THE FUTURE OF SET THEORY

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Abstract. Judah has asked me to speak on the future of set theory, so, as the next millennium is coming, to speak on set theory in the next millennium. But we soon cut this down to set theory in the next century. Later on I thought I had better cut it down to dealing with the next decade, but I suspect I will speak on what I hope to try to prove next year, or worse – what I have done in the last year (or twenty). It seems I am not particularly suitable for such a lecture, as I have repeatedly preferred to try to prove another theorem rather than to prepare the lecture (or article); so why did I agree at all to such a doubtful endeavor? Well, under the hypothesis that I had some moral obligation to help Haim in the conference (and the proceedings) and you should not let a friend down, had I been given the choice to help with organizing the dormitories, writing a lengthy well written expository paper or risking making a fool of myself in such a lecture, I definitely prefer the last.

We shall now try to discuss some relevant axes of interest – so ideally, for each such axis, the people in the area are divided in a meaningful way (the number of exclamation marks reflect my view of how much this motivates my own work).

AXIS A: **Source of interest**

1. foundations/applications to philosophy	!
2. applications to mathematics	!!!
3. historical reasons	!!!
4. inner developments	!!!!
5. beauty	!!!!!!!!!
6. proof with "bones" or at least "meat"	!!!!!
7. generality	!!!!!!
8. sport (*added by popular demand*)	!!!

We can use this also to evaluate and to classify existing research and researchers and as seen below, the differences are ones of emphasis.

To a large extent I was attracted first to mathematics and, subsequently, to mathematical logic by their generality, anticipating that this is the normal attitude; it seems I was mistaken. I have always felt that examples usually just confuse you (though not always), having always specific properties that are traps, as they do not hold in general. Note that by "generality" I mean I prefer, e.g., to look at general complete first order theories (possibly uncountable) rather than at simple groups of finite Morley rank. However, I do not believe in "never look at the points, always look at the arrows"; each problem has to be dealt with according to its peculiarities,

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and finding applications of your own field in another means showing something that interests the people in the other fields; but given a problem, why not try for the best, most general statement available? (Of course, if the theorem exists, and the additional generality requires no substance, it is not exciting.)

From another angle, I was amazed to find that many of my colleagues, including some of the best minds in the field of set theory, feel apologetic about their subject. Many are apologetic toward mathematicians (implying somehow that there are mathematicians and there are logicians, as if they are disjoint species) working in fields which are surely *deeper, harder, more profound and meaningful*, etc., and so feel that we have to justify our existence by finding applications of “logic” to “mathematics”. This leads to setting great store by category A.2, as in the Abraham Robinson school. Now, I love to prove theorems in as many areas of mathematics as I can, but I do not like this servile attitude. (This says nothing concerning the attitude of, say, a number theorist toward this.)

Many others set great store by the role of foundations and philosophy. Again, I do not have any objection to those issues per se, but I am suspicious. My feeling may be akin to that of many authors who, while acknowledging the rôle of literary critics in cultural life, think that heeding their dicta will lead to boring works – but naturally believe that their own works, of course, will shine forever because of their intrinsic beauty.

Still others mourn the loss of the “good old days” when the proofs were with ideas and were not so technical. In general, I am not a great fan of the “good old days” when they treated your teeth with no local anesthesia, and the term *technical* is a red flag for me, as it is many times used not for the routine business of implementing ideas but for the parts, ideas and all, which are just hard to understand and many times contain the main novelties.

My feeling, in an overstated form, is that beauty is for eternity, while philosophical value follows fashion.

I feel these complaints cancel each other nicely; e.g., the third tells the first that mathematical logic is much more mathematics now than earlier, and the second implies that it deals with worthwhile things. By the way, those attitudes are not contradictory in practice – as many will support two or even three of them.

As for beauty, I mean the beauty in a structure in which definitions, theorems and proofs have their part in the harmony; but complicated proofs do not bother me. As an undergraduate, I found Galois theory beautiful (more exactly, what is in the book of Birkhoff-Maclane), and later I found Morley’s theorem (with its proof) beautiful.

A disgusted reader may shout: “Beauty? You find in your mess some trace of beauty?” I can only say that I hear the music of those spheres or that every one likes his own dirt (the difference is small). [ABA hhevarah haAxronah Aulay keday lehajmif me “or that every”]

AXIS B : **The Framework**

1. ZFC	!!!!!!!
2. forcing	!!!!
3. inner models	!!!
4. large cardinals	!!!
5. ZF+DC+ some form of determinacy	!

This is a reasonable division, but there is interaction and in any case, all of us are actually proving theorems in ZFC.

