## Corrections

Mr. J. B. Sutton has written to point out that the solution given in his Note 60.7 (June 1976), *An application of the rectangular distribution*, does not take account of the fact that the minimum duration of an ON period is known to be 26 seconds, so that the statement that $t_i$ is given by $10n_i$ with an error in the range $-10 < u_i < 10$ is invalid for counts of fewer than 4 ONs. Readers interested to see the emended calculations allowing for this restriction are invited to write to Mr. Sutton at the address given at the end of the note.

We regret that by an oversight a number of words were omitted from Note 60.13, *A new proof of a theorem of Erdös and Szekeres*, in the same issue. Lines 8 to 10 from the foot of p. 137 should read: "Thus a quadrilateral all of whose triangles have the same orientation is necessarily convex. It then follows that a polygon all of whose triangles have the same orientation is convex. (Otherwise it would contain a concave quadrilateral.)"

D.A.Q.

# Correspondence

### The languages of mathematical communication

DEAR EDITOR,

I wondered if I may make some general comments on Rolph Schwarzenberger's review of the book *Surfaces*. I do not wish here to argue for or against the book, but to raise certain questions about the educational standpoint that he takes, in common with most other university mathematicians. Rolph's comments indicate innocently that, although he is sympathetic to the learning difficulties of undergraduates, he is basically a hard-liner (though benevolent) and I wish here to challenge the effectiveness of his hard line.

Let me begin by stating a general philosophical position about those aspects of mathematics education which concern problems of communication. It is not sufficiently realised that beginners in mathematics may even lack the feeling for sentence structure and grammar that was once thought to be essential for all educated people. If such structure is not there, then mathematical argument becomes difficult. Thus, the appropriate habits of linguistic thinking ought to be built in to an exposition designed for beginners, and here I am referring to such notions as logical and notational consistency and the use of axioms; these habits tend to be taken for granted by many university lecturers, although those students who have just taken A levels are unlikely to possess such habits. I therefore start from the point of view that most mathematical communication involves a *choice* of topic and then *three languages*. Thus, for various reasons we may wish to teach a particular piece of mathematics, and we find it in the mathematical literature, expressed for professionals in some kind of "official" language L, say, which uses not just an unusual vocabulary, but contains thought habits and nuances of the traditions of the mathematical community. The students for whom the curriculum is being designed have a conceptual stock and language S which they have inherited from their existence in the society in which they live. Now, S is likely to be enormously different from L. Thus a problem arises when we try to produce a bridge language B which will allow the students to appreciate the meaning of the mathematics, expressed in the language L, without their necessarily having to learn all that language first. (For more details, see my article [1], but note that B may contain not just words, but also signs and even facial expressions.) Once the bridge has been securely built, then those students who wish may cross it into the territory of L; but there are many students who may not be able to cross into that territory but nevertheless may get a great deal of pleasure from the inherent meaning; and they may be able to pass on that pleasure, as teachers, to children. Ideally, then, one devises an exposition which is like a long but interesting journey (the bridge) and where people can get off the bus when they are tired, without feeling that they have been passing all the time through an uninteresting desert, with the oasis still miles ahead. In practice, most university mathematics courses are designed by lecturers who want to get to the final oasis as fast as possible (and preferably faster than the people in the next university). In driving their buses over the desert they forget that most of their passengers have been thrown off or numbed to sleep, and the consequences for mathematics education are disastrous. If the bridge (journey) to a piece P of mathematics cannot be designed satisfactorily, then we will probably reject the idea of teaching P (an aspect of *choice*, again). However, pieces of geometry are often suitable for such bridge-building because they have immediate "meaning" for unsophisticated minds; number theory, for example, is not so suitable.

Much of Rolph Schwarzenberger's argument shows that he is thinking solely in terms of the professional language L (here, the way in which professionals think of surfaces). I recall that my book is an attempt to create a bridge-language B, from that of (roughly A level) adults to the language L, and it models parts of L by using paper models, thus reversing the usual process in geometry where a language L is devised to model something in the concrete world. Rolph's worries about it concern the adequacy of the models, the dangers of imposing an odd language, ambiguities, stamina for proofs, and global logic. Let us look at these in turn, because they are likely to be shared by many "academic" mathematicians.

