2}{2Ze^2}\right)p$$

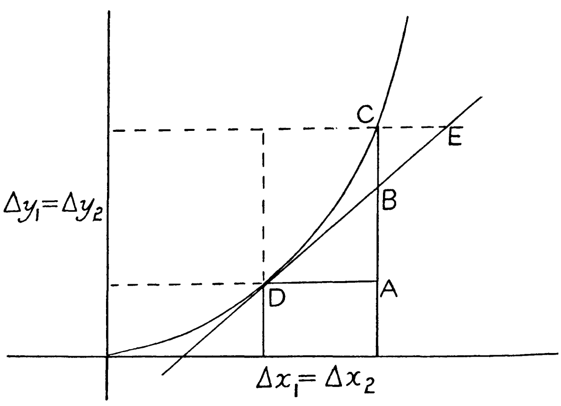
Peterhouse, Cambridge MORETON MOORE

CORRESPONDENCE

To the Editor of *The Mathematical Gazette*

DEAR SIR,—In an article (*The Mathematical Gazette*, Vol. XLVIII (1964) 27), H. Thurston claims as an excuse for revising the definition of a differential the existence of a paradox in the classical definition. One sympathises with the desire for change: nevertheless, to give as a reason something which is little more than an error (and so hardly warrants the title of a paradox), is not very convincing.

Clearly not all the quantities Δx, Δy, dx and dy can take the same values in the two distinct cases mentioned in the article (unless the function is the identity function), and therefore no inconsistency is apparent. For instance, in the example given $y = x^2$, the inverse function is $x = y^{1/2}$ and we have the following diagram in which the Δx and Δy have been chosen the same in the two cases.



Then using suffixes to distinguish between the two functions, we have, in consequence of the classical definition of differentials,

(i) $y = x^2$, $dy_1 = AB$, $dx_1 = DA$;
(ii) $x = y^{1/2}$, $dy_2 = AB + BC$, $dx_2 = DA + CE$.

There is no inconsistency in the equations

$$dx_1 = \Delta x_1, \qquad dy_2 = \Delta y_2.$$

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I assume that in fact the " paradox " was not meant to be taken seriously, but, nevertheless, because it is followed by a serious article, there seems a danger that it might be. In any case the classical treatment has strong teaching advantages; if the sophisticated analyst prefers the definition given by Dr. Thurston, the two are not irreconcilable.

Yours faithfully,

Northampton College of Advanced Technology, M. BRUCKHEIMER
St John Street, London, E.C. 1.

DEAR SIR,—The answer to Mr. Sutton's question—how seriously do I intend my paradox about differentials to be taken—is that I intend it to be taken quite seriously.

The answer to his other question—do I find the same difficulties with homogeneous coordinates—is " no ". The paradox is definitely *not* due to the homogeneity of the relation between differentials. And this should be quite easy to see, because (as I pointed out) if we short-circuit increments and define $dy : dx$ directly by the formula

$$dy = f'(x)\, dx$$

the paradox is avoided, though the relation is still homogeneous.

In his other remarks, Mr. Sutton has used a different notation from Goursat and Hardy. In the context $y = f(x)$, both Goursat and Hardy use dy, not df to denote

$$f'(x) \,.\, \Delta x$$

(Hardy uses δ for Δ throughout but this is a trivial difference.)

Since my concern was to deduce a contradiction from *their definition*, I naturally used *their notation*. Because Mr. Sutton has not done so, he has not rehabilitated the definition which I attacked, but a different one. Perhaps his change of notation is evidence that he realizes unconsciously that there is something wrong with the Hardy/Goursat definition. In fact, as a symbol to denote

$$f'(x) \,.\, \Delta x$$

df is a good deal better than dy. But it is not good enough; $f'(x)\ \ \Delta x$ depends not only on f but also on x and Δx.

An adequate notation would be

$$df(x, \Delta x).$$

This is precisely the notation which I cited as being a sound one. Therefore, if Mr. Sutton will only go one step further in the direction in which he is already going, he and I will agree.

