Noesis

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Regarding submissions: Probably the feeling that one has a one-in-a-million level intellectual production worthy of *Noesis* is more prevalent in the lunatic population than in the highly-intelligent non-lunatic population, assuming we may make such a distinction.

--Richard May

[Ed's comment--Instead of a sharp distinction between lunatics and non-lunatics, there might be a range across which greater mental shortfalls happen to be paired with grander delusions.]

IN THIS ISSUE
COST PER PAGE TABLE
NAIVE RELATIVITY BY ROSNER
LETTER FROM MARILYN VOS SAVANT
REPRINTED LETTER FROM J. ALBERT GEERKEN
A TELESCOPE OF CONSIDERABLE APERTURE FOR THE POOR BY CHRIS HARDING
LETTERS FROM ROBERT DICK
LETTERS FROM ROBERT HANNON
MATERIAL FROM MIKE PAGE, ROBERT LOW & CHRIS COLE

***Noesis* numbers**	**Average # of pgs**	**Cost per issue**	**Cost per page**	**Other stuff**
1 - 5	4.2	22 cents	5.2 cents	Hoeflin editor, varied titles--*Titania*, *Titanic*, *Insight*
6 - 9	8.6	unchanged	2.6 cents	Hoeflin editor
10 - 15	6.7	12 for $10	12.5 cents	"
16 - 20	8.4	unchanged	10 cents	*Noesis* new title
21 - 24	10	"	8.3 cents	Hoeflin editor
25 - 30	9.3	12 for $20	17.8 cents	"
31 - 38	9.5	unchanged	17.5 cents	"
39 - 43	12.4	"	13.4 cents	Erlandson, Cole, Hart, Hajicek, Inada editors
44 - 49	12	"	13.9 cents	Langan editor
50	8	"	20.8 cents	Ray Wise editor
51 - 56	11	"	15.2 cents	Hoeflin editor
57 - 65	15.2	"	11 cents	Rosner editor, Mega and Noetic Societies merge
10 - 65	599/56 = 10.70	$1.44	13.49 cents	

Noesis #	Avg # of pgs	Avg pgs of Hannon	Cost per iss	Cost per pg	Per non-Hannon pg	Other
66 - 72	17.4	.9	12 for $20	9.6 cents	10.0 cents	Hannon 1st appears in issue 66
73 - 76	23.75	5.5	unchanged	7.0 cents	9.1 cents	
77 - 82	22.7	1.7	$2	8.8 cents	9.5 cents	
83 - 85	26.3	7.3	unchanged	7.6 cents	10.5 cents	
86 - 88	16	2.3	"	12.5 cents	14.6 cents	
89	16	16	"	12.5 cents	infinite	
90 - 91	18	8.5	"	11.1 cents	21.0 cents	
92 - 101	17.7	.7	"	11.3 cents	11.8 cents	
66 - 101	693/36 = 19.25	101/36 = 2.81	$1.90	9.86 cents	11.54 cents	

NAIVE RELATIVITY
by Rick Rosner

I got an A in first-semester calculus, a B in the second semester, and a D in the third. Fourth-semester calculus, covering the abstract logical justifications for calculus which were invented a couple centuries after Newton & Liebniz, I flunked at least twice. The class was all proofs, and I quit going. So was group theory, and I quit that too.

I know how to do special relativity. You plug in the lambda, the square root of one minus vee squared, to get length contraction, time dilation, mass increase. It's real simple, and all you need is a trig table. If vee is sin θ, then lambda is cos θ.

Sometimes I have dull fun thinking about confusing or surprising aspects of special relativity (axis rotation, twins, a 20-foot Buick in a 10-foot garage), but not if there's anything good on cable. I almost never think about the standard fundamental equations of special relativity, the axis translation stuff--y = y', x' = x - vt over lambda. That's where the fun isn't. Plus I have a whole different (addled) idea of the fundamentals of relativity. Different assumptions, same lambda, so who cares, unless it leads to different falsifiable conclusions?

Robert Hannon has sent an article, "Time Dilation and the Half-Life of Pions." In the past, I woulda just stuck it in, room permitting. But because his stuff pisses off at least several of you, I decided to actually read it. In the article, Hannon shows that the fundamental axis translation equations can predict only time contraction, not time dilation, requiring speedy pions to expire faster than their stationary buddies.

This doesn't happen to actual pions, and it didn't happen when I had to do the pion problem in class. (I just plugged in lambda. I bet Jane Actual Scientist also just plugs in lambda. She's forgotten undergraduate physics. If she gets nervous about metaphysical foundations, she can go ask the guy down the hall who teaches Phys 321. George Gamow, who co-hypothesized the big bang, had forgotten calculus, plus was often drunk. For help in calculus, he asked the guys down the hall. He was a lot of fun, moreso than the guys who remembered calculus, and he came up with the preeminent cosmological theory of our time.)

Hannon says there's something wrong with what the fundamental equations of special relativity say about pions, though you wouldn't notice just plugging in lambda. There's a sinister curtain hanging between the fundamental equations and the people who use them. Looking through the curtain (if they even bother to look), scientists see the equations incorrectly, and it is this incorrect use of the equations that somehow leads to theoretical predictions which agree with the actual world. Hannon has been able to rip holes in the curtain and see the equations in their naked wrongness.

I haven't wanted to be mean to Hannon. Some of you guys, however, get a charge out of tearing him up. Aren't there people in your very own neighborhoods you can ridicule in person? Yell at bad drivers or something.

If there's room, I'll run his article.

POSTSCRIPT: In case you hadn't noticed, this article contains a lot of sarcasm towards Mr. Hannon. It was fun to write. However, I've just read a month's correspondence from him in order to assemble this issue, and I feel bad, because he seems like a nice guy, a good writer, not crazy and not stupid. (No combination of these characteristics necessarily qualifies someone to have their stuff run in *Noesis*.)

