# What is a ‘challenging problem’?

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## ‘Service to Humanity’

We are, most of us, involved in ‘solving problems’, most of our time. Some of them are ‘real’ or ‘really important’ (as they say), other ones are merely ‘academic’ (they – the other ones – would say).

A few years ago – sometime in the previous millenium –, one of my ‘polytechnical’ friends (an electrical engineer, expert in high power technologies and so on) managed to circumscribe *exactly* – to my mind – the first kind: she claimed she was, simply, ‘in the service of humanity’[1]. That’s ‘real’ and even ‘really important’ (think, e.g., of the poor state of the electrical networks in the States these days). The other kind is equally easy to exemplify: take, for instance, the problem Professor Andrew Wiles (well, along with plenty of others, in the end) was confronted with. since still a teenager: the Fermat Conjecture (now a ‘Theorem’). This one is, definitely ‘real’ – it’s a ‘real problem’, one might say –, and probably even ‘important’ in some sense; yet significantly less ‘serviceable to humanity’, I suspect. We can certainly survive without Fermat (and, even, Professor Wiles[2]) for a long while – a millenium or more, from now on. It is, nonetheless, less obvious that we can survive that long – by our current standards and habits – without people like my power-planting friend.

One might object to the previous example to the effect that I’m confusing things like ‘applied’ and ‘pure’ science (maths[3], in particular). Because even though both my IEEE-friend as well as Professor Wiles are actually ‘using’ highly sophisticated maths, the maths involved are of a different ‘kind’, in each case: the latter is just ‘pure science’, while the IEEE-people are dealing in ‘applied science’, as they use to call it.

[1] This because she was mainly involved in designing / fixing power plants... and / or doing ‘service’ for the IEEE-community, in her free time.

[2] Sorry, Professor: a sad fact of life, alas!

[3] Sic: the plural is British (like in Ancient Greek)! Other people use to think that *mathematic* (sic) is one!

Wrong idea! How do we know Fermat-Wiles won't eventually yield plenty of so-called 'applications', within a century or, even, less? Thereby becoming equally 'serviceable' to the humanity next to come (as, e.g., to our grand-sons and grand-daughters, their sons and daughters, and so on).

Look at the recent whereabouts of the Galois theory: from a purely 'academic' business, this piece of 'pure' algebraic wisdom has become nearly unavoidable in cryptography, nowadays. And don't tell me cryptography is no good 'for humanity', or else that it's only good for spies and secretive persons or governments. After all, cryptographers – highly theoretical folk, Alan Turing included – contributed significantly to the end of the Second World War. And even wars are human affairs, you know? Unfortunately.

"Wait!", my would-be opponent would likely exclaim. "You're awfully rhetorical, Sir! This kind of argument destroys already your previous *distinguo* between IEEE-science – as you've put it, in the footsteps of your electrical friend – *versus* Fermat-Wiles."

Not at all! Certainly, my *distinguo* is 'timed' and, above all, 'empirical', so-to-speak. After all, humanity has survived even without IEEE-engineers, for a long while. Past a certain point in time (called also 'human history'), it won't be the case any more, I suspect. But this doesn't mean the IEEE-people are dealing in 'applied' matters alone, while Professor Wiles – and, possibly, a couple of his friends and fans – are dealing in (mere) 'pure' matters. Both 'kinds' of science (ultimately: maths) are based on theoretical endeavours, on 'contemplation', so-to-speak. And, they're also both based on an 'appetite of knowledge', as old Aristotle would have had it (cf. *Metaph.*, at the very beginning). Usually, I / we just wonder 'why' [such and such is the case]! Sometimes my / our stubborn questioning – '*mirare*' in Romanian: it's easy to find out the Latin therein – would end up in something 'serviceable' for the rest of us, and sometimes not.

Roughly speaking, there is no real point in distinguishing between 'pure' and 'applied' science (maths, in particular). It's a matter of time, not one of... essence.

Ultimately, there is no 'applied science' (and no 'applied mathematics', for that matter).