From the point of view of adherents of ZFC (B.1) (and I tend to agree to a large extent) proving a theorem means proving it in ZFC, and the other attitudes are supplementary; forcing is necessary to tell us when we cannot prove a theorem, large cardinals are needed in some consistency proofs, and – by a happy coincidence – they are ordered on a linear scale. Finally, inner models are used to show that large cardinals are necessary and, even better, to get equiconsistency results.

My feeling is that ZFC exhausts our intuition except for things like consistency statements, so a proof means a proof in ZFC. This is of course a strong justification for position B.1.

Position B.2 in its strong form tell us, in essence, that all universes are equally valid, and hence we should, in fact, be interested in extreme universes. In particular, L has no special status, and proving a theorem in ZFC or assuming GCH is not a big deal. This is the strong defense, but I suspect that it has few adherents in this sense.

But in the moderate sense, this position is quite complementary to the ZFC position: One approach gives the negative results for the other, so being really interested in one forces you to have some interest in the other. In fact, I have been forced to really deal with forcing ([Sh 64]; [BD] was too "soft" in forcing for my taste) because I wanted to prove that I was right to use $\diamondsuit$ on "every stationary subset of $\aleph_1$" in solving the Whitehead problem for abelian groups of cardinality $\aleph_1$, as CH was insufficient.

J. Stern has "accused" me of having explained to him in detail why proofs in ZFC are best, and why I prefer them to independence results, just two years before I launched full scale into forcing. I still feel an outright answer in ZFC is best, even though a new technique for proving independence may be more *interesting*. Cohen's theorem seem to me more interesting than a proof of CH would have been, as it supplies us with a general method.

If you are serious about preferring ZFC, you should set great store by the follwing: ISSUE: *Much better to Carry out constructions in ZFC.*

We know now that it is much easier to carry out constructions if $\mathbf{V} = L$. For some purposes it may be argued that this is not so bad: If you want to show a certain theorem cannot be proved, doing so in one universe suffices. For example, it was proved in [GuSh 151] that in the monadic theory of linear order you can interpret second order logic, under the assumption that for a proper class of cardinals $\lambda = \lambda^{<\lambda}$. Now this is a very weak assumption. How significant is it to eliminate this? I have worked considerably on such problems (see [Sh 300, III], [Sh e] and [Sh 284b]). (Of course, when one is unable to carry out a construction in ZFC it is significant to be able to carry it out in *some* universe).

Earlier, we could consider adopting GCH as an axiom, especially before Cohen, especially when it seems we cannot say anything (non trivial) without it; so not so much *belief* in GCH but the desire to prove theorems drove people in this direction. I do not think this is considered seriously now.

From time to time people argue we should "believe" or "adopt" as an axiom the statement "$\mathbf{V} = L$"; my own inclination is strongly against this. This universe looks like a very special thin and uncharacteristic case, and adopting it would kill many interesting theorems; we shall return to this issue below. In any case, I do not think anybody takes it seriously. In spite of some rumors to the contrary, Jensen flatly does not "believe" in $\mathbf{V} = L$ (though it would certainly be to his personal advantage) but he thinks a proof under $\mathbf{V} = L$ is significantly better than a consistency result.[1] I agree, but how does this compare to a proof under MA? or from no sharp?? or large cardinals??? Maybe the following table will tell us something. (The numbers measure the value of the result on a scale from 0 to 100, and are based on my impression.)

	Jensen	Magidor	myself
consistency	40	40	30
from $V = L$	65	50	35
from large cardinals	50	60	40
from ZFC	100	100	100

I think the investigations of $\mathbf{L}$ are also a fine source of inspiration for work in ZFC, being an extreme case (the second position), as in the case of diamonds and squares (e.g. proving diamonds from cases of the GCH).

But learning the covering lemma, I thought it would be wonderful to

- inset prove a combinatorial theorem (see below in Axis C) by a dichotomy according to whether $0^{\#}$ exists (or another such dichotomy).