I'm a craven guy. Y'all rag me for printing Hannon, so I turn around and join you in dissing him.

I believe in 20th-century physics. Though I also believe that it will be replaced by more complete physics in the future, I doubt Hannon's arguments have much relevance, and when Price and others write that Hannon has made errors, I believe them while being too lazy to follow either side's math very closely.

In Hannon's favor are the following points:
Articles aside, he writes good letters (and he flatters me).
Arguments against his stuff from other members provide material for *Noesis*.
It's not much skin off *Noesis*'s butt to run Hannon's stuff.

Here're the points against Hannon:
I think it's unlikely that modern physics is as wrong as Hannon seems to think it is, especially in the ways he thinks it's wrong.
Many members are angered and embarrassed by crackpot material appearing in *Noesis*.
Perhaps angry and embarrassed members are less likely to remain interested in Mega.

STUFF TO NOTICE–

Some people want to know how to get in touch with me or Chris Cole via e-mail. I don't have a modem. Here's Chris's e-mail address: chris@questrel.com

Send in material! Send material! Send material! Some terrorists have kidnapped my dog, a shi-tzu named Flibble. They say they will shoot Flibble unless you **SEND IN MATERIAL**, so please do even if you despise shi-tzus.

Dues are still two bucks per issue. Make checks payable to me, not Noesis. If I get a check payable to Noesis, I have to forge my name on the payee line. Elvis's dad went to jail for forging checks. Or send old gold jewelry. Each gram of 14K gold equals three issues, but isn't it easier to **SEND IN MATERIAL**? For members, two pages of material equals one issue.

LETTER FROM MARILYN VOS SAVANT

Dear Rick:

Funny stuff! (Especially the "dormant" next to my name on the address label. Good grief. You should angle for Jay Leno's job.) Anyway, enclosed is $10. I certainly wouldn't want to miss any more issues like #97 and #99, would I?

Sincerely,
Marilyn vos Savant
[Ed's note–"Dormant" on an address label means only that the member hasn't sent dues money in a long while. No negative connotation intended.]

COMMENTS FROM J. ALBERT GEERKEN REPRINTED FROM OATH

Dear Ron:

Concerning your publication in OATH (Issue 24), Nov. 1994, I would like to follow it up with some additional information pertinent to the solution to the numerical series:

15 1/4 6 /12 3 15/16 2 15/16 ?

The solution involves only simple arithmetic, without recourse to algebra or other disciplines. Furthermore, only four or five lines, short ones, are required for the solution. None of the solutions submitted to me thus far (from members of your or other publications in which my problem appeared) have come up with the solution and answer I am looking for, including two from a subscriber to *Noesis*. Incidentally, I have not heard from Marilyn vos Savant, who must be a member of the Mega Society, and therefore does receive *Noesis*, as one would expect.

Most, or perhaps all, of the solutions I have received thus far are correct, except that they do not comply with Occam's Razor (simplicity) as far as possible. Not surprisingly, the several answers I got were all different, because, as you well know, there are more answers than one to number series problems. However, one or two came close, but not close enough to agree with my solution and answer.

I had intended not to publish my answer, reserving it for a possible future IQ test, but I have changed my mind on this. Sooner or later, after all else fails, I will announce the answer, as well as the simple solution to this conundrum, but not before Marilyn vos Savant admits that she is unable to come up with my answer! I shall once more (personally, if possible) try to communicate with her. I know she must have her hands full with her Q & A column in *Parade*, meant for the general public, but my problem is one she should not ignore. If she does, I shall send it to the Guinness Book of Superlatives. I hope Marilyn will come up with the right solution and answer.

Season's Greetings to all.
J. Albert Geerken (Al)
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Newark Valley NY 13811

Chris. Harding
c/o P.O.Box 5271,
The Mail Centre, 4702.,
North Rockhampton,
Queensland, AUSTRALIA.

Dear Rick.,

I enclose an item below for publication in Noesis given the new rules of demand on contributions. I am submitting this since it might appear novel in view of the more intellectual or pseudo-intellectual material that finds its way in.

Chris. Harding

A TELESCOPE OF CONSIDERABLE APERTURE FOR THE POOR

The Newtonian telescope (also called 'the poor man's telescope') was (as it's name implies) supposedly first conceived by Issac Newton in the 17th century who built a 1 inch aperature telescope as a young man - though enough evidence exists to show that he was beatened by Leonardo two centuries before who built this form of the telescope with a diameter of 5 feet - which in size if nothing else rivaled the great reflectors of our own time.

History records that Leonardo almost lost his life as a result of this

being called to account to the inquisition when the mirror makers of the time reported these goings on to the Church. A fact which no doubt brought this line of enquiry to a close.

It is interesting to note that Leonardo had access to ancient Roman texts and I suspect that he was not the first by a couple of millenia. Some speculative evidence suggests that the invasion of Ancient Brittany was made possible by the use of the telescope. The Roman hill stations built accross England were said to have been observing stations which used the telescope as an early warning system for the massing and movement of troops.

The Newtonian Telescope focus light not by the use of a lense like the older refractor but by the use of a mirror at the bottom of the tube. While the refractor sufferes from cromatic dispersion the Newtonian form does not. Such mirror telescopes may be made cheaply for their size hence being named 'the poor man's telescope'.

My interests in Mirror making go back to 1957 when I complete a 4.75 inch of 42 inches focal length and later a 6.5 inch mirror of 63 inch focal length shortly afterwards. In 1962 I completed a 10 inch f/7.2 mirror from 1.25 inch polished plate glass ie. one having a focal length of 6 feet. This was mounted that same year and a few months after we shifted house before I turned 18.

