A Letter and a Reply

Dear Uri,

I've enjoyed reading your article in FLM 5, 3 But if you are going to try to formalise the "arm-waving" then there are some further questions to ask about how one settles for the most perspicacious entry into standard theorems and so on. I found your article very difficult to read (this is about me, *l'homme moyen mathematicien,* not about your communication) though I understood and appreciated the simplicity of the basic idea This discrepancy meant that I am not yet convinced that your suggested metaphor (proof is like a structured programme) is the best way of approaching proof-analysis. I think what you are pointing to is very important so I hope you will bear with me as I stumble along somewhat naively in your wake

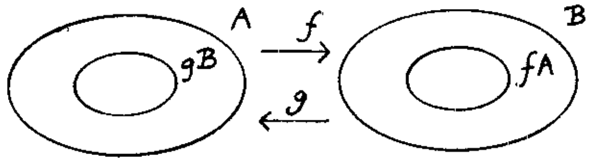
There are two immediate stumbling blocks for me. First, you did not really explain the setting (*why* should it be important to generalise the $m \leq n$, $m \geq n \Rightarrow m = n$ argument?) Second, your diagrams seemed to have been invoked to illustrate a pre-conceived argument in your head; I prefer to be offered imagery which then determines or at least suggests the next approach. I felt that the basic image required was one that displays the to-and-fro injections; hard work on your text suggests this to-and-fro movement was in your head — but it did not seem to be in your diagrams (for me).

Proof analysis does not feel like programming to me. But then I am not very good at either! Incidentally, a very perspicacious proof of the Cantor-Bernstein theorem was proposed by Papy in his 1963 text for 12-year-olds. The onus on later writers must be either to criticise his treatment (a veritable tour-de-force which neatly sidesteps choice-axiom issues) or to offer even better access I think you would agree that you were not making the theorem accessible to 12-year-olds in your article

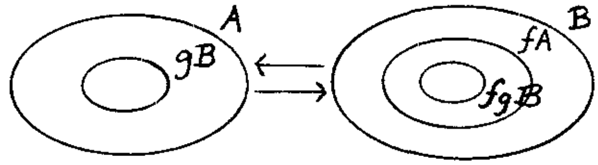
About the context of the theorem: a trivial but not to be underrated stumbling block for students is the question of names. Who was Cantor? Who Bernstein? Why both of them? (As far as I know Cantor conjectured the theorem in a seminar attended by Bernstein who proved it; it was also proved independently by Schroder at about the same time, 1896-8.) Another stumbling block may be that the professional's arm-waving may be only a flap of the wrist for a beginner. The point of the theorem is presumably that it helps establish that transfinites can be ordered — i e., that one of two distinct ones must be greater than the other This will be an obvious meaning for you of the $m \leq n$ .. bit, but not to naive newcomers like me.

So the *intention* seems to be to show that the proposed ordering relation is anti-symmetric. In terms of the injections, f: $A \rightarrow fA \subset B$ and g: $B \rightarrow gB \subset A$ should imply the equivalence $A \sim B$ (through a bijection).

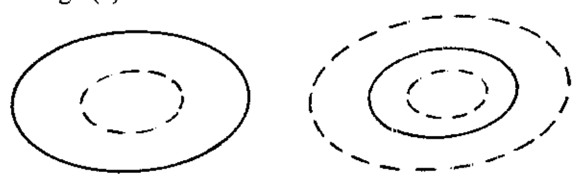
The first step is to offer an image (1).



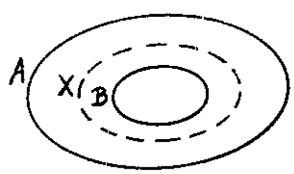
This immediately feels incomplete; gB needs to be mapped back to fgB, suggesting a further image (2) and immediately raising the sense of a to-and-fro regress ..



