# Correspondence

*To the Editor,*
*The Computer Journal.*

Sir,

I should like to point out a rather misleading statement made by Kizner in "Error curves for Lanczos selected points method" (this *Journal* January 1966, p. 372), in which he questions the justification for the choice of Chebyshev zeros I gave in this *Journal*, January 1964 (p. 358) and states that the form of the residual I gave is incorrect. This seems to be due to a misunderstanding about the definition of residual.

Suppose one is solving the equation $S(y) = 0$, finding an approximate solution $z$, then I believe the usual terms are:

(i) error $e = y - z$,
(ii) residual $r = S(z)$.

Kizner seems to use both residual and error for (i) and so not surprisingly concludes that my expression for the residual is incorrect.

Clearly one usually prefers to make $|e|$ as small as possible, but $r$ is usually easier to deal with, and not quite so many assumptions are needed to justify the specification of its form.

The use of the concept of residual would also, I think, clarify the main point of his article. He is, in effect, stipulating the form of the error in the solution of a differential equation and considering what effect this has on the form of the residual—in particular the location of its zeros.

In the case of the first-order differential equation:

$$\dot{Y} = F(Y, x)$$

with approximate solution $Q(x), e(x) = Y(x) - Q(x)$, and one obtains

$$\begin{aligned} r(x) &= \dot{Q}(x) - F(Q, x) \\ &= \dot{e}(x) - e(x)\frac{\partial F}{\partial Y} - \ldots \end{aligned}$$

Kizner is in effect neglecting the whole right-hand side except $\dot{e}(x)$. This seems a rather simpler derivation than using the Picard iteration, and shows more clearly, I think, the assumptions involved.

The new choice of points certainly does seem better for the first-order equation without singularities, but it is important to note that this approach will give different results for higher order equations.

Yours sincerely,

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14 February 1966.

*To the Editor,*
*The Computer Journal.*

Sir,

Two points of interest concerning my paper entitled "A stable explicit method for the finite-difference solution of a fourth-order parabolic differential equation", which appeared in this *Journal*, Vol. 8, No. 3, pp. 280–287, have been raised by G. Fairweather and A. R. Gourlay, Dept. of Applied Mathematics, St. Salvator's College, St. Andrews.

Firstly, in Section 5, p. 287, line 3, the paragraph commencing with the statement "Computation of the exact solution . . . etc." is misleading to the context and should read "Computation of the second derivative of the exact solution."

Secondly, owing to premature termination of the Fourier series evaluation of the second derivative of (63) resulted in the tabulated values not being correct to the specified degree of accuracy. The corrected values are as follows:

**Table 1**

*Exact solution second derivative of equation* 63.

| | | | | | |
|---|---|---|---|---|---|
| 0 | 0·03812 | 0·07626 | 0·11373 | 0·14771 | 0·17751 |
| | 0·20317 | 0·22465 | 0·24184 | 0·25223 | 0·25570 |

**Table 2**

*Exact solution second derivative of equation* 63.

| | | | | | |
|---|---|---|---|---|---|
| 0·25 | 0·24613 | 0·22804 | 0·19845 | 0·18146 | 0·16276 |
| | 0·15045 | | | | |

Yours faithfully,

D. J. Evans

Department of Applied Mathematics,
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Sheffield, 10.
10 December, 1965.

*To the Editor,*
*The Computer Journal,*

Sir,

I should like to make two points in reply to the letter[1] by W. Charles Mylander, in which he has discussed a test problem given in my earlier paper.[2]

Firstly, I must point out that in this paper I was concerned with **finding general methods** for solving constrained optimization problems which possessed no special features. I had found that the solution of the small innocuous-looking problem A presented substantial difficulty with the NLP techniques available to me at the time. As elaboration of this simple model was intended, the existing state of NLP seemed unsatisfactory. The Complex method[2] which I devised may well have no merit other than that it enabled the solution of several small problems to be found more readily than was possible with the other techniques then available to me.

Secondly, in setting out a linear programming formulation of problem A, Dr. Mylander has anticipated my latest paper[3], in which I am primarily concerned with the use of elementary transformations to eliminate constraints of certain simple forms from the formulations of NLP problems.

I agree with Dr. Mylander that it is interesting to note that the transformation he suggests reduces problem A to a convex programming problem, whereas in the original formulation neither the feasible region nor the objective function are convex. Thus the transformation not only makes the solution of the problem much easier, but moreover gives a formulation for which the solution and properties are well known, so that it is possible to associate features of the solution with the various terms of the assumed model.

Yours faithfully,

1 March 1966

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(*see overleaf for references*)