Why mathematicians do not love logic*

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And the Lord said, Behold, the people is one,
and they have all one language; and this they begin to do:
and now nothing will be restrained from them,
which they have imagined to do.
Go to, let us go down, and there confound their language . . .
Genesis, 11.6-7

The low esteem in which logic is held by mathematicians in modern times is known, at least to logicians in mathematics departments. It could be dated back to Descartes.

According to Descartes logic and "its syllogisms and most of its other instructions are more useful to expound one's knowledge to others . . . than to learn something new"[1]. Only episodically it may happen that we reach a conclusion which is certain only on the basis of form, without the clear and direct consideration of the object. On the same basis he found faults also with geometry (too tied to drawings, hence to imagination) and with algebra (a messy bunch of rules for signs).

The low relevance of deduction is proved by the fact that not even errors come from wrong inferences: "Deduction, or the simple inference of something from something other, can be omitted if one doesn't perceive it, but couldn't be applied in a mistaken way not even by the intellect less capable of reasoning. But to get it [knowledge] I find of scarce utility these chains

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[1] *Discours de la Méthode* (1637), in *Oeuvres et léttres*, Gallimard, Paris, 1953, p. 137.

through which dialecticians think to regulate reason, though I do not deny that they can be useful for other purposes"[2].

We are not going however to follow the evolution of the relations between mathematics and logic; we are interested in present times, when logic has become *mathematical* logic.

1 Logic's indictment

There are two charges brought against logic by mathematicians, when they take the trouble to discuss it.

Definite positions are rarely explicitly stated, but the prevailing opinion in the mathematical community is a disparaging one, as can be seen by the overall academic policy in most of western countries. Courses, when offered, are of a character that is ill-suited to make clear the nature and possible importance of logic, so that negative judgments, mostly delivered *ex ignorantia*, reinforce themselves from one generation to the other.

The first indictment is that logic brings with it a (bad) philosophy of mathematics. This philosophy, or philosophical attitude, could be variously labeled "formalism" or "deductivism"; it goes hand in hand with the censure of the pure axiomatic conception; the faults of axiomatics need not be defined in a precise way, but they amount to the elimination of sense and content from mathematics. Logic is considered also the vehicle of some dangerous myths, such as that of infallibility[3].

The second (connected) charge is that mathematics when done and presented according to the rules of logic is a caricature of its normal self. This can be traced back to Poincaré: "If it takes 27 equations to establish that 1 is a number, how many will be needed to prove a true theorem?"[4] Adversaries think that logic dictates a full formalization of the mathematical discourse, in particular of proofs (and, as I hinted above, this is what they expect from logic courses).

It is widely acknowledged that logic provides with its formal deductions

[2] *Regulae ad directionem ingenii* (1701), in *Oeuvres et léttres*, Gallimard, Paris, 1953, p. 41.

[3] For a widely acclaimed denunciation of these evils see Ph. J. Davis - R. Hersh, *The Mathematical Experience*, Birkhaüser, Boston, 1980.

[4] H. Poincaré, "Les dernières efforts des logisticiens", in *Science et Méthode*, Flammarion, Paris, 1908.

the only precise definition of "proof", a template for proofs; but then an awkward uneasiness follows from the fact that full formalization is unfeasible, with the ensuing hardly tenable distinction between "in principle" and "in practice"[5]. Even when feasible, for fragments of logic, the constant use of pictures and other devices in proofs reminds us that "conviction and clarity are not adequately achieved by the proof"[6]. Then proof itself sometimes is damned[7] together with its formal counterpart.

A different sort of discomfort arises from the knowledge of (the existence of) results such as Gödel incompleteness theorem; here, at least in popular accounts, it is logic itself which denies the possibility of a formal reduction of mathematics, contrary to what should be logic's sermonizing. But this inconsistency is perhaps too subtle to cope with, and better passed over in silence.

Set theory, as a kind of logic, deserves a few remarks apart. The general attitude is equivocal. Set theory is accepted as a language, as *the* language of mathematics. This does not mean that the theory is appreciated in its fine points (as a field of experiments in abstract creative thought), or even known. It has been put at the beginning of Bourbaki's *Éléments*, to give the grammar of the mathematical language, but it needs not be studied, since languages are learnt through use.

The set theoretical frame is handy, since it allows Platonists to speak of a mathematical universe. But realists, though more numerous than formalists, do not exhaust the mathematical community. The reason why the set theoretical frame is so widely uncritically accepted is probably the following: mathematicians have a poor historical sense, they like to believe that mathematics has grown by accumulation of various results, all on the same plane, without discontinuities. Sets as the common stuff of all concepts answer this feeling, in that everything in the theory is flattened on one type, all objects have the same essence.