It resolved fine detail in the cloud belts of Jupiter [to such an extent that the markings were at times too numerious to count or put in drawings - since the globe rotates leading to distortion] and that planets four main moons into small globes - the colours on the globe of Jupiter were very beautiful and a sight to behold !.

I was able to watch the seasons change on Mars and do work of a serious nature for the B.A.A. on Saturn's rings which were edge on in 1966. Views of the our own Moon were stunning and a constant enjoyment for visitors of which we had an endless stream.

I purchased a 12.25 inch mirror of f/6.67 [81.8 inches f.l.] in 1969 which was mounted that year. I had it for 22 years before selling it at that point having used it little in the previous 9 years !.

My diary tab on a new 16 inch mirror follows below. It is hoped to shortly begin a much larger mirror and that even the next one will not see an end to it.
This year a long time friend Bob Berry [whom I'd first met in late 1961] began a local astronomy group. This had stimulated both of us to begin making larger mirrors. My efforts are recorded here for a 41 cm diameter (16.14 inch) f/11 (176 inch focal length) mirror [in 15 mm (0.6 inch) thin polished plate glass] begun in the morning of thursday 29th September 1994. The mirror had had 44 hours grinding included the first attempt at polishing when it was returned to fine grinding on the advice of Bob Berry who had assisted with the work to this point. The mirror would not polish properly despite having passed the 'pencil test' and we feared the cause to be the thin glass.
By 7-11-1994 monday it had had 33 hours of polishing on this my second and final attempt at polishing to that point. The last 2 hours had occured with the centre [a T section slightly off centre] of the hcf lap removed. Testing revealed mirror somewhere near correct depth after allowing it to cool for several hours; though image still unstable due to air turbulance (had bedroom airconditioner running whilst others were off ....mirror in bedroom while setup was in the kitchen - hallway being too short to accommodate the 29 ft 4 inch radius of curvature); with 20 mm eye piece image of torch appeared fairly clear ie. x440: at this point it was concluded that the central depressed area had diminished slightly. Total time to 7-11-1994 was 77 hours.
8-11-1994 tuesday - cut out '3 prong tapering star figure' from the hcf lap. 1 hour of polishing with 4-5 inch stroke overhang on far side with no

overhang on near side figure appearing to deteriorate stroke being too long. Next cut out a '6 prong tapering star figure' from the hcf lap with a further 1 hour 30 minutes of polishing with 4 inch stroke overhang (being slightly shorter strokes) the figure appearing to improve once again but only slightly.
9-11-1994 wednesday - 1 hour 30 minutes of polishing with 3 inch strokes (as above) figure continuing to improve slightly. General appearance of the mirror is one of being too deep caused in the main by the central depressed area but this of course of late some what reduced in size and its effect on light scatter. Bob Berry tested the mirror at this point and thought the figure had definitely improved - at x440 the torch filiment seen through a 20 mm eye piece at the radius of curvature could just be made out.
10-11-1994 thursday - 2 hours polishing with one-sided 2 inch strokes. Testing of mirror showed continual improvement of the figure. Used 20 mm e.p. and the just purchased 23 mm e.p. (adjustable barlow zoom lense combination allowing equivalent f.l. to be adjusted from 18 mm to 6 mm. proved too difficult to collumate for purposes of testing the mirror).
11-11-1994 friday - 3 hours polishing with one-sided 1.5 inch strokes. Testing of mirror showed slight improvement of the figure. Used 20 mm (x440) and 23 mm (x382) e.p. [Some astigmatism in evidence - either due to thinness of the glass or poor columation of the set up ?.]. N.B: 23 mm e.p. of superior quality to that of the 20 mm e.p. which can not be accounted for by the difference in magnification.
13-11-1994 sunday - Cut out further segment in the centre between two of the 'star fins' on the hcf lap. 2 hours polishing with one-sided 1.5 inch strokes. Bob Berry tested the mirror with some approval it having improved further since he'd seen it last. It appeared to me to have improved slightly over the last stint of polishing.
14-11-1994 monday - Cut out yet another segment from the centre as above on the same side. 1 hours polishing as above. Under test mirror continues to show slight improvements.
15-11-1994 tuesday - 2 hours polishing as before. ~Seeing~ inside the house appeared ~bad~ but after turning off the airconditioners it settled down enough for me to see that the 'fuziness' over the centre of the 'filiment' image had continued to decrease. Mirror no longer anything like as deep as it was other factors more and more 'taking hold'.
16-11-1994 wednesday - 1 hours polishing as before. Further slight improvement.
17-11-1994 thursday - Cut more away from the hcf laps inner section on the same side as before. 1 hours polishing as before. Further changes in the right direction observed at this point.
18-11-1994 friday - 3 hours polishing as before. Preliminary testing showed knife edge much straighter.
20-11-1994 sunday - 1 hours polishing with a mix of very short one-sided overhang strokes. Mirror depth nearly correct at this point with minor irregularities to contend with. Biggest problem seems to be either missallignment (collummation) or sagging due to thin glass ?. Which still to be determined.
23-11-1994 wednesday - 1 hours polishing with ultra-short one-sided overhang strokes. Mirror seems nearing correct dept. I was surprised by the sharpness of images at x382 now being able to see better than at any time previous to this [Coils in the light element being easily counted]. Had to let the mirror 'cool down' for something like 8 hours prior to testing.
24-11-1994 thursday - 1 hours polishing as above. Mirror seems not to have changed ?.
25-11-1994 friday - 2 hours polishing using 1.25 inch one-sided overhang strokes. Mirror appears to have deteriorated using the 23 mm e.p. [x382].
26-11-1994 saturday - Removed another (off-centre) small section from the centre of the hcf lap. 1 hours polishing using 1 inch one-sided overhang strokes. Mirror slightly smoother under knife edge test. Tried out new 12.5 mm e.p. at radius of curvature which gave x704 this proving not to give a clear image.
27-11-1994 sunday - Removed another (off-centre) chunk from the centre of the hcf lap this reaching further to the edge than any previous cut. 1