This was suggested in [Sh 71] and carried out in [Sh 111] (see also [ShSt 419]), but this has not been particularly influential so far. It constitutes a high hope for the theory of inner models from the point of view of B.1. But naturally, Jensen hope, as I have learned lately, is naturally a much higher:

•• inset find some inner model whose sharp does not exist, so from it we can "exhaust set theory" and really understand everything in two steps – analyze the inner model and then reduce the true set theory to it;

Those looks wonderful; but I do not believe in it.

From the large cardinal point of view: the statements of their existence are semi-axioms, (for extremists – axioms). Adherents will probably say: looking at how the cumulative hierarchy is formed it is silly to stop at stage ω after having all the hereditarily finite sets, nor have we stopped with Zermelo set theory, having all ordinals up to $\aleph_\omega$, so why should we stop at the first inaccessible, the first Mahlo, the first weakly compact, or the first of many measurables? We are continuing the search for the true axioms, which have a strong influence on sets below (even on reals) and they are plausible, semi-axioms at least.

A very interesting phenomenon, attesting to the naturalness of these axioms, is their being linearly ordered (i.e., of course you may find artificial cases were linearly fails but it holds among those which arise naturally), though we get them from various combinatorial principles many of which imitate $\aleph_0$, and from consistency of various "small" statements. It seems that all "natural" statements are equiconsistent with some large cardinal in this scale; all of this proves their naturalness.

This raises the question:

ISSUE: Is there some theorem explaining this, or is our vision just more uniform than we realize?

Intuition tells me that the power set and replacement axioms hold, as well as choice (except in artificial universes), whereas it does not tell me much on the existence of inaccessibles. According to my experience, people sophisticated about mathematics with no knowledge of set theory will accept ZFC when it is pre-

[1]this was not accurate, he preferred results in subcolored definite universy ??? so $\mathbf{V} = \mathbf{L}$ is ideal but also core?? models

sented informally (and well), including choice but not large cardinals. You can use collections of families of sets of functions from the complex field to itself, taking nonemptiness of cartesian products for granted and nobody will notice, nor would an ω-fold iteration of the operation of forming the power set disturb anybody. So the existence of a large cardinal is a very natural statement (and an interesting one) and theorems on large cardinals are very interesting as implications, not as theorems (whereas proving you can use less than ZFC does not seem to me very interesting). Still, the arguments above are strong enough for me to put them higher than inner models and recognize them for consistency proofs, per se, and also as compared with statements from the AD circle of ideas; the comparison of consistency from large cardinal assumption and via AD statements arises (for me), in the context of a large ideal on ω_1. Is a proof of consistency from the consistency of "ZFC+ super compact" a solution, and lowering the consistency strength nice, but not a real change? I tend to say yes to this. And what about starting with "ZF+DC+AD+θ regular"? For me it is an implication, Woodin's view is more or less the inverse. Since my own intuition does not extend beyond ZFC (or ZFC + consistency of large cardinals), I look at all those theorems as very interesting implications.

Maybe the following analogy will explain my attitude. We use the standard American ethnic prejudice and status system, as it is generally familiar. So a typical universe of set theory is the parallel of Mr. John Smith, the typical American; my typical universe is quite interesting (even pluralistic): It has long intervals where GCH holds, but others in which it is violated badly, many λ's such that λ^+-Souslin trees exist and many λ's for which every λ^+-Aronszajn is special, and it may have lots of measurables, with a huge cardinal being a marginal case but certainly no supercompact kill bad results ???. This seems no less justifiable than stating that Mr. John Smith grew up in upstate New York, got his higher education in California, dropped out from college in his third year, lived in suburbia in the Midwest, is largely of Anglo-Saxon stock with some Irish or Italian grandfather and a shade of hispanic or black blood, with a wife living separately and 2.4 children. "Come on," I hear. "How can you treat having no $0^\#$ or even CH? You cannot say somewhere yes, somewhere no!" True, but neither could Mr. Smith have 2.4 children, and still the mythical "normal" American citizen is in a suitable sense a very real one. In this light, L looks like the head of a gay chapter of the Klu Klux Klan – a case worthy of study, but probably not representative.

"Does this mean you are a formalist in spite of earlier indications that you are Platonist?" I am in my heart a card-carrying Platonist seeing before my eyes the universe of sets, but I cannot discard the independence phenomena.

As for the other position – B.5, i.e. determinacy, we shall deal with it in:

AXIS C: **KInf od interst (?) subtitle?**

1. combinatorial, semantical !!!!!
2. syntactical !