Alternatively, in order to avoid unintended – otherwise boring – professional conflicts ('my guild in better than yours!', and the like), I might be able to refine the difference suggested above in terms of a single discipline, the calculus, in maths (also called 'mathematical analysis', in old Europe).

Suppose we are confronted – for some reason – with a huge amount of calculations, involving a high maths background. A talented – perhaps even an average – mathematician (expert in 'mathematical analysis', say) would eventually solve our problem(s), pending time and / or manpower, by using extant / well-known computing methods and recipes. Confronted with the same set of problems, her neighbour next-door – an expert in TCS (you can even forget about the T [of 'theoretical'], here) – would do something else: she will first re-visit the 'theory' thoroughly, rephrase the 'task' in her own (guild's) terms, extract an algorithm for the task and then 'implement' it, as a piece of software. Ultimately, she'd end up with similar solutions to our original problem(s), except perhaps for the fact the latter are going to be – awfully – more efficient: in the latter case it might takes minutes / hours / weeks to get a solution, instead of days / months / years. Now we are speaking about two 'applied' scientists (and both of them mathematicians, in the end). Yet, if 'time' is at premium (as it is usually, nowadays), only the latter would be really 'serviceable to humanity' (at least to a bit of it). For the other one(s) would be always 'too late' in this respect.

## Wonder and Challenge

"Perhaps a bit too... Baconian (a reference to our the famous Lord High Chancellor of Great Britain), your power-planting friend!", my would-be opponent would, once more, object. "For not every kind of science is ultimately 'power', even if 'serviceable to humanity'. Even though there is no 'applied science', ultimately, as you *timely* claimed, you are still confusing the 'science' itself with its specific uses, now and then. As a matter of fact, the recent (2000) Knight Commander of the Order of the British Empire – as well as plenty of string theorists, say, to change the running examples for a while – won't really care about being or becoming 'serviceable', anyway. They're just... wondering, like those venerable Greeks you already mentioned would have said. There is always a kind of *challenge*, involved. But there is still no 'service' intended in actual problem-solving. And, for sure, no 'humanity welfare', or anything like that, among the reasons a scientist may have to 'do science'. I'm usually becoming a 'scientist' just because I'm curious – or only *neugierig*, in your German –, there is an *appetite for knowledge*, to begin with, indeed."

Right! This is, is fact, my next point. – I'd rather dissent, however, on reasons and motivations.

There are at least two distinct ways of 'wondering about'. 'I wonder why', meaning, more or less, 'how' for 'why'. This is a typical starting point for a becoming / future scientist. If you 'wonder' about the right kind of things, you may end up eventually with a Nobel prize (or a Fields medal, for that matter), because you're ultimately 'serviceable' to the rest of us. On the other hand, you'd never get such a high and explicit ('human'!) recognition if you'd wonder about 'being and beings', for instance (like Leibniz, long ago, or, more recently, Martin Heidegger). And only the latter kind of questioners won't really care about 'serviceability', to humanity and / or whatever else. The former kind would, at least implicitly.

In other words, while questioning *en philosophe*, we don't 'really' care about the world itself, humanity included (you can even write it with a capital H, if you like: it would amount to the same thing). So the philosopher can – and will – ask his / her kind of question(s) forever. A ('real') scientist won't even recognize such 'questions' – the 'philosophical' ones – as ('real') questions.

Because a ('real') scientific question is one for which we can – sooner or later – get (converging, and thus... 'serviceable'!) answers.

There is no (converging) point in wondering – again and once more, after ages – *à la* Leibniz: 'Why is there Being rather than Nothing?'. (*Pace* Herr Professor Heidegger, scientists won't bother about Nothing, anyway.)

This doesn't mean all 'philosophical' (or 'philosophically looking') questions are 'un-scientific' (or just 'pseudo-problems', as one of our former friends once claimed). We might usually ask 'wrong' – even 'stupid' – questions now and then ('awfully philosophical', at a first look). The next generation(s) – one or more – would 'repeat' such questions, in slightly different terms, by refining them – by 'getting to the point', more or less, so that would-be answers would come, eventually, 'in sight'.

