0 Preface

Practising mathematicians are supposed to <u>do</u> mathematics, i.e., to design and to present solutions, theorems, proofs, algorithms, and, in the more ambitious case, whole theories. Amazingly —and regrettably— this is hardly reflected in the traditional mathematical curriculum. The traditional curriculum teaches mathematical facts, i.e., it teaches existing theories, concepts, and methods that have withstood the test of time; on the <u>doing</u> of mathematics, however, it is almost totally silent.

For centuries, we can now distinguish two extremes of the spectrum of different ways in which one generation tries to transfer to the next its knowledge and abilities.

At the one end of the spectrum we have the educational practice of the guilds. The original guild was an association of people for whom the secrets of their common craft were of great, usually economic value; consequently, secrecy and restricted membership were typical characteristics of the guilds. Membership was restricted by erecting barriers for the admission of new members, who first had to join for seven —often very!— meagre years a master as his apprentice. Secrecy was maintained by never explicitly formulating or recording the secrets of the craft: the apprentice had to pick them up from his master by imitation and osmosis.

At the other end of the spectrum emerged what, for lack of a better name, I shall call the practice of the universities. The relation between the professor and his students is very different from the relation between the master and his students. It is the professor's task to formulate knowledge and methods as explicitly as possible, thereby bringing them into the public domain, and it is the task of the students to act for their teacher as a challenging whetstone. The absence of secrecy is the hallmark of the university tradition and I don't think it is an accident that the universities as we know them now started to emerge when the art of printing became established.

Many a discipline occupies in this spectrum a multiple position. Medicine is a very clear example: pathology and physiology are taught in the university tradition, whereas clinical medicine is almost exclusively taught in the tradition of the guilds. (It is in this connection

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revealing that, at some US universities, the School of Medicine is "associated with" but not "part of" the university.) In the case of medicine, this need not amaze us: here, the same economic forces that led to the original guilds are at work today.

A little bit more surprising is that mathematics occupies a similar multiple position: existing mathematics is taught quite openly, while the doing of mathematics remains covered by a shroud of mystery. If design and presentation get any attention at all, they are treated as separate issues: the process of mathematical invention is viewed as belonging to the realm of psychology, and presentation is considered a matter of personal style (or of pandering to the taste of the target audience). As will become clear, this attempt at separating form from content is unfortunate.

Remark. In the software industry, the situation is even worse in the sense that design and documentation are often done by different persons. The design is done by "software engineers" who are assumed to be illiterate, and the subsequent documentation is attempted by "technical writers". Experience has shown —over and over again—

that in this division of labour nothing prevents the software engineers from coming up with designs so complex that they, in fact, defy documentation. As we shall see, the same mistake is made by the mathematician that tries to solve a problem without paying attention to the question of how to record his solution. (End of Remark.)

Our purposes are manifold: we wish to streamline the mathematical argument, to demystify the process of mathematical invention, and to turn the design of crisp arguments into a teachable discipline. To this end we shall have to make a lot explicit that the mathematical guild traditionally leaves unsaid, and we want this explicitness to cover both the crisp argument itself and the considerations that went into its design.

In pursuing these goals, we became more and more aware of the virtues of calculational arguments, carried out in a formalism that reduces proof obligations to targets to be reached by formula manipulation.

When rendered in a strict format, such calculational arguments strongly discourage all sorts

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of fudging that is only too common in the traditional guild member's "Emment~~h~~aler proofs" —full of holes— and they therefore tend to be advocated for their greater security against oversights. A further —and perhaps more interesting— virtue of calculational arguments is that, because one's freedom is so clearly delineated, they are much easier to design in a systematic manner. Symbol manipulation is refreshingly tangible in comparison to the Emment~~h~~aler proof, which is too elusive an object to develop a teachable design methodology for. (This experience is similar to the one in computing science, where programming methodology could only come off the ground after programs had been accepted as formal objects.)

A completely different advantage of calculational proofs with their limited repertoire of manipulations is the new light they shed on notational conventions, an issue that is in design and presentation of equal importance. As long as formulae are used for descriptive purposes only, choice of notation remains primarily a matter of taste, style, and convention. Calculational arguments, however, provide a context in which one can raise the question how well a proposed notation is geared to our manipulative needs and, with this question, choice of notation suddenly becomes a technical issue. Here is one of the areas in which we must be prepared to see the meanings of "conventional" and of "convenient" diverge. (As long as numbers are only used to chisel on buildings the years in which they were built, Roman numerals are fine; as soon as we want to do arithmetic with numbers, the Hindu/Arabic decimal system —though unconventional when it was introduced in Europe— is objectively more convenient.)