For me, the determinacy school is strongly on the syntactical side, being very interested in statements about Σ_n-sets of reals. According to the Los Angeles school AD(+DC) is really true in $L(\mathbb{R})$, so why should you bother with proving things in this ridiculously weak system ZFC when the true universe satisfies this wonderful axiom which settles all those great problems the way our intuition tell us is right? Well, I am not so excited by the syntactical flavor of the problems, but more seriously, I agree just that it is a fascinating axiom with a place of honor in the zoo of position B.2 (for problems of this kind), and its following from large cardinals says that it holds for a "positive" family of universes (maybe the Catholics, in the analogy above).

ISSUE: Are there other interesting universes for descriptive set theory?

(L is one, and K, but the LA school thinks these answers are wrong, and put them to sleep.) Of course, the dispute will not be settled, but it may still be interesting (and possible) to give the problem some kind of a concrete answer which may be illuminating; I naturally tend to think there may be others.

Note, the fine structure is also syntactical, but it has a lot of consequences which are not, hence

ISSUE: how much is the syntactical part needed for applications, e.g., is the fine structure needed for the combinatorics in L?

For Jensen, fine structure is the main point, diamonds and squares are side benefits, probably good mainly for proving to the heretics the value of the theory.

Personally, I prefer to get these consequences without the fine structure, but I do not greatly appreciate the search for alternative, "pure", proofs. The question is: when we want to go further, which approach will be preferable? Of course, you will need the fine structure for syntactical statements.

ISSUE: Are these combinatorial principles exhaustive, i.e., sufficient to drive the combinatorial consequences of L?

Of course not. Still, there may be positive theorems in this direction.

ISSUE: Where does the truth lie between the following two extremes

1. Every combinatorial statement is decidable in L.

2. We should have a forcing-like technique to get independence from ZFC+V=L (or for PA, and similar cases, e.g. the twin prime conjecture).

I have strong intuition in favor of both positions, but little knowledge. "Combinatorial" means not syntactical but semantical; consistency strength is discounted as well, as disguised versions of it.

AXIS D: **sizes of interesting sets**

1. natural numbers	!
2. reals	!
3. set of reals	!!!!!
4. arbitrary sets	!!!!!!!
5. large cardinals	!!!

I also have a keen interest in the natural numbers (though too Platonic), but not as a set theorist.

I will put questions on projective sets under D.2, questions on cardinal invariants for the continuum under D.3, the general partition relations of the Hungarian school and the laws of cardinal arithmetic under D.4, and the partition relations for large cardinals under D.5. For model theory, I will put the zero one laws for sentences in some logic under D.1, the investigation of $L(Q)$, $L(aa)$, etc. for models of cardinality $\aleph_1$ under D.3, classification theory as in [Sh c] under D.4, the Łos problem (i.e., categoricity spectra) for $\varphi \in L_{\kappa,\omega}$, κ a large cardinal, in D.5, and Borel linear orders and isols under D.2.

If you are seriously interested (like me) in D.3 , to which this conference is dedicated, then the following would be central:

ISSUE: What occurs if the continuum is $\aleph_3$? Is $\geq \aleph_3$?

With regard to finite support iterations, all regular cardinals bigger than $\aleph_1$ seem to be on an equal footing, but countable support iterations only work for continuum $\leq \aleph_2$. When we become interested in this, the preservation of proper forcing [Sh b, III] and other properties [Sh b, VI] highlights the versatility we have for the case of the continuum equal to $\aleph_2$. We have many consequences of CH, reasonable ways to prove independence from continuum $\leq \aleph_2$ and a few theorems (there is a P-point or a Q-point). But for the continuum being $\aleph_3$ we are quite in the dark (well, more exactly, finite support iteration of ccc forcing tells us a lot, but we were spoiled by the better fate of $\aleph_1$ and $\aleph_2$).

L. Harrington asked me a few years ago: What good does it do you to know all those independence results? My answer was: To sort out possible theorems – after throwing away all relations which do not hold, you no longer have a heap of questions which clearly are all independent, the trash is thrown away and in what remains you find some grains of gold. This is in general a good justification for independence results; a good place where this had worked is cardinal arithmetic – before Cohen and Easton, who would have looked at $2^{\aleph_{\omega_1}}$?

