

# Noesis

## The Journal of the Mega Society Number 131 May 1997

EDITORIAL  
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A second Chris Langan issue.

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Letter to Ian Stewart of *Scientific American* re "Two Boys Problem"

February 27, 1997

Ian Stewart, *Mathematical Recreations*

Dear Ian:

I've been a subscriber to *Scientific American* for years. Unfortunately, I work a lot and often don't have time to read an issue thoroughly. In the March issue, while discussing feedback regarding "The Interrogator's Fallacy" (September 1996), which I haven't read and now can't find, you mention a certain problem. "The Smith family has two children and one is a girl. What is the probability that both are girls?" You then comment on a certain argument - "whether or not we know that one child is G, the other is equally likely to be B or G" - saying that "it is instructive to see why this reasoning is wrong."

A close variant of this problem appeared in the "Ask Marilyn" column of a recent issue of *Parade Magazine* (copy enclosed; I read Ms. vos Savant's column when possible because we both belong to a club called "The Mega Society"). At the time I read it, I had no idea that you'd already dealt with the problem. Accordingly, I went ahead and solved it, mailing the solution to the Mega Society journal *Noesis* for publication.

Here's the rub. Your solution, to which you apparently have an exclusive attachment, is just a special case of mine. I say that the initial information you gave in this problem admits of two distinct interpretations, only one of which you say is "right". I say they're *both* right. The crux of my argument: if you know that "at least one of the Smith children is a girl", then either the sex of **one** child was **randomly** ascertained or the sexes of **both** children were **exhaustively** ascertained, and each assumption generates a distinct and equally valid conditional probability. It's all right there in the paper.

Naturally, you can always say something like "but only *my* solution conforms to the conventions of probability theory". However, since this would leave unremedied any flaws or oversights in the "conventions" themselves, it would amount to a cop-out.

Incidentally, one of your predecessors in this column, Martin Gardner, was quite fond of a certain philosophical problem called "Newcomb's Paradox". In 1989, I published a resolution in *Noesis* that never got much attention. Despite Gardner's unproven hunch that the paradox is unresolvable, the resolution is actually pretty open-and-shut. Want to take a look? The next issue of *Noesis* should contain an informative Q&A about it.

Keep up the fine work. I look forward to hearing from you (should I be so lucky).

Sincerely,

Chris Langan, P.O. Box 131, Speonk, NY 11972

(cc: *Noesis*)

## E-mail to and from Harvard re Newcomb's Paradox

Jojo and I recently sent the following e-mail to Professor Robert Nozick at Harvard. My e-mail server saves mail automatically; Harvard's dumps it upon delivery. Since we used Harvard's, we'll have to reconstruct the letter here.

Professor Nozick:

You've recently been getting some air time in the Mega Society journal *Noesis*. It involves ongoing discussions of Newcomb's paradox, for whose standard formulation you are partially responsible and on which you are widely regarded as the world's foremost authority.

A paper, *The Resolution of Newcomb's Paradox*, was published in the January 1990 issue of the same journal. Being couched in the rather dense language of computation theory, it generated little initial feedback. However, the topic has now resurfaced, and the original paper has been considerably simplified for purposes of discussion. In contrast to other approaches to the problem by our members, mostly involving the theories of games, metagames, and subjective probability, its unique correctness is increasingly being accepted within the Society itself.

You were notified of the publication of this paper by mail shortly after it appeared, but never responded. Would you be interested in examining it now and commenting for the benefit of our members? If so, please contact me at ([clangan@suffolk.lib.ny.us](mailto:clangan@suffolk.lib.ny.us)), and I'll send hard copies to you by regular mail.

Respectfully, Christopher Langan ([clangan@suffolk.lib.ny.us](mailto:clangan@suffolk.lib.ny.us))

To this, I received the following overnight response from Nozick's Staff Assistant.

Dear Chris Langan,

Prof. Nozick has been on sabbatical in Europe during the fall and spring semester. I have forwarded his mail twice a month, so presumably he has received your communication.

I'll forward your e-mail so that he can communicate directly with you.