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Life would be much easier if the proposal to develop such a mathematical methodology were less controversial, for now I am in constant danger of being dragged into polemics (which I dislike). This, however, seems the place where I cannot avoid the discussion, so let me give you a catalogue of the most frequently heard objections and let us try to analyse how valid they are.

The simplest objection to deal with is an indignated "But that is not how mathematicians work!", against which one could retort that, while many people profess to be in favour of progress, some of them are even more in favour of progress without change.

The second objection is that, by playing down or perhaps even denying a rôle for such things as "intuition" or "mathematical insight", (i) I distort the "true nature of mathematics" and (ii) I had better read my Hadamard, because my proposals are completely at odds with how great mathematicians of previous generations have described their processes of invention. Here the answer is "Hark, the guild member speaks.". What other perception of the "true nature of mathematics" and what other description of the process of invention can you expect from people trained to see such things as a matter of "intuition" and "insight"? Note that these terms, that hide more than they reveal, can be viewed as tools of secrecy, used by the guild to prevent the feared explicitness from emerging. (Recently, one of my proposals was called "dangerous". I am not amazed: you cannot expect guilds but to feel threatened when someone tries to blow the gaff.)

The third objection is one of the historical necessity of failure: "It won't work because if it could, it would have been developed a long time ago.". This objection (which is often misused as a general-purpose dismissal) does not hold water because it is quite possible that something has not been pursued in the past because the people that could have done so just disliked the idea. In the current case, such a dislike is more than likely. I have discussed these matters over the years with many colleagues and have often encountered the feeling that the gaps in the Emment~~h~~aler proofs are at least half the fun. They have told me that as readers of proofs they are tickled by the challenge of detecting the gaps and then figuring out how they should be filled and that they would be bored by reading a proof for which the author has done all the work. As authors of proofs they are tickled by the challenge of sketching proofs without having actually constructed them. I have no reason to doubt their words, but it raises the question of what we are doing: are we tickling our colleagues and ourselves, or are we doing mathematics? Besides this rather technical appreciation of Emment~~h~~aler proofs I often encountered a more romantic one. It comes from people who are overwhelmingly thrilled by the mysteries of the human mind and of how one human communicates his "insights" to another; for them, the miracle of mathematics culminates in these mysteries. (This attitude is primarily voiced by Roman Catholics and in Anglo-Saxon countries, where "the humanities" are more rampant than on the European continent.) Not denying the mysteries, I would prefer to move mathematics away from them, rather than enforcing the link; my main reason, of course, is teachability. Finally, I also found that efforts like mine, striving at greater perfection and greater control, can be strongly condemned on moral grounds. In a confusion between the goal of perfection and the claim of infallibility, reproaches of intolerance, presumption, and arrogance, I learned, are easily made; one of my colleagues -admittedly of a rather orthodox Protestant background- characterized my aim of imparting a greater intellectual control to our students as indecent, immoral and dangerous (essentially because one should not put so much raw reasoning power at people's disposal). To my taste, such rejection smacks too much of a defence of mediocrity.

A much more serious objection is "Your dream has been proved to be unrealistic: great men, like Leibniz and Hilbert, for instance, have had similar dreams, and it did not work.". This is a much more serious objection, because now we have to give a technical reason why the current generation could succeed where previous ones failed. In the case of Leibniz, the general consensus is that, with his "Characteristica Universalis" and its "calculus raticinator", he was at least two centuries ahead of his time. But why should we stand a better chance than, say, Hilbert and his contemporaries?

An undeniable difference between them and us is that their work predates the automatic computer, which is now with us for, say, four decades and is quickly becoming ubiquitous. If the "computer revolution" is really the revolution it is supposed to be, it should have a major impact on our perception of what the doing of mathematics could be. At the turn of the century, the reduction of mathematical reasoning to systematic formula manipulation —i.e. the manipulation of uninterpreted formulae— emerged as an intriguing possibility, but primarily as a possibility of something that could be done "in principle". For the first half of this century, mathematical logic was hardly used: mainstream mathematics felt entitled to ignore it as utterly unpractical and the soul-searching logicians did not use it either because they were primarily engaged in exploring its limits. But the advent of the automatic computer has changed all that: when all is said and done, the systematic manipulation of uninterpreted formulae is the only thing these gadgets do for us. By their mere presence and our efforts at using them, they have really driven home the message of what is meant and implied by the manipulation of uninterpreted formulae. We now live in a different culture.