If Mr. Sutton's argument, then, is to be made relevant to the definition under discussion, the inequalities at the top of page 443 should read

$$dy \neq \Delta y, \quad dx \neq \Delta x.$$

And if $\neq$ is interpreted as " is not always equal to " or " is not equal to, in the particular case shown on p. 442 ", then these inequalities are correct, and I willingly acknowledge this.

Mr. Sutton remarks that my paradox cannot be derived from the texts I cite. However, he says nothing to contradict my arguments. I have Goursat open before me at the moment and I see, (deduced from

$$y = f(x)$$

and his definition), the formulae

$$dy = f'(x)\, \Delta x \text{ and } dx = \Delta x.$$

By the logical "rule of substitution", which is valid throughout mathematics and surely cannot be abrogated in this one particular context, the same definition applied to

$$x = g(y)$$

yields $$dx = g'(y)\, \Delta y \text{ and } dy = \Delta y.$$

And these are all I needed.

To sum up, I said that the definition yields a paradox; and essentially this is because it implies both

(i) $$dy \neq \Delta y, \quad dx \neq \Delta x$$

and (ii) $$dy = \Delta y, \quad dx = \Delta x.$$

Mr. Sutton repeats (i) (using a geometrical clarification) but does nothing about (ii). The paradox therefore remains.

Yours faithfully,

H. A. THURSTON

Dept. of Mathematics,
The University of British Columbia,
Vancouver 8, *Canada*

[Δ (this correspondence) $= 0$. E.A.M.]

DEAR SIR,—A. W. Fuller's recent letter (October 1964, p. 324) gives a possible "slow" approach to the integral of $1/x$. Here is another slow approach for those who, like myself, feel that students prefer to meet e through series rather than through a limit of $(x^\delta - 1)/\delta$, mainly because they can then work out e to as many decimal places as they wish and so feel that they have a greater hold over the number. The main assumptions, concerning term-by-term differentiation and multiplication of infinite series, students do not find unacceptable.

1. We should like to solve $dy/dx = 1/x$. Integrals as areas and the graph of $y = 1/x$ leads us to expect that there is a solution except perhaps near $x = 0$. By taking reciprocals and interchanging variables we find that the solution of $dy/dx = y$ is a related problem and we examine this first.

2. If we *had* a solution, $y = f(x)$ say, of this last equation, then $y = Af(x)$ and $y = f(x + a)$ would be other solutions. We compare $y = \sin x$, $A \sin x$ and $\sin (x + a)$ as solutions of $d^4y/dx^4 = y$, but wonder why $y = Af(x + a)$ with the two arbitrary constants should be a solution of a first-order equation.

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3. The solution through the origin is the trivial one $y=0$, so we look for the solution through (0, 1). We then develop the approximations $y=1+x$, $y=1+x+\frac{1}{2}x^2$, etc. until we get

$$y=1+x+\frac{1}{2!}x^2+\frac{1}{3!}x^3+\ldots+\frac{1}{n!}x^n+\ldots$$

which we abbreviate to $y=\exp x$.
4. The convergence of $\exp x$ is considered in a fairly elementary way.
5. §2 suggests that $\exp(x+a)$ is also a solution. We expand this, finding that $\exp(x+a)=\exp a \exp x$, and so the paradox of §2 is resolved.
6. We find $\exp 0=1$, and evaluate $\exp 1$ to a few places of decimals and call it e. Then we use the $\exp(x+a)=\exp a \exp x$ result to obtain (by induction) $\exp n=e^n$ and proceed to $\exp x=e^x$ for positive rational x (the correct qth root has to be thought of) and for negative rational x. A useful discussion on the meaning of $10^{\sqrt{2}}$ follows and we agree to define e^x by $\exp x$ for irrational and transcendental x.
7. At this stage we may try to differentiate e^x from first principles, to confirm that we are on the right track.
8. Finally, $dx/dy=x$ has a solution $x=e^y$, in other words $dy/dx=1/x$ has a solution $y=\log_e x$.