Sincerely,

Ghanda Difiglia  
Staff Assistant ([phildept@husc.harvard.edu](mailto:phildept@husc.harvard.edu))

(Jojo: "Man, this e-mail is *great*.  
Now let's see what happens!")

**TO CHRIS COLE (regarding his editorial on pp. 23-24 of *Noesis* 126):**

Okay, Chris, I see we're back to playing our little editorializing games. Since I don't want to keep you away from your politicking, I'll try to make this reasonably short.

1. You maintain that "more structure (in the Mega Society) will engender controversy", supporting your claim with an old joke to the effect that "in academics the politics are vicious because the stakes are low". That's a pretty good joke, because it's so often true. But in the final analysis, how high or low the stakes are is a relative judgment. For you, the stakes are low for two reasons: you've already achieved (or perhaps were born with, or married, or some combination of the above) a certain amount of success in the nonacademic world, and you obviously don't have any brilliant new theory that might knock 'em dead in collegeville. But these reasons don't apply to everybody, and for some of us the stakes are somewhat higher. Insisting on the triviality of this little enterprise of ours is only going to make you more of a target than you already are.

As I've stated before, I happen to agree with you in principle regarding the benefits of informality. But that would leave a lot of power in your lap, and I don't know how advisable that is when your track record for recognizing the solutions to major problems discussed in the journal is 0%. Currently, you're in the position of opposing the law of large numbers and quite a few well-tried ingredients of logic, including metalanguages and model theory, that brook no *rational* opposition, and you apparently cannot be reasoned with. Your powerbase in this group now consists of one fact: you have "physical possession" of the journal, control its timing, and can always get the last word in. Members tolerate this because in the past, they've asked no more of you than that the journal come "reasonably often" and that they not be asked to shoulder any responsibility of their own. But any member of this group can offer them exactly the same terms, with a vote or without it.

Speaking of voting, whether or not "we" vote on whether or not to exclude low-range tests - and I guess we're all aware that most of the voting around here gets called for and done on a personal basis - I'm not going to accept an *ex post facto* application of that ruling to a candidate whose application was previously submitted. Who am I, you ask, to unilaterally "not accept" it? Well, I'm evidently the only person in this group with a proven ability to solve big, famous problems. And although that seems to mean nothing right now, I predict that as the years roll by, it will mean *more and more*.

2. The set of statistical rules you've presented is nonrandom. So you and I aren't talking about the same experiment. (You knew *that* was coming, didn't you?)

3. Since you don't want to properly conduct the experiment I suggested, here's another prediction for you, and it should be even easier for somebody with all your connections I predict that you can't find one (1) professional probability theorist, now working for a

college or university in the U.S., who will back your viewpoint...i.e., who will identify himself fully and say in print that the law of large numbers - or the relationship of frequency to probability that it implies - fails to apply to a closed and finite set of marbles in a box. If he does so, I think I can either make him print a retraction or cause him some severe credibility problems with his employers (I'm not interested in a government probability expert that can't be fired).

4. You say I "pushed the (10-marbles) problem back a step, from the distribution of colors to the distribution of statistical rules". Come on now, Chris. I'm the one who would have been perfectly satisfied if you'd **never mentioned** the prior (pre-box) distribution as a criterion in this problem. **You** are the one who wanted so badly to make an issue out of the prior distribution. This is what I mean by "doublespeak".

You have some background in physics, so let's use a physical example. Say you were trying to measure the effect of air temperature on water. Unfortunately, the freezing and boiling points of water are also influenced by air pressure. Temperature and pressure are two **independent variables**. To measure the effect of one, you must either **eliminate the other**, or failing that, **exhaustively randomize it so that it cancels itself** when you take an average. Bayes' rule affords us a very convenient way to eliminate the prior distribution from consideration. Since you didn't want to use it, I decided to give you the second option; that's what the "experiment" was about. Yet, you've proposed rules by which the implications of one "independent variable", the prior distribution, obscure those of observational frequency. Let's keep our eyes on the ball here. Since you've apparently chosen to oppose a **300-year-old massively-confirmed mathematical theorem**, there's **no possible way** that you can end up on top. So give it up and show everyone that you not only have a big brain, but a big heart as well.