hours polishing as above. Bob Berry and I tested the Mirror at this stage. Both of us agreed that the Mirror was now only a touch too deep and smooth from edge to edge. We both got sharp images using x704.
28-11-1994 monday - 1/2 hours polishing as above. Mirror tested after 14 hours off its lap and found to be approximately the correct dept. Using x704 the image appeared even sharper than before. [second attempt total polishing time 59 1/2 hours & total time 103 1/2 hours work time all up].
29-11-1994 tuesday - Bob Berry tested the mirror at this point using his own 25 mm (x352) 20 mm (x440) 12.5 mm (x704) 9 mm (x977) 7 mm (x1257) & 4 mm (x2200) eye pieces. The image appeared sharp at x977 but had begun to show evidence of breaking down at x1257 and was not good at x2200 though still able to be focused. At this power we both observed fine scratches on the surface of the tourch glass. He also tested it under the knife edge and mesh. He thought it could be further improved but said it was up to me if I wished to go on with it at this point. He thought it was still very slightly overcorrected on average and said if he were doing it he would have at least attempted the very small possible available gain. The 'artificial star rings' were sharp on both sides of the focus but the disk appeared slightly brighter on the outside of the radius of curvature. I pointed out that the 'brightness' of the rings were slightly more concentrated in the middle region of the disk on the inside suggesting that the 'average dept perception' might be more due to zonal irregularities and there fore less likely to be correctable. I felt I could not without risk do any better and there fore would leave well enough alone at this point !. THE MIRROR IS THERE FORE NOW CONSIDERED COMPLETE.
02-12-94 friday - Mirror sent away to have it Aluminized and hardcoated.

You may wonder why it is this mirror was figured to f/11 and not some more sensible length. After all would not this require a tube of about 16 feet length to complete the system ?. Firstly I had intended to make an f/9 mirror but had not counted on the time I'd have to spend grinding out the glass. I had only 80 grade carbrundum available to me. It had also been over 30 years since I'd done anything similar. Also I was worried about the glass flexing being so thin and in any case had doubts about my ability to do the work given my state of health. I was restricted on available materials not being able to get pitch and having to improvise with cloth and hcf. An f/11 system would be easier to make than an f/9 so in the event I found myself having trouble grinding out the glass (even with help !) I simply gave in to a compromise.

The situation is easily fixed. All I would need to do would be to make a small flat mirror and fold the optical system. A 4 inch mirror would bring down the height of the eye piece about 7 feet which would not be too hard to put up with when observing at the zenith. More would much extra light be lost over a more usual arrangement.

However it is likely nothing will be done with it one way or another since a larger mirror beckonds.

Jan 17, 1995

Dear Rick,

I read with some distress Chris Langan's long letter about how unappreciated his scientific and mathematical contributions are. There seems to be a radical cognative dysjunction between a number of Mega members and me.

Why in the world do so many people expect so many Mega Society members to be expert in the cutting edge of advanced mathematics and theoretical physics? Most of us scored very high on intelligence tests. Why, for example would being highly intelligent make one a fluent reader of ancient Egyptian hieroglyphics? Because intelligent people have large vocabularies? So why expect many of us to have years of training in mathematical logic or mathematical physics? Because mathematics and/or cosmological physics is the queen of the sciences? What if I/we fail to appreciate the importance of these subjects?

Chris Langan wants us to solve large important problems or else what are we worth? Pardon me, but life and its difficulties are not "problems" to be "solved." Chris apparently is an amateur mathematician. More power to him! I am an amateur practitioner of the liberal arts and an amateur student and practitioner of religion. Allow me to get a little hostile and say that it takes a lot of nerve to say that abstract theoretical problems far removed from the lives of the great majority of people are IMPORTANT. No, I say, the meaning and purpose of human life, how life should be lived, what are "the most important Commandments", these are the most important. I don't care about the Big Bang. It's interesting to note that there was such an event, but that's about all I want to say about it.

Telling the difference between appearance and reality, taking care that one's light is not darkness, these are important. Parallel universes and the structures of black holes are not.

Modern science is the process of knowing more and more about less and less. Until finally we reach the ultimate scientist: one who knows everything about nothing. I am fed up with narrow value-free studies. Why not discuss matters that everyone can understand without years of reading of obscure textbooks?

Let me tell you what intelligence is good for by a simple example. I live in central New Jersey a block away from an east-west divided highway, US Route 22. Several years ago my little sister was driving to see me from New York City. She called for directions. I told her how to get on Route 22 westbound. Then I said "Make a cloverleaf U-turn at Routes 202-206." Did she understand what I meant? She did. I bet everyone in the Mega Society can understand what a "cloverleaf U-turn" is. Well, my sister is smart, somewhat like I am. Nobody else I ever gave that direction to knew what I was talking about!

Knowing how to make a U turn via a cloverleaf interchange takes smarts. No doubt being Albert Einstein took smarts. But making sense out of Relativity and Quantum Mechanics takes more than smarts, it takes intensive and extensive training, which in turn requires a burning interest in those subjects. The last paper I tried to write for an engineering journal was rejected with a note from a reviewer

ROBERT J. HANNON 4473 Staghorn Lane Sarasota FL 34238-5626
14 Jan 95

Rick Rosner · NOESIS · 5139 Balboa Blvd · Encino CA 91316-3430

Dear Rick,

To Mike Price (NOESIS 101). You know zero about me, yet you imagine you are qualified to imply I don't understand basic algebra. I am impelled to defend myself against that specific aspect of your overweening arrogance. I hold a BSEE (electronics) summa cum laude from Tufts University (59). I took graduate courses at MIT, BPI, and SEIT. I was a straight-A student in math, physics, the other sciences, and most other subjects. For over 40 years, I worked on and led advanced R+D programs in Radar, AICBM Defense, Communications, Elint, Signal Processing, Air-to-Air Combat Training Systems, Electronic Instrumentation Systems, and Heavy-Lift Space Vehicles. I hold five patents, most of which deal with solutions deemed "impossible" by the conventional wisdom up to that time. I had the privilege of working with and leading the work of some of the finest, best-educated minds in the US electronics industry. None of those people ever found my understanding of algebra or physics questionable. But of course, none were members of MEGA, so they weren't competent to judge.