Of course independence proofs, of CH and other mathematical statements, of which everybody has heard, are a problem, since mathematicians are not

[5]For a mocking rendering of this situation see K. Devlin, "Computers and Mathematics", *Notices AMS*, **39**(1992), pp. 1065-66.

[6]Ph. J. Davis, "Deductive mathematics", in *Mathematics and Common Sense*, A K Peters, Wellesley MA, 2006, p. 79.

[7]In some empiricist or humanistic philosophies. See G. Lolli, "Mathematics as a human endeavour", Proc. Conference *Philosophy of Mathematics Today*, Pisa, Scuola Normale Superiore, january 23, 2006.

ready to accept a plurality of universes, as was the case with non-euclidean geometries. But the problem cannot even be approached, because of the lack of the necessary expertise.

2 Confessions of a reluctant logician

The opinions summarized above have to be extracted from implicit considerations, often contradictory, sometimes addressed to other topics.

The one exception to the unwillingness to discuss the uneasiness the working mathematician feels in regard to logic is represented by Paul R. Halmos. Halmos knows logic well, having given important contributions to algebraic logic, and that's why he probably feels enough confident in his beliefs to make them public.

We are going to read and comment his remarks (often no comment is needed), contained in the autobiography[8], in a section entitled "Is formal logic mathematics?".

> Logic has always fascinated me. In the common-sense sense of the word logic was easy; I had no inclination to commit the obvious errors of reasoning, and long chains of deduction were not intimidating. When I was told about syllogisms[9], I said "sure, obviously", and didn't feel that I learned anything.

Descartes' remarks come obviously to mind.

> The first genuine stir in my heart towards logic came from Russell's popular books, and just a few weeks later, from Russell and Whitehead: the manipulations of symbolic logic were positively attractive. (I liked even the name. "Symbolic logic" still sounds to me like what I wanted to do, as opposed to "formal logic" ... At a guess, the old-fashioned word wanted to call attention to the use of algebraic symbols in place of the scholastic verbosity of the middle ages, and the more modern term was introduced to emphasize the study of form instead of content. My emotional reaction is that I like symbols, and, in addition, I approve of

[8] P. R. Halmos, *I want to be a mathematician*, Springer, New York, 1985, pp. 202-6.

[9] But what was he told?

> them as a tool for discussing the content–the "laws of thought"–, whereas "formal" suggests a march-in-step, regimented approach that I don't like. One more thing: "symbolic" suggests other things, "higher" things that the symbols stand for, but "formal" sounds like the opposite of informal–stiff and stuffy instead of relaxed and pleasant. ...)

Again Descartes. The "formal" has a negative connotation. This is not so with every mathematician. There are many definitions of mathematics as "science of forms"; in such cases however the form is not empty, as it is in formal logic, it's a pattern.

> I liked symbolic logic the same way I liked algebra: symplifying an alphabet-soup-bowl full of boldface p's and q's and or's and not's was as much fun, and as profitable, as completing the square to solve a quadratic equation or using Cramer's rule to solve a pair of linear ones. As I went on, however, and learned more and more of formal logic, I liked it less and less. My reaction was purely subjective: there was nothing wrong with the subject, I just didn't like it.

Logic is a calculus. It is an object of symbolic manipulations.

After this qualified statement of his liking of logic, Halmos imagines a discussion with a straw-man logician trying to argue that logicians are mathematicians, since they have the same rigour and their purpose and fulfillment is to prove theorems.

> What didn't I like? It's hard to say. It has to do with the language of the subject and the attitude of its devotees, and I think that in an unexamined and unformulated way most mathematicians feel the same discomfort. Each time I try to explain my feelings to a logician, however, he in turn tries to show me that they are not based on facts. Logicians proceed exactly in the same way as other mathematicians, he will say: they formulate precise hypotheses, they make rigorous deductions, and they end up with theorems. The only difference, he will maintain, is the subject: instead of topological notions such as continuity, connectedness, homeomorphisms, and exotic spheres, they discuss logical notions such as recursion, consistency, decidability, and non-standard models.

If these are the arguments the friend uses to correct Halmos' feelings, that says a lot about such feelings: do not logicians work like other mathematicians, do they not prove theorems, do they have and expound vague or jumbled thoughts?.

> No. My logician friend misses the point. Algebraic topology is intellectually and spiritually nearer to logic than to organic chemistry, say, or to economic history–that much is true. It make sense to house logicians of the university in the department of mathematics–sure. But the differencs between logic and the classical algebra-analysis-geometry kind of "core mathematics" are at least as great as the analogies on which the logician is basing his argument. ...