The above approach includes thorough teaching of the exponential series, enables one to comment (in §6) on the build-up from natural numbers to real numbers, shows (albeit artificially) how investigation along one line in mathematics can lead to discoveries in other directions, and has some degree of motivation running all the way through.

Yours faithfully,

C. M. Davis

King's College, Budo,
P.O. Box 121, *Kampala, Uganda*

Dear Sir,—I would like to comment on the conclusions which Mr. Wilson draws from his teaching experiment ("Modern Mathematics" in Schools; Feb. 1965) and in particular the questionnaire (p. 29–33). He classifies the boys in the senior form by their answers to the questionnaire (i.e. whether they found the topics easy, difficult, interesting, dull, etc.) and compares this, for the purposes of his conclusion, with their form order based on an end-of-year examination. There does not seem to me to be any basis of comparison between a boy's subjective assessment of his abilities and interest, and the objective assessment of a written test. Indeed, I am very doubtful about the value of the tables reproduced on p. 30. Given the choice: "do you find the following topics (*a*) very easy, (*b*) fairly easy, (*c*) a bit difficult or (*d*) very difficult". I would expect about half the replies to be (*b*)—a reasonable reply for the average boy—and the other half split between (*a*) and (*c*): those who think they are good at the work and those who think that they are not. A few will be courageous and choose (*d*). The second table shows similar results.

Not only is his evidence rather shaky, but I would question the implication (p. 31 §1) that those boys who found groups and sets easy are

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the real mathematicians and those who do well at " O " level are not. It all depends on your definition of " mathematician ". In practice, though, I would expect that teaching methods have more to do with a pupil's success than the topics taught.

Has Mr. Wilson no sense of humour? What intelligent 14 yr. old would say seriously, " There was generally a refinement in the precision employed, and a grace in the simplicity of the proofs " or " Sets are beautiful but matrices ugly "? I think this is a little light relief from the task of form-filling.

Mr. Wilson asks " ' Would you like to see some elementary set theory or group theory in the " O " level syllabus or would you rather keep all the long, heavy calculations which are in the present syllabus? ' Some people might think this question was loaded. . . . ! " I am sure that if he looked at the present syllabus (Alt. B) he would notice the almost complete absence of long, heavy calculations, which we all agree are unnecessary.

I am pleased to see (p. 33 §1) a valid defence of Euclid, as being a game played with pencil and paper based on axioms and definitions, and showing " abstraction, rigour, and beauty and enjoyment ". It all depends on what the individual finds interesting.

So it would seem that Mr. Wilson started with his conclusions and then produced the evidence to support them, writing it up in a rather emotional way. I hope that we neither throw out present topics without much careful thought, nor, by reaction, be inhibited from experimenting with topics such as sets and matrices in the context of a general mathematical education.

Yours faithfully,
PAUL FISHER

University College School,
Hampstead, N.W. 3.

DEAR SIR,—Surely the mathematical English of the British classroom must be the most slipshod and confusing of all the colloquial mathematical languages! We use " circle " indifferently to refer to a one-dimensional circle (as when two circles meet in two common points) or to a two-dimensional circle (when we talk about its area being πr^2). " Triangle ", too, may be one-, two- or even sometimes zero-dimensional. " Line " is used indifferently to mean line, ray, or line segment. The length of a line segment, (represented for example by AB) is cheerfully confused in most of the textbooks with the *measure* of its length in inches (e.g. 5, a number). Angles and measures of angles are equally well muddled together. For children, whose power of inductive inference is the source of their marvellous learning ability, all this and much more beside is very unhelpful.

Now, with the new syllabuses, they are to be further confused by instruction in " directed " number. *Is* it a number? If so, how does it have direction? Is it a *length*? Is it perhaps a *vector*? How did it get into the approved vocabulary of the Mathematical Association?

More important, how can it be expelled again? It would be a great pity if it were to survive and spread in the present spate of text-book

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writing. One way out is to talk about " directed lengths " on the number line, associated with " signed numbers " in the same way that an ordinary length is associated with its measure in a chosen unit of length.