Your letters to Rosner and Langan prove you both arrogant and uninformed. You seem to know zero about the physical and mathematical bases and origin of the Einstein-Lorentz Transformation. I suggest you **study** Einstein's two derivations, (1905) and (1916). You may learn something, assuming **you** understand his algebra! You'll find no mention of "photons" in Einstein's derivations, or in anything I've published on SR; only "rays of light" or "light signals".

The relationships x=Ct and X=CT (therefore x/t = C = X/T) are a predicate, ie, an initial assumption, of all rigorous derivations of the ELT. Those relationships are the simplest algebraic form of Einstein's assumption that C, the velocity of light in empty space, is a **universal** constant. x = Ct and X = CT are intrinsic to both of Einstein's derivations. It is impossible to derive the ELT withoutthose relationships or their algebraic equivalent. I've analyzed many different derivations; x=Ct and X=CT are always there.

Derivation of the ELT is an exercise in kinematics (the branch of dynamics dealing with motions, excluding force and mass) and analytical geometry. Einstein's two slightly-different kinematic models deal only with two **motions**: that of two Cartesian Systems of Coordinates (SCs) relative to each other at **velocity** V, and that of the wavefront of a ray of light (WRL), at **velocity** C relative to what Einstein calls "the moving system". The x and X axes of the two SCs coincide, and the coordinates (x,t) and (X,T) are those of the intersection of the WRL with the x and X axes. The ELT equations describe those coordinates, **nothing else**. There are no static "events" in Einstein's model. To apply the ELT equations using values of x and t (or X and T) which are not in the ratio C violates a fundamental rule of algebra, of which you seem to be unaware.

Prove me wrong. Provide me with a rigorous derivation of the ELT in which x=Ct and X=CT do not appear in any form, recognizing that it is a kinematic analysis involving two constant velocities, V and C.

No member of MEGA has even attempted to **prove** me wrong about the ELT. All I have had on that subject is arrogance, dogmatism, authoritarianism, calumny, ridicule, and a few proud displays of ignorance.

Robert Dick (NOESIS 100): Your approach to differences of opinion seems to be: "If I can't offer rational argument, then I can try to "prove" my beliefs by ridiculing my opponent." Your "Advertising Supplement" is very amusing. I am humbled by the honors you heap on me by placing me right up there with the greatest genius since Newton: James Clerk Maxwell. Clearly my hypothesis has stirred your imagination to the point of psychotic obsession, and, unfortunately, further displays of your rather peripheral knowledge of your subjects.

Note: there is a practical device that dynamically separates fast molecules from slow molecules. It is very simple. Many have been built, and they have been produced for sale. They have been used to provide cooling air to miners working in deep mines where the ambient temperature would otherwise be intolerable.

TO THE MEMBERS OF MEGA: Rick Rosner is the near-ideal editor for NOESIS. He understands that the functions of an editor do not include those of a CENSOR. If you want NOESIS to be a censored publication, then it will never include an idea with which its editor disagrees. Only the editor's beliefs will be seen in NOESIS.

Most sincerely,

Robert J. Hannon

"Political scientist" L David Raub reports a poll of 72 of the "leading cosmologists and other quantum field theorists" about the "Many-Worlds Interpretation" and gives the following breakdown [T].

1) "Yes, I think MWI is true" 58%
2) "No, I don't accept MWI" 18%
3) "Maybe it's true but I'm not yet convinced" 13%
4) "I have no opinion one way or the other" 11%

Amongst the "Yes, I think MWI is true" crowd listed are Stephen Hawking and Nobel Laureates Murray Gell-Mann and Richard Feynman. Gell-Mann and Hawking recorded reservations with the name "many-worlds", but not with the theory's content. Nobel Laureate Steven Weinberg is also mentioned as a many-worlder, although the suggestion is not when the poll was conducted, presumably before 1988 (when Feynman died). The only "No, I don't accept MWI" named is Penrose.

The findings of this poll are in accord with other polls, that many- worlds is most popular amongst scientists who may rather loosely be described as quantum gravitists/cosmologists. It is less popular amongst the wider scientific community who mostly remain in ignorance of it.

More detail on Weinberg's views can be found in *Dreams of a Final Theory* or *Life in the Universe* Scientific American (October 1994), the latter where Weinberg says: "The final approach is to take the Schrodinger equation seriously [..description of the measurement process..] In this way, a measurement causes the history of the universe for practical purposes to diverge into different non-interfering tracks, one for each possible value of the measured quantity. [...] I prefer this last approach"

In the *Quark and the Jaguar* and *Quantum Mechanics in the Light of Quantum Cosmology* [10] Gell-Mann describes himself as a post- Everett. His exact metaphysical status on the reality of the other Everett-worlds is left ambiguous (perhaps delibrately -- shades of his earlier position on the "reality" of quarks, methinks), however he describes himself as an adherent to the Everett interpretation.

Steven Hawking is well known as a many-worlds fan and says, in an article on quantum gravity [H], that measurement of the gravitational metric tells you which branch of the wavefunction you're in and references Everett.