Unfortunately Halmos does not expand on these differences[10] and goes over to mention logic's most important achievements.

> He argues well, that logician, and he has a lot going for him. Certainly every mathematician finds the famous accomplishments of Gödel and Cohen, about the necessary existence of undecidable propositions and about the consistency status of the axiom of choice and the continuum hypotesis, both fascinating and admirable. On a less spectacular level, logicians have shown us that there are unsuspected relations between the Peano axioms and Ramsey's theorem, and both of those subjects are indubitably parts of "core mathematics". And, finally, logicians point proudly to a handful of mathematical theorems whose first proofs used the techniques of formal logic; the one nearest to my work is the Bernstein-Robinson result concerning invariant subspaces of polynomially compact operators.
>
> All this is true, but it is not enough to alleviate the mathematician's discomfort. Why not? A partial answer is that, however admirable some of the accomplishments of the logicians might be, the mathematician doesn't need them, cannot use them, in his daily work. The logic proofs of mathematical theorems, all of them so far as I know, are dispensable: they can be (and

[10]The dots do not cover anything relevant to the point.

> they have been) replaced by proofs using the language and techniques of ordinary mathematics. The Bernstein-Robinson proof uses non-standard models of higher order predicate languages, and when Abby (Abraham's Robinson's nickname) sent me his preprint I really had to sweat to pinpoint and translate its mathematical insight. Yes, I sweated, but, yes, it was a mathematical insight and it could be translated. The paper didn't convince me (or anyone else?) that non-standard models should forthwith be put into every mathematician's toolkit; it showed only that Bernstein and Robinson were clever mathematicians who solved a difficult problem, using a language that they spoke fluently. If they had done it in Telegu instead, I would have found their paper even more difficult to decode, but the extra difficulty would have been one of degree, not of kind.

In the above passage Halmos attacks first the issue of mathematical theorems proved for the first time with logical techniques. His reasons are clearly stated and need not be repeated. But Halmos fails to ask himself the following question: if a mathematical insight is prompted or induced by logical concepts, are these not mathematical, and isn't worthwhile at least to know something of the language logicians speak so fluently?

Mathematicians cannot use the result of the logicians, according to Halmos, because they are expressed in another language. Is it impossible to be fluent in more than one language?

Halmos insists on (the necessity of) translation. Such a necessity arises also for the disciplines in the core mathematics: algebraic, geometrical or analytical arguments have often to be and can be translated one into the other.

Halmos says that he translated Robinson's insight in terms of concepts he was familiar with; he did so because he is a clever mathematician, but the translation was made possible, here as in many other cases, from the fact that many of the logicians' insights have given rise to constructions which belong to the same family of already existing ones, but that mathematicians had not had the insight to invent, nor could have had.

The ultrapower construction is a case in point; it is a purely algebraic construction, which translates a consequence of the logical compactness theorem, but to take full advantage of it one has to know its properties as stated by Łoś theorem, which refers to all the formulae of some algebraic language.

Otherwise one must be content with local applications, of a translation, to single formulae or problems.

It is true that the translation between a logical and a mathematical argument is sometimes harder than the usual intercourse between mathematical theories, the reason being that the objects of the logician are the very languages of mathematics. The relation at play here should probably not be called a translation. We'll come back to this issue.

Halmos' next remarks on non-standard methods have clearly a wider import.

> As long as I am on the subject, this is the time to put into words what I suspect is the role of non-standard analysis in mathematics. It is a touchy issue: for some converts (such as Pete Loeb and Ed Nelson), it's a religion, and they get touchy if someone hints that it has imperfections. For some others, who are against it (for instance Errett Bishop), it's an equally emotional issue–they regard it as devil worship. To most mathematicans it is a slightly worrisome puzzle: is there really something there?–do we have to learn it? My suspicion is that, yes, there is something there–it is a language that is, for those who can use it without stuttering, a convenient tool in studies of compactness. But, if I am right, that's all it is: a language, not an idea, and a rather sharply focused one at that. Here is a somewhat unfair analogy: Dedekind cuts. It's unfair because it's even more narrowly focused, but perhaps it will suggest what I mean. No, we don't have to learn it (Dedekind cuts or non-standard analysis); it's a special tool, too special, and other tools can do everything it does. It's all a matter of taste.

Here a general question arises: is it not the case that one can better appreciate a translation if one is familiar also with the original text?

That non-standard models are not an idea is a rather quizzical remark. If they are a language, it is a curious language, one in which one can talk of things that cannot be mentioned in the mathematical language. Here is a problem for the Platonist: do non-standard models exist in the mathematical reality, and if so why not to investigate them?