5. When all is said and done, I don't need to make a single **prediction** to justify the CTMU. All I need is one or more solid **retrodictions**. We've all read your primer on Special Relativity and we all know about your one-time acquaintance with the late Dick Feynman of QED fame. I wonder if Dick ever told you that both relativity and quantum mechanics were introduced, and achieved initial acceptance, on the basis of retrodictions? E.g., Planck's quanta explained extant data on black body radiation. Einstein's three 1905 papers were almost completely retrodictive. And Bohr's atom did nothing more than explain the known spectrum of hydrogen. The foundations of 20th century physics is built not on the later predictions of General Relativity and new "fundamental" particles, which were made within retrodictively-generated implicative frameworks, but exclusively on retrodictive advances. Care to argue this point?

The fact is, you know I don't want to reveal too much of my theory before I manage to "unstack the deck" that you and your friends have so carefully arranged here. I need to insure that I receive proper credit for what I've accomplished, and that obviously isn't going to happen with an audience who can't even see that the original 10-marbles

certainly **should**. And saying that "if my stuff is intelligible I should be able to get *somebody* to intellige it" is like saying that if my grip is strong enough, I should be able to squeeze blood from a stone. Communication is a two-way process, and if - even after I've given an ebonics-level explanation of how the CTMU resolves an interesting and intractable philosophical paradox - you still don't want to sustain your half of the process, then what the hell can I say? Are you *really* interested in philosophy, or is it just the rhetorical component that you like?

You know, nobody ever guaranteed that reality was simple enough for everybody to understand. Maybe it can't be broken down into the bite-sized pieces preferred by tube-watching dummies. Maybe reality can only be understood in terms of a structure whose minimal descriptive complexity exceeds that of a box of tinkertoys. Maybe that's why I've introduced it to a group of people who claim to be super-smart. Maybe these people should make at least a token effort to live up to their would-be reputations.

3. You clearly have no idea what kind of "empirically testable propositions" the CTMU contains. I've already presented a few such propositions right here in this journal. In fact, I've managed to sneak so much profound material into *Noesis* right under your nose that when you finally manage to figure out just how much, you'll probably be mortified, especially considering how many times you've publicly repeated your patented nobody-gives-a-hoot-about-the-CTMU-but-Chris Langan routine.

After considerable effort, I've at last come to grips with that fact that no matter what I say to you, you're still not going to understand more than a fraction of it. But I ask you to consider this. There have been many great theories that were at first considered incomprehensible. Yet only a few years later, when popular intuition had been appropriately enlarged, they seemed obvious. If, in a few years, the CTMU seems obvious, what will your present attitude look like then? I assure you that the intellects of those who dismissed relativity and quantum theory as "incomprehensible" when they were first introduced were not favorably evaluated as a result. Furthermore, I assure you that the CTMU has already been presented in just as direct a way in *Noesis*. Don't you think that just a little more interest and attention - "IQ insurance", if you will - might be in order here? Think of the drain on your *bushido* if you're wrong.

4. You state that "we certainly live in a world in which logic can be used to solve certain classes of practical problems, but that doesn't imply that the world is isomorphic to a set of logical categories". Better watch out. You've just come dangerously close to denying that the world conforms to the **mathematical category of logic itself**, and that would be the end of your philosophical reputation! The connection between reality and logic (as considered apart from any incidental set of "logical categories") is actually of implicative strength, and the CTMU is based on it. In fact, every part of reality is isomorphic to some part of logic. Otherwise, you not only wouldn't be able to perceive reality logically, you *wouldn't even be able to sense it*. That's because logic defines not

only how your brain works, but how reality communicates with it.

Think about this. If you deny it, then there exist modes of communication which do not conform to 2-valued logic on any level. In this event, you can't use 2-valued logic to distinguish what is being communicated from what is not, or even the receptive sense impulse from its absence. But if no part of you knows what is being "communicated" to it, then no communication is transpiring! And if no communication is transpiring on any level, then we have no basis to lump the non-communicants into a common reality. Reality would then fall apart, leaving you in the part that actually *does* connect with you by past or future chains of 2-valued logical, cause-and-effect communication.