Now consider cardinal invariants of the continuum. There can be relations between them (provable in ZFC) which become trivial if the continuum is at most $\aleph_2$ (like one being always equal to one of two others); but the present methods for independence are too weak.

If you are interested in D.4 (general sets) the following may seem to you central: ISSUE: What are the laws of cardinal arithmetic?

Certainly, I am now very involved in this [Sh g], so my current views may be even less objective than usual, but this subject traditionally lies at the center of set theory:

Zermelo's well ordering says every cardinality is an aleph;

Gödel's **L** was found to show CH may hold;

Cohen forcing was discovered to show that CH may fail;

Jensen's covering lemma comes to answer the singular cardinal problem.

Notice that some "religious wars" are between the two sides of a Möbius strip: i.e we do not understand that the different views are just different ways to express the same thing. E.g. [Sh g] show that looking at things just below the cardinality of the continuum does not make cardinal arithmetic redundant so the restriction to sets of $< 2^{\alpha_0}$ has smaller influence on the kind of problems we encounter.

Still, even between people working on Boolean algebras and the topology of extremely disconnected compact topological spaces, there are differences: Are you interested in free sets as an Boolean Algebraist or independent sets as a topologist?

The future, the reader may well remind me, what will be the future of set theory? Being optimistic by nature, and proving theorems which look to me reasonably satisfying, I am not at all gloomy. More seriously, looking at the last hundred years, repeatedly old mysteries have been clarified by deep answers, dark interludes were followed by the opening of new horizons; some directions require a substantial amount of preliminary study while others can be approached with little; and I find the old lady as fascinating as ever.

* * * *

Let us reconsider the purpose of this note.

In the first place, rather than being accused of "personal distortion", "ideological distortion", "Stalinism", or "making your work the most important", I prefer to claim as my alibi that I am giving nothing more than my personal perspective. Accordingly, I may be foolish, but it is quite hard to prove I am wrong. In any case I have the support of a 20th century trend in history – prejudice is fine, the crime is pretending it does not exist. Also, no originality is claimed – in fact I assume everybody thinks as I do, except when proved wrong. In fact, a small sample indicates to me that the views (= prejudices) expressed here are shared by quite a few, who almost by definition do not tend to write learned articles about it; so in the literature they are non-existent. (E.g., after my lecture, Gitik said that his view is essentially the same, except that he must think more about the American analogy).

Secondly, this all applies *ipso facto* to mathematical logic as I know it. I have little knowledge of recursion theory and considerably less of proof theory, so I refer to model theory and set theory.

Thirdly, why would anyone want to read this article? You know in your heart that you know better what is *important*, what is *good taste*, and so forth. A reasonable guideline may be this: What would I like to be able to read by a reasonable mathematician of Cantor's time? A possible answer: Why has he dealt with particular problems (even if it was just because his tutor[[asviser]] told him to, or perhaps because his tutor told him not to), what were his views – even if not so well-considered as ours, or even self-contradictory, and something about himself and his colleagues.

Note – what a professional philosopher would say should *a priori* be more *coherent*, but it is less clear how it is related to what mathematicians do. Locke's books are not necessarily the best explanations of why Churchill[2] deserted James II, nor Rousseau's of why Robespierre guillotined Danton.

So the reader may ask, how do the views here relate to the author's own work?

I tend toward A.3 (historical reasons) quite strongly, as we should have some objective measure. Hence, I think that having a good test problem is usually crucial to the advance of mathematics. It is, to a large extent, the duty of the new generation to solve the problems of the older one. I thought that while developing classification theory I should try to solve the problems of Keisler and Morley (problems which were what made me start my investigations in the first place). For a long time I was not satisfied with the structure/non-structure theorem on $\aleph_\epsilon$-saturated models as it deals with a class I have introduced, and so it looks like cheating – introducing the class and then solving the problem for it. This is also the reason for the existence of Chapter 14 (For Thomas the Doubter) in [Sh c]. Even though I thought and still do think that the main gap theorem is the main point, I thought I ought also to solve Morley's conjecture, as the main gap was my own conjecture, and I did not want to end like the king who first shot the arrow, then drew the circle. Still, the main gap *is* called the book's main theorem.