But it is strange, and unbelievable, that he invites us not to study Dedkind's cut; he certainly has studied them, and he knows them. Does he think that the notion of a complete ordered field suffices for every question related to real numbers?

> As for the spectacular theorems–Gödel and Cohen–we admire them, but they haven't changed our work, our philosophy, our life. If someone should succeed in proving that the Riemann hypothesis is undecidable, we'd be shocked–as shocked as we were when the parallel postulate was proven undecidable–but we'd recover. We'd probably go on to discover and study non-Riemannian number theories, and live happily even after.

The abrupt shift from specialized concepts and techniques to the spectacular theorems is unfair: nobody expect that they change mathematics. Probably only the discovery of the incommensurability of the side and the diagonal of the square changed the working and the philosophy of mathematicians. Now we are wiser, and we know how to cope with such unforeseen events, possibly in the way indicated by Halmos (actually not the one chosen by set theorists). Nor logicians claim that all logical results are of comparable relevance.

> In the meantime, when the subject of the next colloquium speaker is announced as an application of the second order predicate calculus, we go to listen–politely, respectfully, but not eagerly, not hopefully. We don't think we are likely to learn something; at best we'll be entertained, at worst worried. When the logician says: "it's all the same, it's all mathematics", we feel insecure and humble–we don't know how to refute him–but, at the same time, we feel sure that what we are about to hear is not, cannot be the same as a talk on Brauer groups or Baire functions.

Halmos is honest to admit that "we feel insecure" ("humble" is less credible ...), but isn't insecurity the daughter of ignorance?

Next argument refers to the separateness of logic: as if the logician wouldn't want to know "the connection of his subject with other parts of mathematics". Halmos' very research experience in the field of logic contradicts such claim: he worked to extend the connection between propositional logic and Boolean algebras to that between predicate logic and a type of structures that had to be invented, polyadic algebras.

Both the logician and, say, the harmonic analyst, look for a certain kind of structure, but their kind of structures are psychologically different. The mathematician wants to know, must know, the connections of his subject with other parts of mathematics. The intuitive similarities between quotient groups and quotient spaces are, at the very least, valuable signposts, the constituents of "mathematical maturity", wisdom, and experience. A microscopic examination of such similarities might lead to category theory, a subject that is viewed by some with the same kind of suspicion as logic, but not to the same extent.

Ah, there's a clue: the microscopic examination. The logician's attention to the nuts and bolts of mathematics, to the symbols and words (0 and + and "or" and "and"), to their order ($\forall\exists$ or $\exists\forall$), and to their grouping (parentheses) can strike the mathematician as pettifogging–not wrong, precise enough, but a misdirected emphasis. Here is an analogy that may be fair: the logician's activity strikes the modern analyst the way epsilons and deltas might have struck Fourier.

It's been said that the biggest single step in the logic of the last 200 years was the precise explication of the concept of proof; as a tentative parallel let me advance the thesis that the biggest single step in the analysis of the last 200 years was the precise explication of the concept of continuity. But oh, no, says our hidebound mathematician; the two situations are not alike at all. Epsilons and deltas were a step forward, they made it possible to avoid many errors, and they opened new vistas–they allowed us to call many more functions continuous than we had dreamt before. But as for formal proofs, in the sense of certain admissible finite sequences of well-formed formulas–what, besides technicalities within proof theory itself, have they done for us? A clever graduate student could teach Fourier something new, but surely no one claims that he could teach Archimedes to reason better.

...

I didn't say all of this to convince anyone of anything–I said it in an attempt to describe the reaction, as I see it, of many mathematicians, myself included, to formal logic. It is not a

> quesation of right or wrong. It is a matter of native competence, professional upbringing, and mathematical taste; the fact is that, for whatever reason, a certain negative reaction does exist.

Thus ends Halmos' personal appraisal.

3 Fear of the languages

In the end we found Halmos giving voice to the most widespread misunderstandings we have recalled at the beginning.

The "microscopic examination": the old argument of the excessive, exclusive love for the *minutiae* comes to the fore. Halmos began his hate-love war with logic with symplifying expressions, and ends up with the idea that logic is concerned with parentheses. Is this consistent with the possibility, granted above, of being "worried" by some logical result? Could one be worried by parentheses?

Is, in the opposite direction, this concern consistent with the work Halmos did in logic in between? His work was inspired[11] by the theorem that the free Boolean algebra on the set $\mathcal{P}$ of generators *is* the propositional logic built on the set of letters $\mathcal{P}$. Such theorems, as those proved by Halmos for polyadic algebras, are called metatheorems, with respect, in this case, to the system of propositional logic, and belong to metalogic.