If you can understand *at least that much*, I'll believe that there's hope for you yet.

5. You say that "according to the understanding at the heart of contemporary philosophy of science, a proposition that isn't falsifiable isn't meaningful". Well, all I can say is that contemporary science is pursued according to the rules of logical implication, and that logic is based on a set of tautological axioms which, though empirically confirmable to an arbitrary extent, are not falsifiable. Yet, if they were "meaningless", then likewise for whatever they were used to establish. Goodbye, science! Fortunately, the CTMU - whose mathematical structure incorporates these axioms in a special way - owes no debt to such a ridiculous philosophy of science.

6. Well, Kev, you've got me on this one: "Chris Langan is not a happy camper." If *you* were now confronting the apparent fact that in order to get any attention for your own intellectual work, you were going to have to reveal it in a way that would allow it to be completely ripped off by any hack with enough connections in the academic network, and that you didn't have one single friend of your own in the entire phony stinking academic world, then you might not be very happy either. Let's face it - for networking purposes, the Mega Society is about as useful as paps on a boar, and I poured way *too much* into this sinkhole before learning that nothing good would ever come back out.

You see, the CTMU has the potential to make people happy by changing the nature of human interaction. But that potential is still unrealized. With as many problems as I've solved for members of the Mega Society, I should by now be interacting with all kinds of friendly, helpful people. Instead, I'm still "interacting" with zombies on *siesta*. And no, I'm not too happy about it. Jojo's Uncle Albert published the three papers that launched his career circa 1905. How happy would he have been in the year 1912 if no one had yet admitted that he'd solved a problem of any kind? You tell me.

7. A "high IQ" may not make someone's work automatically valid, but it does make it worth enough attention to determine that it contains no obvious psychopathy. And if it contains no obvious psychopathy, then it should not be treated dismissively. So again, if Ron's IQ tests have any validity, he should pay closer attention to my work. Your

Now Kevin, at this point I've all but ceased to care about anybody's two-bit excuses for not reading my work or not promising not to steal it. From my viewpoint, it's all real simple: either you're interested or you're not, and either you're honest or you're not. And if you claim to be a philosopher or mathematician, then you're **interested by definition**, especially if you're either (a) in the employ of academia or (b) in the Mega Society. Academics have a moral responsibility to be interested in any new knowledge in their specific fields, especially if it might be lost due to their *lack* of interest, and Mega Society members have a responsibility to fairly evaluate each other's work. And just so there's no mistake, that means **you too**.

I think I've been - what's a good word here? - *saintly* about the way I've let this whole matter slide as long as I have. Much of the value of what I accomplished was understood from the beginning to reside solely in the evaluation promised by our fellow member, and I worked for over a year on the strength of that promise. So it's **too late** for the member in question to decide that he has "nothing to gain by arguing about the obvious fallacies" in my paper (if fallacies there are). If I were "a dangerous crank" or even as litigious as his friend accused me of being, don't you think that something **awful** would have happened by now? You bet it would have! So let's cut the crap, shall we, and drop all of these ridiculous pretenses before they cause more damage to the cause of intellectual progress than they already have.

By the way, if I were the member who promised that evaluation, I'd be gritting my teeth and wishing like hell that people like you would shut up and leave well enough alone. Because when the guy who's getting stiffed on a promise is in as obviously frustrated a state as I am, rocking the boat is potentially a very bad idea. It can lead to impatient outbursts like this one: **Again, I urge the member charged with evaluating my proofs to live up to our agreement and confirm them, cite a specific invalidating error, or refer me to an honest, qualified and level-headed contact willing to promise confidentiality in writing and deliver a timely evaluation in his stead.**

Meanwhile, the proofs are being validated by this member's inability to find an error.

9. I'd love to get involved in Mega Society politics as you suggest, but until I'm convinced that at least one or two other members can recognize a valid solution to a major problem when they see it, I'd kind of feel like I was *lowering myself*.

