CORRESPONDENCE

The Editor, *The Mathematical Gazette*

DEAR SIR,

One of the causes of my dissatisfaction with "modern" mathematical syllabuses has come to crystallize in my mind recently, largely through the difficulty I find in setting questions on "new" topics in my annual entrance scholarship examination. It is quite simply this: that the current emphasis on structure is reducing elementary mathematics to a descriptive science, resembling natural history (as opposed to biology). For example, "How many sides has a pentagon?" is natural history; "What is the interior angle of a regular pentagon?" is mathematics (if the answer is not quoted from memory!). Elementary questions on such topics as groups, topology, transformations, etc., tend to be of a natural history type, and it is very difficult to set problems in them: and I want to find out what my candidates can *do*, rather than what they *know*. I was thus particularly interested to read in the obituary of C. G. Nobbs (*Gazette* 384, p. 180) that "he wanted his pupils to *do*, rather than *learn*, mathematics, and perhaps for that reason was hesitant to embrace some parts of "modern" mathematics which he felt presented ideas but little opportunity to use them". Precisely—an idea, a concept, a structure, must be capable of being *used*, and to be *seen* to be useful (either within mathematics, as leading to further developments, or in applications) to be worthy of study. The mere contemplation of structure, however elaborate or beautiful in itself, cannot satisfy. I am one with Polya in holding that the heart of mathematics is the solving of problems; mathematical structure is only the framework within which the problems arise. Of course the structure is important: the better it is understood, the more clearly the problem will be perceived, and the more effective will be our attempts at its solution. But the mere contemplation and elaboration of structure is a sophisticated pursuit, and ultimately an arid one, and I deplore the over-emphasis upon it at elementary levels which is now so fashionable. It is like teaching children grammar, and offering them little to read; or explaining the theory of musical scales and sonata-form, and not letting them hear—or play—much music; or worse, letting them think that the theory is more important than the music.

Yours sincerely,

A. R. PARGETER

Blundell's School,
Tiverton, Devon.

https://doi.org/10.2307/3612115 Published online by Cambridge University Press

The Editor, *The Mathematical Gazette*

DEAR SIR,

Confusion in mathematical calculations is often the result of the use of the same symbol with more than one meaning. This is the sole trouble with Messrs. Bruckheimer and Scratons' example at the top of p. 248 (issue 385): (as they admit later) V is not the same function of x and y as it is of r and θ. Recast it: "If upon substituting $x = r\cos\theta$, $y = r\sin\theta$, $V(x, y)$ becomes $U(r, \theta)$, prove that

$$\frac{\partial^2 V}{\partial x^2} + \ldots = \frac{\partial^2 U}{\partial r^2} + \ldots"$$

and there is not the slightest need for any confusion to arise.

Moreover anyone with any sense will, when he gets down to the calculations, replace the cumbersome $\partial V/\partial x$, $\partial^2 U/\partial r^2, \ldots$ by V_x, $U_{rr}, \ldots$ etc.; by which time I don't see that the last line on p. 252 is really any improvement on the traditional formulation—if anything, it is less explicit; and what is gained by writing $V \circ f$ rather than U? (This destroys the symmetry of the relationship, in that we could equally well transform $U(r, \theta)$ into $V(x, y)$ by means of $r = \surd(x^2 + y^2)$, $\theta = \tan^{-1} y/x$.)

On the other hand the expression (p. 250)

$$h' : x \to [(x \to \cos x) \circ (x \to x^2)] \times (x \to 2x)$$

contains 7 x's whose precise shades of meaning require very careful interpretation, and could be thoroughly confusing.

I have seen it said that mathematics is not about symbols, it is about concepts, and that there is no need to teach symbols because if you teach the concepts the symbols will look after themselves. One can assent to the first half of this assertion, whilst recognizing that the second half is complete nonsense. The very publication of the article to which I am referring is proof of this. But the authors are trying to make the point that the newly fashionable notation for functions and their derivatives is superior to the traditional one, in that it is more illuminating and so less liable to lead to errors in calculation. This may be so, up to a point; but the errors they attack arise from the traditional notation being used in a slovenly way, which is perfectly avoidable, and which can just as easily bedevil the use of the modern notation, especially as it is so much more cumbersome. The conventions governing the use of *any* symbolism have to be learned; and an undergraduate who misunderstands $f'(0)$ simply hasn't bothered to master his mathematical ABC. (And the fact that dy/dx and $f'(x)$ are not interchangeable symbols is evident when you consider that one does not write $dy/d3$ for $f'(3)$!)

I am all in favour of clear thinking about what we are doing in differentiation, and in paying more attention to the explicit statement of domains and ranges (which is by no means a prerogative of the "new" approaches); but is it really necessary to overthrow the estab-

https://doi.org/10.2307/3612115 Published online by Cambridge University Press

lished notation, which has undoubted merits in practical use—or it would not have become established and survived for so long—rather than tighten up the loose bolts in the existing structure?

Yours sincerely,
A. R. Pargeter

P.S. If the excerpt quoted in the review on p. 326 is a fair sample of what to expect when modern notation invades integration, heaven help us all! A.R.P.

The Editor, *The Mathematical Gazette*

Dear Sir,

I wonder if you, or your readers, can tell me anything about the life and works of Lill, who is mentioned on page 27 of "Theory of Equations" (Turnbull) Oliver and Boyd.

The topic is a graphical method for evaluating $f(a)$.

Yours sincerely,
John Y. Benzies

P.O. Box 819,
Riyadh, Saudi Arabia.

REVIEWS

The Mathematical Papers of Isaac Newton. III. 1670–1673. Edited by D. T. Whiteside. Pp. xxxvii, 576. £10 10s. 1969. (Cambridge University Press.)

This third volume embraces the first four years of Newton's tenure of the Lucasian chair, to which he was elected, probably on Barrow's recommendation,* in 1669. His early professorial lectures contained the fruits of his optical researches, and relevant material is reproduced in the third section of this volume.

The major item is, however, the tract *De Methodis serierum et fluxionum*, which was well advanced by the end of 1671 and was intended to appear as an appendix to a book of " Dioptrick lectures ". There were difficulties about publication, as well as the annoyance of the controversy, distasteful to Newton, already aroused by his optical discoveries; so Newton, never the most patient of men, abandoned the project, in order that he might " enjoy my former serene liberty ". Copies of the fluxional manuscript were made, and no doubt were circulated among the favoured few, but not until 1736 was the work made generally available by means of Colson's English version; the original Latin text had to wait for Horsley's 1779 edition of Newton's works. In view of the richness and novelty of the content, Newton's decision not to publish was regrettable; as Dr. Whiteside remarks, had early publication taken place, mathematical history " might well subsequently have taken a different course ".

* Dr. Whiteside gives reasons for regarding as an exaggeration the story that Barrow generously gave up the Lucasian chair in order to expedite the progress of his young disciple.