Whence a kind of (meta-) question I intended to ask from the very beginning (sorry for the long détour: I was unable to put things in a simpler way):

'What about the *motive ground* of a (*real*) question? What's *behind* a question that makes me try, again and again, to get an answer (to it)?'

It's an offending state of affairs, I'd say. The French would likely call it a *défi*, something to be taken, more or less, *personally*. This comes about to the English 'challenge', more or less. (Britons and most – English speaking – descendants thereof would rather have it as a kind of club – sport, say – affair.)

Apparently, there is no generic, no 'metaphysical' (meta-) answer to this. Because, simply, why is not every question 'challenging' to me? Or else 'equally challenging', indeed? Subjective conditions, knowledge / background, all this is immaterial, here: I can modify my state of mind, I can also learn, etc. etc. –

Well, if you'd ask me about a specific something, there might be no 'challenge' in your question / problem. Because – simply – I already know the answer: so – while answering – I'm eventually acting as a mere tutor, as a teacher or so. No funny business, after all! You can also use a book, instead, with the same effect, if not even with more profit.

If, however, I don't know the answer to your question / problem in advance (and, moreover, if I'm aware of the fact nobody else does), your question / problem would certainly look 'challenging' to me.

Still, there are different kinds of 'challenge'. Planning to go into this next.

Yet, before addressing the *difference*, here is a good place – as any other, perhaps – to add a parenthesis.

## A Bad (Meta-) Dream and the Drunk Little Monads

We can first think of 'science' as of a vast repository of data, a kind of encyclopedia, let's say.

If the answer to your question / problem is not *out there*, in the Universal Data Base, then your question / problem looks 'challenging' to me / to [all of] us: worth taking the trouble to look into it.

This is a rather superficial view on 'science', though.

First, 'real science' is highly 'regionalised': this would amount to many, apparently unrelated 'encyclopedias' instead.

There are 'sciences' not a single 'science', indeed: the 'science itself' looks rather like a maze of un-communicating containers, not like a unique Holy Grail.

On the book-metaphor, we are only confronted with disparate chapters in a novel written by an absent-minded author – at different times –, inspired by very different, unrelated 'real-life' episodes.

On a more 'philosophical' metaphor, the true picture of 'real science' ressembles to a world *à la* Leibniz, one without a pre-established harmony, though: a collection of un-communicating monads, with no Over-All Monad to integrate – and to harmonise – little poor monads – the folk below – from the above.

The older neo-positivists' (meta-) dream of a would-be 'Unity of Science' is, at best, a reductive idea, one based – more or less – on the model of a single God-blessed 'discipline': the Theoretical Physics. This was a piece of wishful (meta-) thinking, in the end. Because we are actually dreamers of many Holy Grails, as many as the historically attested 'scientific disciplines' (if not even more, since – as we go – we are always tempted to invent new 'disciplines' from scratch, from trifles, or from something like that).

Second, even if granted the (multi-) disciplinary perspective, the would-be – intended – coherence of a single scientific 'discipline' is not always automatically granted: the 'real' little poor monads are often skyzophreniac entities, or else the monads actually populating our 'scientific' world would look like drunk people, *derilium tremendes*, rather than lucid, sober, and thus respectable folk. And this picture reflects – unfortunately – the rule of the game, not mere exceptions.

In spite of all appearences, even the Paradigmatic Queen so beloved by the earlier neo-positivists – Theoretical Physics – is nothing but a fat, highly delirious, old lady, nowadays (look at those many, highly imaginative 'string theorists', and to their [highly] 'theoretical' zizanies!).

With a popular joke (stolen from the world of mathematics, more or less), the only 'unifying principle' in contemporary physics seems to be the label 'Theoretical Physics' displayed on the wall of the physics departments and / or institutes, 'round the world. Just like that![4]

On the drunkard metaphor, the 'real', immediate / urgent challenge would consist of making people sober, first.