I suspect that I have the reputation, or notoriety, of emphasizing the value of sport for fun and for competitive value (I do not mean sport for exercise; in fact I find it strange to try to prove known theorems on one's own with casual glances at the existing proof, for the sake of exercise). As I love doing mathematics, I find it more entertaining to solve a problem than to argue about its possible significance,

[2]First duke of Malborough.

and I have a normal size vanity, so I am also glad to solve a problem just because it is considered hard or important by someone. But even when I know that nobody will be impressed and it may even harm me in some ways, I usually will not refuse the temptation.

"Solovay's inaccessible" started completely as sport: I heard very little about it, and then in January '78, in Berkeley, Harvey Friedman told me "You will not be disappointed by the response if you solve it", (Friedman's conjecture was right.) To my reservations that I do not really know anything about random reals, he responded assuring me that everybody knew that the version with the Baire property is equivalent (on a close reading, this was right, too). Given a choice between working on choiceless universes and Σ^1_3 sets of reals, with a groan I prefer the latter, and I have dealt with it several times till its solution. This naturally improves my view of parts of descriptive set theory, as do the works on the number of equivalence classes ([HrSh 152], [Sh 202],) and on "every set is Lebesgue measurable if large cardinals exist" ([ShWd 241]), though to some extent, this is all cheating – these works were in forcing and/or model theory and not in descriptive set theory proper. According to the proverb "if you want to convince him your country is right, do not argue, just sell him war bonds." (My view changed to some extent only with [JdSh 292], as it makes a connection with the theory of iterated forcing.)

I looked at Fuchs's book on abelian groups out of curiosity, as it does not require much background and looks like interesting mathematics, but also because I hoped to find applications of classification theory. As it happened, I found mostly applications of set theory, which strengthened my belief that you should usually start from the problems and not the method. Had my student Mati Rubin not abandoned his assignment to classify the automorphism groups of saturated models (of first order theories) by interpretability strength to work on the special case of Boolean algebras, I would not have been dragged to [RuSh 84], and then to a long investigation of the quantifier on automorphism of Boolean algebras, etc. Without Cherlin, the non-isomorphic ultrapowers of countable models would not exist ([Sh 326] and [Sh 405]). Fuchs's book and a gallery of good and friendly abelian group theorists persuaded me to write a lot about it, and Haim Judah has led me to much work on the reals.

The other side of this is that if Yuri Gurevich had not left Beer-Sheva and mathematics, we would probably have an additional volume or two on monadic logics and ramifications by now.

But there is more to it, less cynically. I have the weak "neighbor's grass is greener" syndrome. In the strong sense you "know" your neighbor's grass is greener. I "know" it is not, but I would like to have a proof. Also, some doubts always linger as I have some cocky neighbors. So I was curious to try my hand at descriptive set theory too, for example. The reader may ask: How do I like my neighbor's grass? Usually, the grass is quite green, and is really interesting; but certainly not more so. [[rephrase]]

Of course, having a fresh view can be good for your old problems, and what may look like a side problem can give the impetus needed toward one you consider central: So a problem on cardinal invariants of Boolean algebras started the current series of my works on cardinal arithmetic (see [Sh 345]).

But all this is just one side of the "sport" issue, it was my choice, to start from Morley's work, and generally I have not dedicated years of work with little outside attention if I did not either feel the material was important *per se* for reasons I believe in, or just love it. In fact, a large part of my time has been dedicated to such projects, and the result is usually a book. So it should follow that having written a paper on something my books correspond more with what I have stated and less to random chances of proving something. For classification theory, [Sh a], [Sh c] seem to me clear – both the level of generality and the extra work to prove things in ZFC (nonstructure is much easier to prove under extra axioms). In [Sh b] the level of generality is fine, but is it in ZFC? We have explained the excuse above. [Sh g] is strongly on ZFC, but its generality is less clear cut. Probably, I tend to soften with age, which seems quite normal.

I was once told that too diverse work necessarily implies poor taste, but I would never give a theorem a negative value. Moreover I do not mind if the work does not fit (as in fact it cannot fail to fit my views), because I sincerely think that:
THESIS: One should never let ideology or "good taste" stop one from proving a good theorem.

So this means the beauty of a theorem is not defined from all the previous description. Rather it is like the beauty of a work of art, i.e. (by our present knowledge) though criticism can shed light on why we like it or why it is important (a topic on which I could say more), we do not have a precise definition of it.

Mona Lisa was never proved to be great art, and different generations of art critics have viewed it differently, but we can still admire it.

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