Let me report to you two personal experiences that made me aware of the extent of that cultural difference. The one was when —I guess around 1970— I read Polya's "How to solve it". I read it with sympathy and admiration because the topic was dear to my heart and because I was reading the work of a pioneer. In succession, I noticed the following reactions of mine: "It is nice, but also very dated.", "Obviously, he has never programmed.", and "He would have been an absolutely terrible programmer!". The other experience I had with Whitehead, who had my full sympathy for the eloquence with which he argued the advantages of "letting the symbols do the work". Can you imagine my utter amazement when, half a page later, he praises the mathematical community for the economies it had obtained by the introduction of the invisible multiplication sign? The remark was very revealing, for it showed that he lived in a world upon which the full implications of systematic formula manipulation had hardly dawned.

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These are just two of the many experiences that have convinced me that we do indeed live in a different culture and have a better chance than our predecessors. In particular, unfortunate former experiences with a more calculational approach should not deter us in our search for a teachable mathematical methodology since there is ample evidence —notational and otherwise— that formal techniques have never been given a fair chance.

Let me end this section with a somewhat sad anecdote. In the sixties —I was then with a department of mathematics— I conducted an investigation among my colleagues. I phrased it as a hypothetical question: "Suppose we knew how to give a solid two-semester course on Thinking, how would you feel about including it in our freshmen curriculum?". Asked in private, quite a few ended up with an answer along the line: "Well, if such a course could be given, I would personally be in favour of it but, to say the honest truth, I don't think the colleagues would allow it.". The last one happened to be faced with the question in public; he panicked and gave an impromptu harangue in which he began with arguing that such a course was obviously impossible and ended up this was precisely the type of course he had been giving all the time.

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At first thought one might think that, in order to really sell the message, I should show the relevance of my considerations in mathematical areas as many and as varied as possible, but at second thought that suggestion has to be rejected.

Firstly, I find the underlying suggestion that the specialist is only willing to look at things that are presented in direct connection with his specialty rather offensive.

Secondly, the effort would be vain, for, in its diversity, the field of mathematics is so vast that no single person can be familiar with more than a negligible fraction of it.

Thirdly, the effort would be ill-directed because a few areas, almost all of them familiar to almost all of us, suffice to provide a carrier for an illustrative example. Extending the amount of mathematical knowledge and terminology involved would only make the book harder to write or to read.

Finally, I would prefer to maintain the distinction between the scientist and the salesman. I feel —and try to honour— a professional obligation to offer the fruits of my considerations and to do so as clearly as I can, but prefer to do so neutrally, on the basis "Take it or leave it.".

For all those reasons I tend to confine my universe of discourse to those domains that are reasonably familiar, such as booleans, integers, reals, sets, graphs, and some Euclidean geometry. (At this stage I cannot promise that this enumeration will turn out to have been exhaustive.)

In short, in the choice of mathematical areas, I accept the type of constraints accepted in Mathematical Olympics and similar contests. This choice could therefore easily create the impression that this book is primarily a guide to problem solving and that the youngsters preparing themselves for such contests are my primary target audience, but they are not. If I help them in their contest, that is fine, but I would like to help them in the rest of their mathematical lives.

The book is on how to keep things as simple as possible, on what it means for arguments to be crisp, and on how to make them so; it so happens that the knowledge of how to pursue these goals greatly enhances one's problem-solving ability, but the latter is a —be it

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welcome— byproduct.

My target is to write a book that deserves to be read slowly, not as slowly as it has been written, but markedly slower —say at half the speed— than the average technical text, not because I am aiming at a difficult text —on the contrary!— but because I hope to relieve the reader from the job of filtering out the noise by not including it. Whenever you notice that I fail to keep your full attention, you probably started skipping formulae —always the first victims— some time before. Put the book away and when you resume reading it, do it at an earlier point than where you stopped.

Above all, however, I hope that you will read the book with as much pleasure as I intend to write it with.

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[To be completed later.]

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