What mathematicians seem unable to grasp is the difference between (i) defining with precision the syntax of a language (which, besides, is not something one does in the introduction of every paper or at the beginning of every lecture), (ii) using it as a communication medium, which is done only in communicating with computers (the same papers and talks show that no such use is at work there) and (iii) assuming it as an object of study among other mathematical objects.

A precise inductive definition of the formulae of an algebraic language is necessary only if one wants to prove some metatheorem, say Łoś theorem, by induction on formulae; the presentation of the language will appear in the opening, then the investigation will be concerned with structures. Halmos would say that in such a case a piece of mathematics in done in a strange and unfamiliar language. Is this language so unfamiliar to mathematicians?

[11] He was "inspired by [the] desire to make honest algebra out of logic", Halmos, cit., p. 210.

It does not seem so: they are used since childhood to talk of polynomials. A polynomial may be thought of in many ways, for example as a function, but when considering rings of polynomials no other conception is available than that of terms and equations between terms, that is of formulae. Most of the applications of logic to algebra stem from a generalization of this experience to formulae other that equations.

But it is a curious historical phenomenon that in the passage from rethorical and syncopated algebra to symbolic algebra, with Raphael Bombelli, the symbolism is not considered any more a language.

Mathematicians never discuss, or even mention metamathematics, but in the course of cursory recapitulations of Hilbert's program. When Bourbaki mentions it[12], he calls it "an independent science of considerable interest, dedicated to the study of the mechanism of mathematical reasonings". "Independent" means here self-contained, but probably alludes also to its unrelatedness to mathematics, or to its sole proof-theoretic content. Abraham Robinson's metamathematics of algebra was then still unknown[13], but the judgement has lasted to these days.

When "the study of the mechanism of mathematical reasonings" is equated to proof-theory, the impression is strengthened that the study of logic aims at teaching Archimedes to reason better[14], a claim mathematicians proudly and disdainfully rightly reject. But "the precise explication of the concept of proof" is not a contribution to mathematics, the analogy with ϵ-δ is misleading. You cannot do analysis without a definition of "continuity"; you can do mathematics without a definition of "proof".

The definition of formal deductions (which is clearly what is being alluded to with "precise explication") as sequences (or trees) of formulae was a necessary step to construct abstract representations of theories and to have an object for the displaying of metalogical considerations. Following Hilbert, all mathematical logicians are well aware that formal systems could be "just

[12]In *Éléments d'histoire des mathématiques*, Hermannn, Paris, 1960.

[13]Robinson's *On the Metamathematics of Algebra* was published in 1951, but only his next book *Introduction to Model Theory and to the Metamathematics of Algebra*, North Holland, Amsterdam 1963, met with a wider audience. The purpose of the first was "to make a positive contribution to Algebra using the methods and results of Symbolic logic". In the Preface to the second Robinson could observe that some applications were now well-established, but not so "the suggestion that numerous important concepts of Algebra possess natural generalizations within the framework of the Theory of Models".

[14]Though this is not the aim of proof theory, but of some interested misrepresentations.

a flat photograph of some solid mathematics"[15] but the photograph is only a preliminary step in the study of the relations between picture and reality.

As to the biggest step of the last 200 years of logic, if it is possible to pick up one, I would vote for the completeness theorem, a mathematical (and philosophical) result with a lot of interesting mathematical consequences. It unifies for example the many varied phenomena of compactness, as Halmos is aware in the quotation above.

Summing up, the deep reason for the opposition, depreciation and misunderstandings concerning logic among mathematicians lies in their inability or unwillingness to accept the *binomium* language-metalanguage as a mathematical tool; they don't even seem capable of understanding its sense.

This could be due to their habit of talking in an informal quasi-natural language, where metalanguage is flattened on the language itself, or the languages are absorbed in the metalanguage, a habit legitimated and reinforced by the set-theoretical framework. They should know however, as everybody is now aware, that this very identification is the source of dangerous circularities. Only the conceptual distinction, at least in principle, of language and metalanguage avoids the paradoxes.

This notwithstanding, they seem to think that to make explicitly and effectively such a distinction is not worthy of mathematics. Not only they refute to make it, and to study it, they do not even accept a discussion of its possible merits. They do not consider for example how it could help mathematical education.

Could it be that they think, like God, that the plurality of languages is a curse, and that one language gives strength? But that, on the man's part, is a blasphemous conceit, and it runs counter to God's will.

[15]Halmos, cit., p. 208 actually says: "formal logic might be just a flat photograph of some solid mathematics".