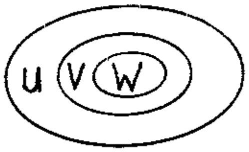
Looking at this image another way — in terms of the bijections — suggests some equivalences: $A \sim fA$ and $B \sim gB \sim fgB$. These can be emphasised by colour coding the image (3)



The right-hand part of this is very suggestive — of a possible sandwich argument. fA is trapped between B and the equivalent fgB. The proof analysis replaces the theorem being discussed by a sandwich theorem. (This feels like a displacement to me rather than a descent to a lower level; but I expect it could feel different for others.)



So now we have to consider the sandwich theorem: reframe and re-draw this and commence its proof analysis. Here $A \supset X \supset B$ and $A \sim B$ should imply $A \sim X$. The opening diagram suggests that there is going to be a need to look at what happens in different regions and these might be labelled in order to ease bookkeeping.



Now $A \sim B$, with B a proper subset, means that A is infinite, which also means (in Papy's treatment) that there are distinct chains (*ribambelles*) from all distinct elements of U to W:

If there are any isolated points v', w', not in one of these chains, then they may be mapped onto themselves (*ornons d'une boucle*). The chains $U \rightarrow V$ together with the reflexive loops now define a bijection $f: A \rightarrow X$ so that $A \sim X$ as required.

Of course there is no ending to this im-proving of provings. Ultimately our improvements are rehearsals of our own understanding (and public display of our narcissistic delight). This can all too easily make the path for others too smooth. But I do think we have to continue to pay attention to what it is we are asking students to work on. I am prejudiced in favour of images. I thought you were proposing a metaphor rather than an image (an algebraic entry rather than a geometric one?) Maybe some people would prefer this; maybe your next article might contrast different types of *entry* into proof analysis. But for the moment I personally want to find geometric ones.

Dick Tahta

Dear Dick,

The way I see it, there is hardly an argument between us. We are simply talking about different issues. My article is not intended to present a complete educational philosophy or practice. Neither is it about "how best to teach the C-B Theorem to kids or adults". The article is restricted in purpose to a limited, well-defined section of the teaching/learning process: the organisation and communication of previously-discovered proofs. The following remarks, then, are not really "answers" to your comments (with which I mostly agree). Instead I should like to indulge in some free-associating, triggered by your letter and our earlier discussions.

Formalism vs. arm-waving. What I am trying to do is not "formalise the arm-waving", but rather "armwave-ise the formalism". The difference is important, since it means pulling the formalism a little towards the arm-waving, not the other way around. In this sense, my two articles on structuring proofs were addressed more to the mathematical community than to the math ed community. Or was this a feeble attempt at narrowing the gap between the almost incompatible world views of the two cultures?

The Cantor-Bernstein Theorem: what proof? what style? The main issue of the article is structured proofs vs linear proofs, or how to make the formalism more communicative. This particular proof of the C-B Theorem only serves as an example, to help make the general discussion more specific. To demonstrate the benefits of the structural method, I have chosen on purpose a proof that is elegant but hard to understand. Papy's proof is more communicative but probably less "elegant". (Note again the tension between the aesthetics of the professional mathematician and that of the math educator.) Personally, I wouldn't dream of teaching this proof to kids. I had taught it to college students and saw how hard it was to digest despite its apparent simplicity. Since this was not meant to be a didactic presentation of the proof, I have also omitted the other parts—history, motivation, consequences—you found lacking.

Packing and unpacking. I enjoyed your proof analysis using the sandwich theorem. But suppose you have finished analysing and investigating the proof with your students, and you now wish to summarise and store your results in a non-redundant, rigorous proof. Then again you are likely to find that the standard linear formalism hides any trace of the intuitive, global ideas that gave coherence to the proof, while a structured proof does offer some hope. More generally, the game of mathematical communication largely consists in packing mental images and intuitions into words and symbols at one end of the channel, then unpacking them at the other end. If we view formalisation as an exercise in communication (= "community-cation"—sharing your ideas and discoveries with the community), then I believe the superiority of the structural method over the linear method becomes apparent, since it makes unpacking so much easier.