Looking for (a lost) coherence: a very different kind of problem-solving, indeed! Rather – if compared with usual: punctual / local problem-solving – a would-be meta-business, so-to-speak.[5]

## 'System', the Kantian Dream, the Actual Theory-Building, and the Perpetual *Zizanie*

The ideal situation for a specific 'scientific discipline' is best described by an old Kantian dream: the *Normative Idea*.

[4]The original maths joke amounts to the observation that the fictitious French mathematician Nicolas Bourbaki has invented a new French singular noun, from a French noun defective of singular, *mathématiques*. As a matter of fact, the French plural comes from an Old Greek [plural] phrase: there was no *mathématique* in Old Greek, either: *ta mathēmatika* were simply 'the mathematical disciplines', even for Plato and Aristotle – a venerable example of skyzophrenia, in this kind of plural(s), we must agree.

[5]This has nothing to do with 'metaphysics', 'meta-physics', and the like, by the way.

In 'real life', this points out to something like the 'systematic character' of a (particular piece of) science: in a given 'discipline', things must 'hang together' first: there is no 'science' otherwise.

Whence, the meta-business alluded to above, a normative idea ('look for coherence first!') should come *before* the actual (punctual / local) business of problem-solving. If our 'science' ('scientific discipline') consists a mere *bric-à-brac* collection of conflicting data and statements, there is little chance we can ever solve local / punctual problems in this area; the said 'science' is, in fact, useless.

*Aside*. There is a famous general theorem on computation, in Lambda-Calculus (due to the father of the business, Alonzo Church, and to one of his students, J. Barkley Rosser), the so-called *Confluence Theorem* (also known as the *Church-Rosser Theorem*; CR, for short). Roughly speaking, the CR Theorem says that the result of any particular computation [$3+(2\times3)+(5\times2)$, say], in a reasonably 'coherent' computation system, must not depend of the order of performing smaller computations steps. So, it would be reasonable to ask from a specific computation system to satisfy this kind of (minimal) requirement [here: *confluence*, in technical terms]. Indeed, imagine the disaster, otherwise: a kind of algebra / arithmetic where $3+2+1$ would lead to different results, according to the path of computing you might choose: 'do first $3+2$' or 'do first $2+1$'. – This is mainly to stress the fact that a 'coherence' requirement might not be a mere ['traditional'] logic requirement, where coherence is taken to mean logical non-contradiction or consistency.

The meta-business above – characterized by the (more or less Kantian 'normative') recommendation / requirement: 'look for coherence first!' – has been always among the main concerns of 'ordinary' scientists; so obvious that it was not even worth mentioning as a separate 'scientific' task. Whatever the philosophical ancestry (Aristotelian, Galilean, Cartesian or so), it is only in recent times that 'professional' philosophers managed to 'take over', and grab the subject for their own. The modern catchword for this kind of [meta-] concern is 'epistemology'.[6]

In ordinary scientific practice, the 'normative' requirement, mentioned above, would amount to the fact that any 'scientific discipline' should have a 'systematic character'; specifically, to the fact that any 'particular science' should be presented / organized as a 'deductive theory'.

Initially, the implied – more or less managerial – task looked like a paradigmatic business for ('professional') logicians. In a longer run, in spite of an endemic – oft much too loud – agitation (around the early thirties till the late forties), in both Europe and overseas, it quickly turned out the logicians 'proper' have actually little – or nothing – to say about the subject itself. In retrospect, the extant contributors to the so-called 'logic of science' have done more damage than otherwise.[7]

[6] In plain English: 'theory of science'. To be fair, there is no 'theory of science' so far, there are just some – otherwise rather smart – people around, thinking 'theoretically' about such things and trifles.

[7] Honestly speaking, 'logicians' are *not* and *should not* be concerned with such trifles! 'From

In the meantime (during the last half a century, say), we managed to learn / acknowledge a few more things, nevertheless.