Feynman, so far as I know, is the only one who had not made a public statement, apart from via the Raub poll, directly favouring the Everett interpretation. However Feynman always emphasized to his lecture students [F] that the "collapse" process could only be modelled by the Schrodinger wave equation, which sounds Everettish.

[H] Stephen W Hawking *Physical Review D* v13 p191 (1976)
[F] See the Feynman biography "Beat of a Different Drum"
[T] FJ Tipler *The Physics of Immortality*, pages 170-1

Q32a Is Everett's relative state formulation the same as many-worlds?

Yes, Everett's formulation of the relative state metatheory is the same as many-worlds, but the language has evolved a lot from Everett's original article [2] and some of his work has been extended, especially in the area of decoherence. (See "What is decoherence?") This has confused some people into thinking that Everett's "relative state metatheory" and DeWitt's "many-worlds interpretation" are different theories.

Everett [2] talked about the observer's memory sequences splitting to form a "branching tree" structure or the state of the observer being split by a measurement. (See "What is a measurement?") DeWitt introduced the term "world" for describing the split states of an observer, so that we now speak of the observer's world splitting during the measuring process. The maths is the same, but the terminology is different. (See "What is a world?")

Everett tended to speak in terms of the measuring apparatus being split by the measurement, into non-interfering states, without presenting a detailed analysis of *why* a measuring apparatus was so effective at destroying interference effects after a measurement, although the topics of orthogonality, amplification and irreversibility were covered. (See "What is a measurement?", "Why do worlds split?" and "When do worlds split?") DeWitt [4b], Gell-Mann and Hartle [10], Zurek [7a] and others have introduced the terminology of "decoherence" (See "What is decoherence?") to describe the role of amplification and irreversibility within the framework of thermodynamics.

Q32b Was Everett a "splitter"?

Some people believe that Everett eschewed all talk all splitting or branching observers in his original relative state formulation [2]. This is contradicted by the following extract from [2]: [...] Thus with each succeeding observation (or interaction), the observer state "branches" into a number of different states. Each branch represents a different outcome of the measurement and the *corresponding* eigenstate for the object- system state. All branches exist simultaneously in the superposition after any given sequence of observations.[#] The "trajectory" of the memory configuration of an observer performing a sequence of measurements is thus not a linear sequence of memory configurations, but a branching tree, with all possible outcomes existing simultaneously in a final superposition with various coefficients in the mathematical model. [...]

[#] Note added in proof-- In reply to a preprint of this article some correspondents have raised the question of the "transition from possible to actual," arguing that in "reality" there is-as our experience testifies-no such splitting of observers states, so that only one branch can ever actually exist. Since this point may occur to other readers the following is offered in explanation. The whole issue of the transition from "possible" to "actual" is taken care of in the theory in a very simple way- there is no such transition, nor is such a transition necessary for the theory to be in accord with our experience. From the viewpoint of the theory *all* elements of a superposition (all "branches") are "actual," none are any more "real" than the rest. It is unnecessary to suppose that all but one are somehow destroyed, since all separate elements of a superposition individually obey the wave equation with complete indifference to the presence or absence ("actuality" or not) of any other elements. This total lack of effect of one branch on another also implies that no observer will ever be aware of any "splitting" process. Arguments that the world picture presented by this theory is contradicted by experience, because we are unaware of any branching process, are like the criticism of the Copernican theory that the mobility of the earth as a real physical fact is incompatible with the common sense interpretation of nature because we feel no such motion. In both case the arguments fails when it is shown that the theory itself predicts that our experience will be what it in fact is. (In the Copernican case the addition of Newtonian physics was required to be able to show that the earth's inhabitants would be unaware of any motion of the earth.)

Q33 What unique predictions does many-worlds make?

A prediction occurs when a theory suggests new phenomena. Many-worlds makes at least three predictions, two of them unique: about linearity, (See "Is linearity exact?"), quantum gravity (See "Why *quantum* gravity?") and reversible quantum computers (See "Could we detect other Everett-worlds?").

Q35 Why *quantum* gravity?

Many-worlds makes a very definite prediction - gravity must be quantised, rather than exist as the purely classical background field of general relativity. Indeed, no one has conclusively directly detected (classical) gravity waves (as of 1994), although their existence has been indirectly observed in the slowing of the rotation of pulsars and binary systems. Some claims have been made for the detection of gravity waves from supernova explosions in our galaxy, but these are not generally accepted. Neither has anyone has directly observed gravitons, which are predicted by quantum gravity, presumably because of the weakness of the gravitational interaction. Their existence has been, and is, the subject of much speculation. Should, in the absence of any empirical evidence, gravity be quantised at all? Why not treat gravity as a classical force, so that quantum physics in the vicinity of a mass becomes quantum physics on a curved Riemannian background? According to many-worlds there *is* empirical evidence for quantising gravity.

To see why many-worlds predicts that gravity must be quantised, let's suppose that gravity is not quantised, but remains a classical force. If all the other worlds that many-worlds predicts exist then their gravitational presence should be detectable, in the sense that we would all share the same background gravitational metric with our co-existing quantum worlds. Some of these effects might be undetectable. For instance if all the parallel Earths shared the same gravitational field small perturbations in one Earth's orbit from the averaged background orbit across all the Everett-worlds would damp down, eventually, and remain undetectable.

However theories of galactic evolution would need considerable revisiting since, according to the latest cosmological models, the original density fluctuations derive from quantum fluctuations in the early universe, during the inflationary era. These quantum fluctuations lead to the formation of clusters and

super-clusters of galaxies, along with variations in the cosmic microwave background (detected by Smoots et al) which, therefore, vary in location from Everett-cosmos to cosmos. Such fluctuations could not grow to match the observed pattern if all the density perturbations across all the parallel Everett-cosmoses were gravitationally interacting. Stars would bind not only to the observed galaxies, but also to the host of unobserved galaxies.