10. Jojo Einstein wants me to tell you that the reason cannibal butcher shops don't sell "CTMU-understander brains" is that there are evidently only two brains that big and powerful, and that unlike California brains, they're both in use. Furthermore, selling them by the pound would require dividing them into pieces so microscopic as to boggle cannibal technology. Anyway, Jojo - who has followed his Uncle Albert's lead and donated *his* souped-up noodle, complete with curly orange wig, to the Klown Kollege Department of Neurological Research - figures that if you spent half as much time



actually thinking about the CTMU as rationalizing why you shouldn't have to think about the CTMU, you'd already have a CTMU-understander brain of your very own!

**TO RON HOEFLIN (regarding his letter on pp. 19-20 of *Noesis* 126):**

1. I'll begin by assuring you that I'm the last person in the world who would ever have belittled your psychometric or metaphysical research, had you not previously seemed to dismiss my own. Even though your theory is primitive in comparison to the CTMU, it has some good features and seems to be as attention-worthy as any pre-CTMU system. I know how important it is to you, and in light of the work you've put into it, I think you deserve to be recognized for it...provided you can do so without denying the CTMU its rightful place in the scheme of things.

2. In *Noesis* 121, Cantor's paradox was resolved. This paradox comes in two nearly equivalent forms. One states that the powerset of any so-called "largest set" is larger than the set itself. The other version, which serves as a build-up to Russell's paradox, goes as follows. "Consider the set of all sets. Since it contains all other sets, it **is** the largest set. Moreover, being a set, it must contain even itself. But since it is subject to containment, it **is not** the largest set." Although this paradox is nominally about mathematical objects called "sets", it is really a metaphysical paradox in disguise.

When we apply set theory, we fixate on the sets themselves. But in so doing, we ignore a number of concomitant factors. These include not only the extended logical framework of set theory, but the larger structure containing both this framework and the set theorists who work within it. Accordingly, the CTMU recognizes that a "set" is not a concept that can stand alone; it can only exist within a system whose logical apparatus supports it, and - where implication and inference are dual aspects of cognition - its implications and their mathematical contemplation as well. Because of the generality of the term "set" and the comprehensivity of the universal quantifier "all", Cantor's set of all sets must be identified with the supporting system (this is the "more sophisticated definition of a set" to which you refer as a requirement of resolution), and to resolve Cantor's paradox, we must interpret it in systemic terms. Resolution can then be achieved by utilizing certain properties of this system which are not available within any standard (non-CTMU) version of set theory (as described in *Noesis* 121).

The moral of the story: set theory cannot be comprehensively formulated apart from a system of metaphysics, and the CTMU is the one and only system of metaphysics in which set theory possesses mathematical integrity...i.e., in which its paradoxes are resolvable. It's for reasons like this that we **know** that the CTMU is correct.

3. You suggest that I provide a list of basic concepts and axioms for the CTMU, and claim that you are under no obligation to acknowledge it otherwise. The claim is ridiculous, given that I've applied the theory to solve important problems (and no, a

theory does not have to be simplified and explained in order to be truly applied). As far as the suggestion is concerned, I'd love to provide a list, but cannot yet do so.

Thus far, I've described the CTMU only to the extent necessary to apply it to specific problems. This has been quite deliberate on my part, and we've already been through the reasons. The main reason is that I have no academic credentials, and the reasons for *that* are primarily economic. Academia, far from being the Temple of Knowledge that it claims to be, is a money-making enterprise, and I don't have the scratch to buy what they're selling. Since they completely control intellectual commerce at my level of production, this leaves me in no position to benefit from my own work. On the other hand, once I release these theories, they're gone. Because I lack so much as one influential connection through whom to convincingly prosecute an accusation of intellectual plagiarism, they can be misappropriated with probable impunity by any dishonest academic hack in need of them. In other words, I've been disenfranchised with respect to both intellectual credit and remuneration by a system whose members I can outperform virtually on demand, often in their own fields.