If the main (meta-) business in 'science' – specific 'disciplines', I mean – is the so-called Theory-Building, then we must take for granted the actual state of affairs (and, possibly, remember that this was *always* the 'current' state of affairs in 'science', along the ages, at any specific point in time), namely, the fact that there is no 'normal science' (to use a recent catchword), except for a while or two (!): the (historical) rule of the game is, rather, the 'theoretical conflict': a nasty thing to cope with, indeed!

In other words: 'true / real science' is, in fact, a Perpetual Quarrel among Dis-Agreeing Factions – it's actual locus is a Battle-Field, down-here, rather than a Serene Privileged Room, high-there, in the Ivory Tour.

## Epistemic *vs* Technical Challenge

Coming back to 'challenge(s)', my concern in the title. –

There are plenty of things to say about.

Let me first display a few examples of 'challenging' questions / problems in my sense (in logic, my preferred spare-time, for a rather long while).

Rather elementary, I'd say (the required background can be reviewed / explained in a few minutes – and /or a few lines in print).

[*A conceptual*] *Aside*. If I remember well, Henk Barendregt pointed out, in conversation, already thirty years ago or so, to the fact that my colloquial uses of the (epistemic) qualifier 'elementary' (as applied to maths problems, mainly) might [have] look[ed] oft [those times] rather loose and even confused. The point concerned standard – yet careless – ways of of speaking in maths: PA (Peano Arithmetic) was / is still usually referred to as 'elementary arithmetic'. A very wrong idea, if you really care about 'complexity' matters and things like that! So, I'd rather think twice or more – since –, before referring to a specific PA-question as being 'elementary'. A bit later, I had the opportunity to dissert at length about 'colloquial' proof-qualifiers like 'obvious', 'easy', 'as ever' etc. [in the introduction to *Abstract* AUTOMATH, 1983, kind of a digest, recording endless conversations with Nicolas G. de Bruijn, in Eindhoven, on *proving in mathematics* and so on], and even managed, next, to put together a little private theory of the would-be 'degrees of elementarity' in mathematical practice. (Whence also my disappointment as regards the current talk about 'complexity' in logic, maths, TCS, and the like.) – Here, I'd rather avoid such subtleties, however. In particular, I'm going to use the qualifier 'elementary' only as regards the way of formulating / expressing a particular problem /

a logical point of view', if 'physics' – or 'mathematics', for that matter: set-theoretically re-shaped as **ZFC**, say – would turn out to be 'inconsistent', then much worse for the 'physics' – and / or the **ZFC**-based 'mathematics'! Why bother? For – remember, please! – these are... worldly affairs logicians are not and should not be concerned with. – The fact is that recent, serious logicians would typically avoid the subject, nowadays, half a century – or so – later, the associated message being, roughly said: 'mind your own business, folks! we don't really care about yours!', etc.

statement, *not* as a qualifier of the would-be methods / techniques etc. that might be needed in order to solve a problem and / or to prove a particular statement.

[*1: The Belnap story*] Suppose you would have asked me, some thirty five or forty-so years ago ('round 1975 thus), to prove the following little puzzle (in 'propositional logic'):

> Let [**B**], [**CB**], [**I**] be formulas as below, and $\mathcal{BB}$' be a 'propositional logic' with implication [→] only, axiomatized by [**B**], [**CB**] and *modus ponens* for → (substitution rule tacitly assumed).
>
> [ **B** ] (p→q) → ((r→p) → (r→q)) – [prefixing]
>
> [ **CB** ] (p→q) → ((q→r) → (p→r)) – [suffixing]
>
> [ **I** ] p→p – [identity]
>
> [ mp ] A→B, A ⇒ B – *modus ponens*, detachment for →.
>
> Show that no substitution instance of identity [**I**] is derivable in $\mathcal{BB}$' (*idest*: there is no proof of [**I**] from the [**B**] + [**CB**]-axioms, with *modus ponens* and substitution). –

This is a rather 'elementary' problem in (propositional) logic. Easy to state for everybody, no 'advanced' knowledge required to grasp its meaning, etc.