A theory of classical gravity also breaks down at the scale of objects that are not bound together gravitationally. Henry Cavendish, in 1798, measured the torque produced by the gravitational force on two separated lead spheres suspended from a torsion fibre in his laboratory to determine the value of Newton's gravitational constant. Cavendish varied the positions of other, more massive lead spheres and noted how the torsion in the suspending fibre varied. Had the suspended lead spheres been gravitationally influenced by their neighbours, placed in different positions by parallel Henry Cavendishs in the parallel Everett-worlds, then the torsion would have been the averaged sum of all these contributions, which was not observed. In retrospect Cavendish established that the Everett-worlds are not detectable gravitationally. More recent experiments where the location of attracting masses was varied by a quantum random (radioactive) source have confirmed these findings. [W]

A shared gravitational field would also screw up geo-gravimetric surveys, which have successfully detected the presence of mountains, ores and other density fluctuations at the Earth's surface. Such surveys are not sensitive to the presence of the parallel Everett-Earths with different geological structures. Ergo the other worlds are not detectable gravitationally. That gravity must be quantised as a unique prediction of many-worlds.

[W] Louis Witten *Gravitation: an introduction to current research* New York, Wiley (1962). *Essays in honor of Louis Witten on his retirement. Topics on quantum gravity and beyond*: University of Cincinnati, USA, 3-4 April 1992 / editors, Freydoon Mansouri & Joseph J. Scanio. Singapore ; River Edge, NJ : World Scientific, c1993 ISBN 981021290

Q36 Is linearity exact?

Linearity (of the wavefunction) has been verified to hold true to better than 1 part in 10^27 [W]. If slight non-linear effects were ever discovered then the possibility of communication with, or travel to, the other worlds would be opened up. The existence of parallel Everett- worlds can be used to argue that physics must be *exactly* linear, that non-linear effects will never be detected. (See "Is physics linear" for more about linearity.)

The argument for exactness uses a version of the weak anthropic principle and proceeds thus: the exploitation of slight non-linear quantum effects could permit communication with and travel to the other Everett-worlds. A sufficiently advanced civilisation [F] might, therefore, colonise uninhabited other worlds, presumably in an exponentially spreading fashion. Since the course of evolution is built upon random quantum events (mutations, genetic recombination) and environmental effects (asteroidal induced mass extinctions, etc) it seems inevitable that in a minority, although still a great many, of these parallel worlds life on Earth has already evolved sapient-level intelligence and developed an advanced technology millions or even billions of years ago. Such early arrivals, under the usual pressure to expand, would spread across the parallel time tracks, displacing their less-evolved quantum neighbours.

The fossil record indicates that evolution, in our ancestral lineage, has proceeded at varying rates at different times. Periods of rapid development in complexity (eg the Cambrian explosion of 530 millions years ago or the quadrupling of brain size during the recent Ice Ages) are interspersed with long periods of much slower development. This indicates that we are not in the fast lane of evolution, where all the lucky breaks turned out just right for the early development of intelligence and technology. Ergo none of the more advanced civilisations that exist in other worlds have ever been able to cross from one quantum world to another and interrupt our long biological evolution.

The simplest explanation is that physics is sufficiently linear to prevent travel between Everett worlds. If technology is only bounded by physical law (the Feinberg principle [F]) then linearity would have to be exact.

[W] Steven Weinberg *Testing Quantum Mechanics* Annals of Physics Vol 194 #2 336-386 (1989) and *Dreams of a Final Theory* (1992)

[F] Gerald Feinberg. *Physics and Life Prolongation* Physics Today Vol 19 #11 45 (1966). "A good approximation for such [technological] predictions is to assume that everything will be accomplished that does not violate known fundamental laws of science as well as many things that do violate these laws."

Q37 Why don't worlds fuse, as well as split? Do splitting worlds imply irreversible physics?
This is really a question about why thermodynamics works and the origin of the "arrow of time", rather than about many-worlds. First, worlds almost never fuse, in the forward time direction, but often divide, because of the way we have defined them. (See "What is decoherence?", "When do worlds split?" and "When do worlds split?") The Planck- Boltzmann formula for the number of worlds (See "How many worlds are there?") implies that where worlds to fuse together then entropy would decrease, violating the second law of thermodynamics. Second, this does not imply that irreversible thermodynamics is incompatible with reversible (or nearly so) microphysics. The laws of physics are reversible (or CPT invariant, more precisely) and fully compatible with the irreversibility of thermodynamics, which is solely due to the boundary conditions (the state of universe at some chosen moment) imposed by the Big Bang. (See "Why can't the boundary conditions be updated to reflect my observations in this one world?")

Q38 Why can't the boundary conditions be updated to reflect my observations in this one world?
What is lost by this approach is a unique past assigned to each future. If you time-evolve the world-we-now-see backwards in time you get a superposition of earlier starting worlds. Similarly if you time evolve a single (initial) world forward you get a superposition of later (final) worlds.
For example consider a photon that hits a half-silvered mirror and turns into a superposition of a transmitted and a reflected photon. If we time-evolve one of these later states backwards we get not the original photon, but the original photon plus a "mirror image" of the original photon. (Try the calculation and see.) Only if we retain both the reflected and transmitted photons, with the correct relative phase, do we recover the single incoming photon when we time-reverse everything. (The mirror image contributions from both the final states have opposite signs and cancel out, when they are evolved backwards in time to before the reflection event.)
All the starting states have to have their relative phases coordinated or correlated just right (ie coherently) or else it doesn't work out. Needless to say the chances that the initial states should be arranged coherently just so that they yield the one final observed state are infinitesimal and in violation of observed thermodynamics, which states, in one form, that correlations only increase with time.