Proofs and programs. I agree that proofs are not like programs, though I find the analogy interesting and fruitful. I do believe they are very much alike in their aspects that concern organisation and communication of complex processes. In particular, the very convincing arguments made in modern computer science in favor of the structured vs. the linear style of programming (in which I do not include rigid top-drawn practices), apply with equal force to the presentation of mathematical procedures. An intruiging question in this respect is, how come this wonderful piece of wisdom was discovered and adopted by 40-year-old Computer Science rather than by 2500-year-old Mathematics. Certainly it is not lack of talent. I think it shows how little mathematicians (at the community level) care about communication issues. In computer science, good communication practices are a central and respectable issue. If nothing else, bad communication there costs a lot of money.

How human is mathematics? One issue that keeps surfacing in this discussion is the different value systems, the different "aesthetics", of the two communities: math educators and professional mathematicians. At the level of communities (neglecting small minorities within each community), these value systems are best studied not by what people say, but by how they behave professionally. Consider for example the expected writing style in research journals or the procedures for academic promotion. At this level, the two value systems are not only different—they are almost orthogonal. For in education we are always stressing the human side of mathematics, whereas official pure mathematics has always evolved in the direction of more objectivity, formality, "purity"—in short, away from the human, personal perspective. Building bridges between these two cultures seems to me a task that is both important and difficult. I believe topics like the relationship of proof to mental images and "generic" objects (or generic arguments), and the gradual refinement of intuitive argument to formal proof, are good starting points for such bridges. Unfortunately, because of the atrocities performed in the name of mathematical formalism on people of all ages, math educators have largely tended to avoid this topic. I hope we see more "human approach to formalism" in the future.

Uri Leron

Continued from page 38

Discussion

La détection des biais dans les tests est un problème fort complexe. On peut l'entreprendre par une analyse logique suivie d'une analyse empirique statistique.

> The entire process of establishing that sets of items are unbiased is the same as the process of construct validation of the test. For tests or items, this requirement is larger than strictly actuarial or statistical validity. The demands for logical validity include not only verification of the match between items and constructs but also a coherent theory regarding relations among variables, expecially between the test and criterion performance. Logically deduced patterns must then be confirmed empirically, in the first case by internal item bias methods and in the second by external relations.
> [Shepard, 1982, p. 26]

Les auteurs ont fait ressortir les différences qui peuvent paraître dans un test polyglotte et qui ont échappé à des experts dans le procédé classique de traduction-retraduction. Ces différences ne sont pas nécessairement toutes des biais, mais elles en soulèvent la possibilité. Des recherches empiriques sont nécessaires pour confirmer la présence ou l'absence di biais. Il serait nécessaire de comparer le rendement d'élèves avec une version corrigée des tests, à celui obtenu avec la version déjà utilisée.

Ces différences font aussi ressortir la difficulté de comparer les rendements obtenus dans les différents pays qui ont participé à la seconde enquête internationale sur l'enseignement des mathématiques sans vérifier si de telles différences existent entre les autres versions des tests et si de telles différences sont des biais. Il est clair qu'une comparaison doit être faite, item par item, par des experts qualifiés, avec des critères bien établis. Les auteurs suggèrent des critères déjà soulevés, soit le niveau de langage, la clarté, le sens, la présentation typographique et les processus cognitifs suscités. Des recherches pourraient en établir d'autres.

Bibliographie

McKnight, C C, Travers, K J & Dossey, J A Twelfth-Grade Mathematics in U S High Schools: A Report from the Second International Mathematics Study. *Mathematics Teacher* 78, 4, April 1985. 292-300, 270

McLean, L., Raphael, D & Wahlstrom M The Second International Study of Mathematics: An Overview of the Ontario Grade 8 Study *Orbit 67,* 4, 3, Oct. 1983, 4-7

Robitaille, D F, O'Shea, T J & Dirks, M.K. *The second international study of mathematics: the teaching and learning of mathematics in British Columbia Mathematics 8 and Algebra 12.* Learning Assessment Branch, Ministry of Education, Province of British Columbia, 1982

Shepard, L A Definition of Bias. In R A Berk (ed.), *Handbook of methods for detecting test bias.* Baltimore: The Johns Hopkins University Press, 1982