As to models, he agrees that it is important to induce three-dimensional thinking in students, but then says "Pictures of three-dimensional objects by themselves do little to induce three-dimensional thinking". How does one induce thoughts about three-dimensions without putting either objects or pictures in people's way? Some people will seek out such objects for themselves, but most need to have them provided, and their provision is part of the bridge-language B. Rolph then implies that he can follow the various arguments of my book simply from the two-dimensional diagrams (so he does not need three-dimensions in a vital sense), and as a Professor of Geometry he personally may well be able to do so; but I myself cannot and I know few other people who can. I had to compose the arguments either by first making elaborate drawings or modelling plasticene, or by piecing together pieces of paper to make models. I learned a lot about three-dimensions by so doing. Rolph falls into the University mode when he seems to think that scissors and paste are for children and not for undergraduates. Most undergraduates agree with him, thinking it to be very undignified to soil their hands (a sad commentary on attitudes to craft-work in many "academic" secondary schools), but those who can be induced to make models appear to find the process valuable, in a mathematical as well as therapeutic sense.

As to language, Rolph says, truly, "It is uncertain whether to view the material through the eyes of the student, for whom the book is written, or of the children whom the student might later teach". The point is that both types are naïve in spatial thinking and we meet a further expository problem, that language suitable for students may not be suitable for children or vice versa. I compromised by using a language for my bridge B, that is very close to ordinary English, so that an undergraduate could read it more easily, *and*, should he or she teach, then the language could be converted into *spoken* language. One cannot expect children to be able to read mathematics without special training, since so many adults (including Honours mathematicians) find reading it appallingly difficult.

Rolph then complains about my use of certain words, saying that they seem "strangely childish" when compared with the adult stamina needed to follow the argument; but at least they sound familiar, and in fact several of them have no good synonyms in the official language of mathematics (to my knowledge). However, familiarity has its dangers, so for example I deliberately did not use the two words he cites "with essentially the same meaning"; the familiar word "cocoon" for a topological sphere was used rather than "sphere" because beginners think that all spheres are round ones. To generalise the meaning of a word too early can induce a great deal of puzzlement. Rolph warns against the danger of dogmatically imposing a particular language, but some of these dangers are spelt out in the teaching notes (to which he does not refer) at the back of the book. I agree that the danger exists, but at least, by using the language close to ordinary language, one is providing a help for students to talk mathematics back to their teachers, and they are less likely to garble it. It takes a long time to learn to talk back in 'Mandarin mathematics' and it is important to talk back somehow, which is perhaps why few mathematics students ever start (even if their lecturers encourage them to do so). Further, to build up security through familiarity, I gave *names* to theorems rather than—as some tidy-minded lecturers do even when lecturing verbally—referring to (say) Theorem 3.2.5. In the past, some professionals have devised names for certain theorems (e.g. the Sphere Theorem, the Loop Theorem) and mere mention of the name can convey a lot of information. It is a fact about the mathematical community that Rolph should be surprised here: "his" professionals adopt a different dialect from that of "my" professionals.

As to stamina, Rolph contrasts the "childish language" with the stamina rather than sophistication needed in order to follow the proofs. This is because of the mathematical content of the proofs, not because of the language—if so much stamina is required when we use a familiar vocabulary, how much more difficult must it be when people are presented with this mathematics written in the unfamiliar language of analytic topology? Good theorems always have difficulties at their heart, but we should surely try not to create unnecessary difficulties that arise for social, linguistic or other non-mathematical reasons. But stamina is also needed to cope with extreme precision of language, so consider that part of Rolph's criticism of the statement "Every $S_{pqr}$ lies in one of three families..." which relates to the fact that there is really an infinity of families. This is quite true, but a technically precise statement of what I should have said requires a proper use of logical quantifiers, and it is common mathematical practice among professionals (let alone the students for whom the book is intended) to drop quantifiers if the meaning is clear. This is precisely because of a linguistic problem that if you carry around a lot of quantifiers the basic meaning of what you have to say is obscured. For precision, quantifiers are vital, but if we demand too much precision in the early stages then the basic mathematical meaning may well be lost. More generally, not just with quantifiers, you cannot avoid the Uncertainty Situation, that if you put a lot of effort into clarifying (in the sense of professional mathematicians), then a reader needs so much stamina to recreate the message, let alone its proof, that it becomes meaningless for him. A message in simple form may carry ambiguities, but frequently these are not perceived except by very sophisticated people. Sophistication is acquired by going through stages of being dissatisfied with different types of precision; and the high-minded approach of Bertrand Russell, of trying to be "clear" from the beginning, is simply unsuitable for anyone but professionals—and even they have problems.