The standard line of academia is that everything is based on trust, and that nobody is any better protected than I am. Well, let's take a closer look. If you don't mind, I'll use you as an example. You have the letters "Ph.D." after your name. That means that if you telephone a colleague, journal editor or university administrator to ask for help or complain that someone is claiming false priority over your theory of purposive action, you have some initial clout. You will certainly be asked for your academic background under these circumstances, and if you were in my position - with no more than a high school diploma - you would probably have a very hard time getting anyone to believe that you were *capable* of producing work that another credentialed academic would want to steal. Furthermore, your academic background has probably allowed you to cultivate certain personal relationships with people in helpful positions...people who could be persuaded to pursue the matter on the strength of your acquaintance.

Would my history of publication in *Noesis* serve as proof of priority? I simply don't know. *Noesis* is not an academic journal. It's a nearly unknown, visually unimpressive, sporadically-published stapled-together booklet that, from the viewpoint of an academic bigshot handling a priority dispute, would probably require eyewitness corroboration. But who can I rely on to provide that corroboration? For example, I've been told by people who have requested copies of my work from Chris Cole - who as Publisher has claimed to be providing reprints at nominal charge for those who request them - that he has never failed to ignore a request involving my material. And both of you have been so unappreciative of my contributions that you even refuse to acknowledge open-and-shut, cut-and-dried solutions to problems that you've personally presented! Even now, you - Ronald K. Hoefflin - are obediently looking the other way as Publisher Cole denies my validation, by direct implication of the law of large numbers, of a problem that you yourself designed. So even if you wouldn't disavow all knowledge of my work, why

should I think that you won't simply ignore crucial inquiries?

The members of a group like this are supposed to be each other's *friends*. At some point, you and certain others must have decided that you preferred the opposite role with respect to me. That, at least, is what I infer from your failure to acknowledge that I've solved problems I've clearly solved. Why you decided that, I don't know; someone with my abilities could have been quite valuable to people in your fields of endeavor. But in any case, it's not my fault. So if you want to know why you don't have a road map to the CTMU yet, consult the mirror on your dresser. Meanwhile, until I'm convinced that you're capable of treating me fairly, I'll continue to use just enough of my theory at any given time to solve whatever problem I'm addressing. And if I get ripped off anyway, I guess I'll just have to go to *60 Minutes*. (Hey, now there's an ideal. Unfortunately, as things stand, the Mega Society would look ridiculous.)

4. The CTMU not only relates to classical philosophy by embedding every valid philosophical system ever devised (including yours), but it has dynamic applications in mathematics and virtually every empirical science. Furthermore, it is every bit as "purpose-intensive" as your own theory. Again, if you want a complete, simplified description, just convince me that I won't get abused for my kindness by publicly acknowledging the problems I've already solved, and by promising in writing to stand behind me in case of a priority dispute. On these points there can be no compromise.

#### **REGARDING PAUL MAXIM:**

Well, I just read a lot of real nasty stuff about Paul Maxim again. And do you know what? I see hardly any nasty material from Paul himself!

Now, what does that mean? Why, it means that the Nasty Contest has become one-sided, with poor Paul fielding way more than his share of mudballs.

As everybody will recall, I said several issues ago that "I hope we can accept Paul's word as a gentleman to curtail the vitriol". And so he has. In fact, Paul has been exercising a downright admirable degree of self-restraint lately, proving beyond the shadow of a doubt that he is Mega material through and through. Because, you see, in a dirty low-down mudslinging contest where everybody gets smeared and from which nobody can possibly benefit, it finally makes no difference who threw the first mudball. It's the first man to give up who wins the prize for intelligence.

Paul evidently realizes that the Mega Society has more pressing things to accomplish than ragging on each other all the time. And I'm sure I speak for a lot of other members when I say that that's a very desirable, very refreshing attribute in a new member.

I'd just like to take a couple of pages to talk about what a great member I think Paul

would make. In my recent correspondence with him, he has revealed himself to be a considerate and level-headed gentleman who has a lot of good ideas about how the Mega Society can begin to offer its members something of actual value.

We all have ideas or theories for which we'd like to get some attention. That attention will never come as long as we have a journal staff too busy and preoccupied to make the necessary moves. We all know that Chris (Cole) has a software company and that Rick now has a TV production company (congratulations, Rick!). Their futures no doubt look pretty bright, and they will understandably be working hard to make their lives even brighter. **But what about the rest of us?**

I don't recall reading anything about any of the rest of us when that Wall Street Journal article came out a few years back, do any of you? By that time, I'd already solved all kinds of famous problems and paradoxes, and certain other members had made interesting contributions of their own. So what happened?