Incidentally, among the various ways of showing the " signed "ness of the integers, e.g. (-5), or $^{-}5$ for the *additive inverse* of $(+5)$, the simplest and most effective one is verbal. When the minus sign denotes subtraction it is spoken " minus ": when it denotes negative it is spoken " negative ". In the same way + is " plus " or " positive ", though " positive " is usually omitted both in speaking and writing. This habit is surprisingly easy to acquire, and the message gets across by inductive inference. Sixth form boys are no longer surprised to discover that " negative of x " may well be positive.

Glasgow Academy,
Glasgow, W.2.
E. C. THORNTON

DEAR SIR,—I wonder if a non-member of the Association may be permitted to contribute some thoughts arising from the reading of the article by W. G. Bickley (" Mathematics for Engineering Students ") in the December 1964 issue of *The Gazette*.

Assuming that I may do so, being an engineer, it is most refreshing to know that some mathematicians think along these lines. One might be tempted to imagine that the Author is, in fact, an engineer; but this prompts me to ask whether some engineers might not teach mathematics to other engineers (and even to mathematicians) with mutual benefits to be gained. I do know of isolated cases where this does happen and I am sure that it works very well.

It can probably be accepted that engineers need mathematics as tools and nothing else (as far as their profession is concerned—they may like to *be* mathematicians in their spare time) but there is no reason why they should not have a detailed or even profound knowledge of the working of these tools. It is frequently very clear that the mathematician cannot see his mathematics as " tools " at all and may even resent the idea of engineers using them and leaving their " greasy fingermarks " all over them.

Many professional engineers bewail their own lack of knowledge of mathematics (I include myself here) and it is reasonable to suppose that they are not wholly to blame in this regard.

The author's reference to casting " false pearls before real swine " may be true; and if this is true of mathematics teachers generally, how sad is the lot of most schoolteachers who have to cast pearls of all shapes, sizes and values before school-children who, in most cases, are never shown the value of these matters (as tools or for their own intrinsic worth) until they reach tertiary level. I well remember, as a matriculation level school-boy, being told that the forces in the members of a particular triangulated bridge truss were all determined by the simple use of the triangle of forces. This struck me as impossible or, at least, as unlikely.

Clearly my physics and mathematics teachers at that time were unaware of this situation. There is a strong case to be made out for using engineers as teachers at secondary level but no doubt, as is true in this country, it would hardly be fair to single out the engineer to make this financial sacrifice.

Even the rather slow-thinking engineer can enjoy mathematics presented by those who are sympathetic to engineering problems and needs; led gently by the hand, engineers can be presented with the full " works " i.e. with all the rigour and robustness desired, and thoroughly enjoy the experience. I have been subjected to such treatment only twice in my life, once in some lectures on hydrodynamics given by Sir Thomas Havelock and once in a course of Mathematics for engineers given at Honours B.Sc. level by an electrical engineer—turned Ph.D. Mathematician. These experiences will not be forgotten.

Yours faithfully,

B. P. Opie

Dept. of Civil Engineering,
South Australian Institute of Technology,
North Terrace, Adelaide

Dear Sir,—A question appearing in a recent School Certificate Additional Mathematics paper brought the following " solution ", which at any rate had the merit of giving the correct answer, from more than one candidate:

Solve the equation $3^{2x+1} + 3^2 = 3^{x+3} + 3^x$.

" Since we are adding numbers of the same base, we multiply the indices.

$$\therefore\ (2x+1)2 = (x+3)x$$
$$\text{i.e. } x^2 - x - 2 = 0$$
$$\text{or } (x-2)(x+1) = 0.$$
$$\therefore\ x = +2 \text{ or } -1. \text{"}$$

I thought it possible that this might be of interest to readers of the *Gazette*.

Yours faithfully,

D. Perry

College of Technology,
Letchworth

Editor's Note [*H.M.C.*]

$3^a + 3^b = 3^c + 3^d$ is *unlikely* to have rational solutions (can one say *impossible*?) unless

Either $a = c$ and $b = d$
Or $a = d$ and $b = c$

and in each case $ab = cd$!

So the " method " always (?) works.

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