Finally, Rolph worries about the fact that the whole argument of my book rests upon several unproved properties of surfaces called "agreements". He says "All are acceptable as reasonable assumptions but is not seven a rather large number?". Well, how about the number of axioms one needs for a Durell approach to geometry? The whole point is to cultivate a logical approach by using "local" logic (i.e. deducing interesting consequences of 'reasonable' assumptions) instead of being "globally" logical—which to beginners looks like spending all the time proving the obvious. Academic mathematicians so often love the global approach, and their training conditions their consciences to prick at the "local" approach. So, in choosing seven agreements, I was trying to choose reasonable statements that respect the meaning of the mathematics rather than its syntax. But, says Rolph, "more discussion of the seven agreements is needed for the curious reader and even more important for the curious child who asks why—if so many things are being assumed—he should not assume a few more, and begins to suspect that other unstated assumptions are being made". If such a reader *is* so stimulated then I would believe that I had really achieved something; so often the by-now-traditional academic approach to mathematics is to hammer the student into the ground, telling him what proofs are, asserting that one proof is "rigorous" and another is not, so that he completes a course of mathematics feeling too afraid to question anything at the end. Once a person starts asking about proof or saying that he is dissatisfied with a proof, then we can begin a fruitful discussion about rigour and proof with him. If he demands greater rigour then he will presumably accept a subtler change of standpoint, a higher state of abstraction and the introduction of more refined language. This problem was put very well by the Geometry Panel at a conference in Nottingham in December 1975 (see [2]), when its report contrasted those geometry courses where everything is in place and beautiful, but dead, with the untidy stimulating course which has many things left unproved. Tidiness among professionals is an aesthetic criterion as with gardeners, but it can be totally off-putting to those who are coming to the subject for the first time. Many academic expositors forget that they themselves went through a very considerable period of untidiness when they were learning; and their exposition may itself be the final sign that they have tidied up their minds, and have enjoyed the therapy of doing so.

Only time and criticism can tell whether the particular bridge-language of my book is any good. I hope, however, that my attempt will stimulate others to question their own assumptions about mathematical language and skills.

Yours sincerely,

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*References*

1. H. B. Griffiths, Mathematics education today, *Int. J. Math. Educ. Sci. Technol.* 6(1), 3–15 (1975).
2. Proceedings of the Nottingham Conference, December 1975, on "Adapting university mathematics to current and future educational needs".

# Reviews

**Space structures: their harmony and counterpoint,** by Arthur L. Loeb. Pp xviii, 169. $19·50 cloth, $9·50 paper. 1976. SBN 0 201 04650 4/04651 2 (Addison-Wesley)

Dr. Loeb is a physical chemist with interests in crystallography, a mathematician, musician and artist. As a member of the "philomorphs", a group dedicated to the "search for order", he has written this charming book which crosses many disciplinary boundaries. Basically, it is a study of the topological relationships between the elements of two- and three-dimensional networks which emerge from Schläfli's generalisation of Euler's formula, in the form $N_0 - N_1 + N_2 - N_3 = 0$. By working with *valencies*, that is, the numbers of elements of each dimension incident with a particular element (e.g. the number of edges incident with a vertex), Dr. Loeb is able to deduce a whole set of equations and inequalities which such networks must satisfy. Regular structures are those for which all elements have equivalent environments; semi-regular structures have faces regular and vertices equivalent, or vice versa. Much familiar ground is thus covered without using any metrical considerations; the admission of digons and dihedra introduces some less familiar concepts. Some statistical results are demonstrated for random structures, and all semi-regular structures in two and three dimensions are classified. Chapters on Dirichlet