Yet, 'challenging', indeed! Thirty five or forty years ago, I won't have had the slightest idea on how to prove the claim.[8]

I'd classify this querry as an 'epistemic challenge' (it is, more or less, clear why).

The specific point, here, is in the fact that the problem can be solved in different ways, and at different 'levels of abstraction', so-to-speak.

This being said, I won't mention the [associated] bibliography, anyway.[9]

[*2: The Tarski story*] Suppose somebody [else] would have asked you some 85 – resp. 91 – years ago (i.e., in 1925) to prove the following equally puzzling (meta-) statement:

> Let [**K**], [**D**] be (purely implicational) formulas, in a propositional language as above (→ stands for implication).
>
> [**K**] p→(q→p) – irrelevance
>
> [**D**] p→(q→((p→(q→r))→r)) – pairing

[8]Historically, 'the Belnap conjecture' – now a 'theorem', due to Nuel Belnap Jr – stated above remained, actually, a very stubborn open problem for more than fifteen years. At the time of writing there are three distinct ways of showing this. In each case, the techniques therein involved are surprisingly 'advanced', in maths terms. No connection whatsoever with the popular wisdom on 'propositional logics' you may eventually learn about, in a first year logic-course, in maths and / or philosophy departments, etc.

[9]Just try the problem 'by hand', without cheating (looking on the web etc.): if you end up with an answer in less than a year, say (!), then you are a smart mathematician, likely (although – to be fair – you won't thereby contribute a bit to the welfare of humanity, alas).

> Let $\mathcal{L}$ be a finitely-axiomatizable propositional logic (i.e., a logic with finitely many axioms) and *modus ponens* for $\to$ (substitution tacitly used, as ever). Show that $\mathcal{L}$ is axiomatizable with a single axiom (*modus ponens*, and substitution), if both [**K**] and [**D**] are derivable in $\mathcal{L}$.

Historically, this statement was claimed in 1925, without proof, by Alfred Tarski. The Pole is, among other things, the founder of modern Model Theory, a branch of [contemporary] Mathematical Logic, by the way. Parenthetically, the first proof of Tarski's claim was obtained by the undersigned, in 1979 [published 1982], using a technique involving concepts *not available* in 1925 [$\lambda$-calculus: invented around 1931–1932, in the United States, by Alonzo Church].

*Aside*. A meta-puzzle: how did Alfred Tarski manage to get such a result, in 1925? The meta-puzzle points out to a famous, and very similar, in fact, 'epistemic' situation, namely, to the case of Fermat's claim. If Fermat did really have a proof of the 'Fermat theorem', his proof was, definitely, not like the one Andrew Wiles was able to produce a few centuries later. Wiles' proof won't fit the 'margin' of that famous Greek book, either; but the real point is in the fact that it won't fit the overall epistemic context, anyway. Because Professor Wiles was inheriting of a very different kind of mathematical wisdom. Pierre Fermat couldn't have ever dreamed about elliptic curves! At least not in this context. In the end, if we are to be 'really fair', Andrew Wiles has *not* actually solved the *original* – 'historical' – problem; namely: 'prove the Fermat Theorem with the conceptual means of his time'! –

On the other hand, my own 'meta-conjecture' would consist of saying that Tarski anticipated – somehow – '$\lambda$-calculus', 'combinatory logic' and the like. Because there is no other – reasonable – explanation: we cannot prove Tarski's claim by essentially 'other' means![10]

This is also an 'epistemic challenge', like the one under [1] above.

Specific point, here: very likely, this problem can be solved in only one way (mine!) and Alfred Tarski was apparently aware of it, *avant la lettre*. (How?)

