CORRESPONDENCE

To the Editor of the *Mathematical Gazette*

DEAR SIR,—I teach scholarship and Common Entrance maths at the above prep. school and wonder if you would find it worth publishing this true little story.

My top form were doing a set of geometrical riders and I have rammed into them that whenever they make a statement in geometry, they must qualify it with a reason ; for example :

$AB = CD$ opp. sides of parm $ABCD$
$AB^2 = BC^2 + AC^2$ Pythagoras.

In one of the riders they had to use the converse of Pythagoras to prove the final part, and one little boy wrote :

$AB^2 = 5$
$AC^2 + BC^2 = 4 + 1$
$= 5$
therefore $\angle ACB$ is a right angle. SAROGAHTYP

Yours etc., J. K. PRIESTMAN

Orley Farm, Harrow-on-the-Hill, Middlesex.

To the Editor of the *Mathematical Gazette*

DEAR SIR,—In replying to the letter of Mr. D. F. Lawden about the laws of friction in the *Mathematical Gazette* of May 1957, I should like to make a few general points.

The version of the laws of friction quoted by Mr. Lawden :

(1) The reaction must lie within the cone of friction, A.
(2) The friction opposes the tendency to relative motion,

represents the view accepted in books by Routh, Loney, etc. Also current is a version which I now paraphrase from *Lamb's Statics*, p. 62 :

> The configuration of a body which touches a rough surface at points $P_1, \dots, P_n$ is an equilibrium configuration if, and only if, there exist forces of reaction $R_1, \dots, R_n$ such that (i) the statical conditions are fulfilled, and (ii) R_i does not lie outside the cone of friction at P_i. B.

Now although A itself is not known to be inconsistent, A and B together are shown to be inconsistent in Note 2510 (*Mathematical Gazette*, May 1955), by the example of a rod resting obliquely against a wall. It seems reasonable to express the matter in this way since, for example, Loney's treatment in § 174 of his *Statics* of a rod resting obliquely over a wall suggests that A and B can be applied according to convenience and that therefore they are surely not inconsistent. Moreover the conjecture that A and B are equivalent is supported by most soluble friction problems including, as is shown in Note 2606 (*Mathematical Gazette*, May 1956), those involving a rigid body which touches a rough plane at a number of points.

Law B states a rule whereby the set E of *equilibrium configurations* of a specified dynamical system may be predicted. The boundary E' of E will comprise the *limiting equilibrium configurations*. In general law A may predict a different set F of equilibrium configuration, with boundary F'. Because of (1) F is contained in E. Rule (2) is open to different interpretations. Thus

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according to Ramsey, but not Loney, the friction forces must oppose what would be the initial motion of the system if everything were smooth. But one might demand merely that the friction forces oppose some one *possible* motion of the system. For an elastic body any motion is possible and one would find $F = E$. One might even allow non-rigid motions when rigid bodies are involved, because they cannot, physically, be ideally rigid. In any case the various interpretations of (2) seem to aim at predicting the set of limiting equilibrium configurations, and before such a theory is acceptable it must be shown that this set bounds. Further, although version A should be generalised to deal with anisotropic friction, there seems to be no obvious way of doing this.

It can scarcely be denied that Law B is simpler than Law A, and Lamb considered it to be the natural generalisation of the (undisputed) laws of friction for a particle. Moreover version A does not correspond to a unique clearly defined general theory, and whether it can be modified so as to do so seems to me to be open. Still it is of course important to make experiments in special cases where A and B lead to different predictions. Naturally, in experimental work, the *stability* of the equilibrium configurations is important. Those which are nearly limiting will tend to be unstable because the dynamic coefficient of friction is usually less than the static. I have heard that some rough experiments initiated by Professor S. Goldstein at Harvard on a rod resting against a wall seem to support version B.

Yours etc., T. A. S. Jackson

University of Liverpool

To the Editor of the *Mathematical Gazette*

Dear Sir,—The sentence " One might ask . . . section " in my review on p. 78 of the last issue of the *Gazette* is of course nonsensical and is due to carelessness on my part. My intention was to point out that the reader is not told that integration is with respect to t and that a new symbol p is introduced without any explanation of its significance. The exposition is of course intelligible and unnecessary to anyone who knows what a Laplace Transform is. The beginner who does not know will, in my opinion, find the explanation in the book inadequate.

Yours etc., D. E. Rutherford

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To the Editor of the *Mathematical Gazette*

Dear Sir,—I am presently a sophomore attending Rensselaer Polytechnic Institute in Troy, New York. While reading a copy of the *Mathematical Gazette* from December 1903 (Vol. II, No. 42), I came across an error in the third problem which I would like to call to your attention, if it has not already been noticed.

The problem reads as follows :

Shew that the result of eliminating x, y, z from

$$x + y + z = a$$
$$(y-z)^2 + (z-x)^2 + (x-y)^2 = b$$
$$x(y-z)^2 + y(z-x)^2 + z(x-y)^2 = c$$
$$x^2(y-z^2) + y^2(z-x^2) + z^2(x-y^2) = d$$

is

$$b^2 - 2a^2b + 12ac - 18d = 0.$$

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The fourth equation, giving the value of d, should read

$$x^2(y-z)^2+y^2(z-x)^2+z^2(x-y)^2=d,$$

in order for the solution to be valid.

As a matter of interest I also eliminated x, y, z from the original four equations the result being :

$$4a^4-12a^3-8ba^2+6ba+24ac-18c+3b^2+108d$$
$$+3\sqrt{3}\sqrt{(2a^3-6c-ab)(2a^3+6c-5ab)-(2a^2-b)^2(a^2-2b)}=0.$$

My method of solution was to employ the identity

$$xy^2+yz^2+zx^2-x^2y-y^2z-z^2x=(x-y)(y-z)(z-x),$$

square both members of this identity, and evaluate the left-hand side in terms of a, b, and c.

Yours, etc., PETER D. ZVENGROWSKI

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To the Editor of the *Mathematical Gazette*

DEAR SIR,—I should like to make a few remarks in connection with Mr. B. C. Brookes' excellent review of *Théorie mathématique du Bridge à la portée de tous* by Borel and Chéron.

I was somewhat startled to find this book described as marking the end of an epoch in the Theory of Games, because that theory—with capital letters—deals with much more general and more abstract aspects of games than a book on probabilities in Bridge. But what prompts me to these notes is a feeling that students of the history of mathematics might be interested in a few facts that might get distorted in the course of time.

The Theory of Games did not start " with a problem posed a few years ago at the poker tables of Princeton ". It is true that Professor Braithwaite finished his delightful inaugural lecture on " Theory of Games as a tool for the Moral Philosopher " by speaking of a " radiation from a source—theory of games of strategy—whose prototype was kindled round the poker tables of Princeton ", but this was, of course, rhetorical. He also reminded his audience of the fact that " Emile Borel in 1921 thought of some of its basic ideas but he was held up by inability to establish the mathematically fundamental theorem ". (I am glad that Mr. Brookes alluded to Borel's contribution.) The latter theorem was established by John von Neumann. Now it is a moot point whether a theory starts with the conjecture of a theorem or by its proof. At any rate, the proof was communicated by von Neumann in a talk given to the Goettingen Mathematical Society in 1926 (published in Mathematische Annalen 100, in 1928), and there was no connection, at that time, with the poker tables of Princeton.

After this insistence on historical truth I think I ought to add that, since about 1944, it was indeed a galaxy of Princeton scholars who have made the Theory of Games what it is now : an edifice based on algebra, geometry, set theory, topology, economics, psychology and sociology, but by no means finished as yet.

Yours, etc., S. VAJDA

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