Well, I think we all know what happened. The reporter's nose got pointed towards Southern California, and that was all she wrote for us rank-and-file problem-solving nobodies in colder and less glamorous parts of the world. A little honesty and consideration at that point might have changed my life. How about some of yours?

**Enter Paul Maxim.** Paul knows whom to contact and how to go about setting up an inter-organizational network from which all of us can benefit. And better yet, he'd be willing to maintain these connections by serving the Mega Society as its Secretary... that is, by doing exactly what the current staff of Noesis doesn't care enough to do. Simply put, if Paul Maxim doesn't do these things, they won't get done. How many of our members want to throw away what may be their only chance to finally have somebody working on their behalf?

Let's not forget that this whole controversy began when Paul was rudely and without explanation turned down for Mega membership because somebody didn't like the tests he took. It seems that a few key members believe that only Ron's and Kevin's tests have high enough ceilings to serve as means of qualification. But that assertion requires proof...proof that no one has ever provided.

How much ceiling a test has depends in some respects on the age of the person taking it. A test matching the ability of an average 20-year old has enough "ceiling" for a 10-year old child with an IQ of 200. Since the Pintner intermediate, a very reliable IQ test once very popular among educators in this part of the country, was designed for and normed on groups of students up to the 12th grade level, that's just about where it topped out. Now, does anybody want to guess how old Paul was when he took the Pintner Intermediate? That's right - Paul was 10 years old! So there seems to have been plenty of ceiling, now doesn't there?

As Mega Society members, we know that Ron's and Kevin's super-high ceiling tests have some very real advantages over tests like the Pintner Intermediate. Their problems are far more difficult, and they place a higher premium on power than on speed. But they have drawbacks as well...drawbacks which put us on very shaky ground if we become too picky about which tests we'll accept. The fact is, we lack a coherent **theory of intelligence testing**, and without such a theory, we can't pass final judgment for or against any particular well-normed IQ test.

Let's face it. We have many members who did **not** take a Hoefflin or Langdon test to qualify. Are we going to threaten to expel them if they refuse to take one of the newer tests? Of course not. That would be cruel and inhumane. So why on earth should we inconsistently reject a potentially valuable member on the same cruel and inhumane grounds? Clearly, we should not. We should welcome Paul with open arms.

IQ is not a matter of opinion, and it is certainly not a popularity contest. So why should we let the opinions of certain very opinionated members determine which mega-level scorers will or will not be admitted, and will or will not be allowed to contribute to the well-being of the group? If these members were offering us more than we have been getting in return for bowing to their opinions, then perhaps we'd have a hard decision to make. But they aren't, and their opinions are thus of immeasurably less value than the gift of a single intelligent and potentially productive new member.

We all know that the way the Mega Society is now set up, there is **no provision for denying membership to anyone tendering a qualifying score on any well-normed IQ test**. Maybe there was once, but the official structure on which the old constitution was based is a thing of the past. For this very reason, members with axes to grind are now scrambling to convince everybody to adopt an organizational structure which will let them achieve their ends at group expense. Are we stupid enough to let this happen?

Let's hope not. What we have now is a group that provides meaningful benefits for only a very small fraction of its members...the small fraction that designs and markets IQ tests, and secondarily, their personal friends (who, not accidentally, run *Noesis*). Yet, as grateful as we may be to these members for what little time and attention they do manage to spare us, the privilege of claiming a mega-level IQ is not their property to give. So we owe them nothing but a handshake in return for their charity.

By remaining humbly silent while others berate him, Paul is proving that he is worthy of our fellowship. Granted, he was terribly frustrated, but political inequity always breeds frustration and the harsh words that follow. It is not Paul's fault that he was unjustly singled out for rejection even when he holds credentials equal to superior to those of some members. So let's all cast a vote for fairness and compassion by welcoming Paul Maxim into the Mega Society as both a friend and a helper!

Chris Langan