No bibliography, again.[11]

[*3: The Wos story*] Suppose now you are a member of the relatively new [scientific] AAR-community (the *Association of Automated Reasoning*) – so you are already familiar with various AR (Automated Reasoning) software packages (OTTER, etc.) and / or techniques of [soft-] proof –, and the current President of the AAR (actually, it's Larry Wos, these days[12]) would have displayed – at a recent AAR-meeting – a rather long formula – [**R**] say – containing circa hundred symbols or so, claiming [**R**] is axiomatizing classical propositional logic CL, with *modus ponens* (for 'material' implication) alone. The Wos Question / Problem is: show this using AR software (*idest*: prove [**R**] from a particular axiom system for CL, usig OTTER, for instance).

[10] Well, all this would leave Fermat as mysterious and as cryptic as he has been so far, I'm afraid.

[11] If you manage to do it 'by hand', without ever mentioning $\lambda$-calculus, combinators and the like, then you're certainly at least as smart as Alfred Tarski!

[12] These notes were written in September-October 2010.

Rather tricky, I would say, if you are not already familiar with the Tarski-story, already mentioned above, under [2].

Here, formula [**R**] (they call it a 'Rezuş formula' nowadays, although it should have been 'Tarski formula', as a matter of fact) can remain anonymous (you can easily obtain explicit examples thereof from the solution to the previous problem, anyway).

The 'Wos Question / Problem' is, however, not of the same type as under [1] and [2].

This question / problem is, rather, an example of a 'technical challenge'. Because associated with a specific 'technical' constraint (here: 'use AR software').[13]

*Acknowledgements.* I owe the last *distinguo* above to Branden Fitelson (now [2016], at Northeastern University, Boston, MA), who pointed out the fact that (generalizing a bit), actual conditions / constraints occurring in our current problem-solving endeavours may not be always 'purely epistemic', but rather – and typically so, in 'real life' – of the kind claimed by my power-planting friend, the IEEE-engineer, a while ago. Moreover, such ('real-life') constraints would usually make things less obvious than they might appear at a first look, *in abstracto* ('laboratory conditions' and so on). — The latter *distinguo* – epistemic *vs* technical challenge – deserves further reflection. Putting things in slightly different terms: 'does *common science* need... translation in order to become *serviceable*?' For, if so, this would involve a different kind of knowledge, apparently!

Thanks are also due to John Halleck (University of Utah, Salt Lake City, UT) for making comments, similar to those of Branden Fitelson – including a huge amount of technicalities on so-called 'Rezuş formulas' and the like.

> "I think what we have here is a difference of cultures, and cultural context. The context in which Dr. Wos is publishing is people working on automated therorem proving programs, trying to make them solve a wider class of theorems. Dr. Wos is himself famous for 'pushing the envelope' and developing techniques to do just that. [...] At the current 'state of the art', there are huge areas where there are human proofs, and automated theorem proving does very badly and either can not solve them at all, or only some programs can. His [Wos'] challenges are not intended to be particularly challenging to people, and certainly not to any half-way educated person with access to the literature. What they ARE intended to be is something that is very difficult for PROGRAMS to do, and are areas where research in methods is sorely needed. And the Rezuş axiom for **BCIW**, while never intended for human display, is perfect as a test of an area that (as he pointed out) computer programs are very weak." [JH 20100811]

[13] Actually, at the meeting of the AAR, in June 2010, Larry Wos gave a different, more complex, example, based on the same Tarski-story as above. I confess I didn't first see the 'challenge', whereupon I certainly owe him an apology!

Nicely said! Although I'd have rather mentioned a 'diference of *mathematical* culture(s)', instead!

*Postscript* [20101003]. Apparently based on a previous approach due to Dolph Ulrich (2004–2005), John Halleck has provided, a few days ago [20100923], a genuine solution to the `challenging puzzles' [2] and [3] above. Actually, Halleck did not prove [2] 'by hand', the old-fashioned way; he used specific software instead, enabling him to produce derivations very close to the Łukasiewicz style derivations (*modus ponens cum* substitution) of the late twenties and early thirties. For a short $\lambda$-calculus version of Halleck's proof, see the *Addendum* to my note *Tarski's Claim: Thirty Years Later* [October 1, 2010].

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