Q39 What is a relative state?
The relative state of something is the state that something is in, *conditional* upon, or relative to, the state of something else. What the heck does that mean? It means, amongst other things, that states in the same Everett-world are all states relative to each other. (See "Quantum mechanics and Dirac notation" for more details.)
Let's take the example of Schrodinger's cat and ask what is the relative state of the observer, after looking inside the box? The relative state of the observer (either "saw cat dead" or "saw cat alive") is conditional upon the state of the cat (either "dead" or "alive").
Another example: the relative state of the last name of the President of the Unites States, in 1995, is "Clinton". Relative to what? Relative to you and I, in this world. In some other worlds it will be "Bush", "Smith", etc ..... Each possibility is realised in some world and it is the relative state of the President's name, relative to the occupants of that world.
According to Everett almost all states are relative states. Only the state of the universal wavefunction is not relative but absolute.

A2 Quantum mechanics and Dirac notation
Note: this is a very inadequate guide. Read a more comprehensive text ASAP. For a more technical exposition of QM the reader is referred to the standard textbooks.
Richard P Feynman *QED: the strange story of light and matter* ISBN 0- 14-012505-1. (Requires almost no maths and is universally regarded as outstanding, despite being about quantum electrodynamics.)
Richard P Feynman *The Feynman Lectures in Physics* Volume III Addison- Wesley (1965) ISBN 0-201-02118-8-P. The other volumes are worth reading too!

Daniel T Gillespie *A Quantum Mechanics Primer: An Elementary Introduction to the Formal Theory of Non-relativistic Quantum Mechanics* (Takes an axiomatic, geometric approach and teaches all the Hilbert space stuff entirely by analogy with Euclidean vector spaces. Not sure if it is still in print.)

Quantum theory is the most successful theory of physics and chemistry ever. It accounts for a wide range of phenomena from black body radiation, atomic structure and chemistry, which were very puzzling before quantum mechanics was first developed (c1926) in its modern form. All theories of physics are quantum physics, with whole new fields, like the semiconductor and microchip technology, based upon the quantum effects. This FAQ assumes familiarity with the basics of quantum theory and with the associated "paradoxes" of wave-particle duality. It will not explain the uncertainty principle or delve into the significance of non-commuting matrix operators. Only those elements of quantum theory necessary for an understanding of many-worlds are covered here.

Quantum theory contains, as a central object, an abstract mathematical entity called the "wavefunction" or "state vector". Determining the equations that describe its form and evolution with time is an unfinished part of fundamental theoretical physics. Presently we only have approximations to some "correct" set of equations, often referred to whimsically as the Theory of Everything.

The wavefunction, in bracket or Dirac notation, is written as |symbol>, where "symbol" labels the object. A dog, for example, might be represented as |dog>.

A general object, labelled "psi" by convention, is represented as |psi> and called a "ket". Objects called "bra"s, written <psi|, may be formed from kets. An arbitrary bra <psi'| and ket |psi> may be combined together to form the bracket, <psi'|psi>, or inner product, which is just a fancy way of constructing a complex number. Amongst the properties of the inner product is:
<psi'|(|psi1>*a_1 + |psi2>*a_2) = <psi'|psi1>*a_1 + <psi'|psi2>*a_2
where the a_i are arbitrary complex numbers. This is what is meant by saying that the inner product is linear on the right or ket side. It is made linear on the left-hand or bra side by defining
<psi|psi'> = complex conjugate of <psi'|psi>
Any ket may be expanded as:
|psi> = sum |i>*<i|psi> i = |1>*<1|psi> + |2>*<2|psi> + ...
where the states |i> form an orthonormal basis, with <i|j> = 1 for i = j and = 0 otherwise, and where i labels some parameter of the object (like position or momentum).

The probability amplitudes, <i|psi>, are complex numbers. It is empirically observed, first noted by Max Born and afterwards called the Born interpretation, that their magnitudes squared represent the probability that, upon observation, that the value of the parameter, labelled by i, will be observed if the system is the state represented by |psi>. It is also empirically observed that after observing the system in state |i> that we can henceforth replace the old value of the wavefunction, |psi>, with the observed value, |i>. This replacement is known as the collapse of the wavefunction and is the source of much philosophical controversy. Somehow the act of measurement has selected out one of the components. This is known as the measurement problem and it was this phenomenon that Everett addressed.
When a bra, <psi|, is formed from a ket, |psi>, and both are inner productted together the result, <psi|psi>, is a non-negative real number, called the norm of the vector. The norm of a vector provides a basis-independent way of measuring the "volume" of the vector.

The wavefunction for a joint system is built out of products of the components from the individual subsystems.

For example if the two systems composing the joint system are a cat and a dog, each of which may be in two states, alive or dead, and the state of the cat and the dog were *independent* of each other then we could write the total wavefunction as a product of terms. If
|cat> = |cat alive> * c_a + |cat dead> * c_d
and

|dog> = |dog alive> * d_a + |dog dead> * d_d
then
|dog+cat> = |cat>x|dog>
where
x = tensor product = (|cat alive> * c_a + |cat dead> * c_d) x (|dog alive> * d_a + |dog dead> * d_d) =
|cat alive> x |dog alive> * c_a * d_a + |cat alive> x |dog dead> * c_a * d_d + |cat dead> x |dog alive> *
c_d * d_a + |cat dead> x |dog dead> * c_d * d_d =
|cat alive, dog alive> * c_a * d_a + |cat alive, dog dead> * c_a * d_d + |cat dead, dog alive> * c_d * d_a +
|cat dead, dog dead> * c_d * d_d

More generally, though, we states of subsystems are not independent of each other we have to use a more general formula:
|dog+cat> = |cat alive, dog alive> * a_1 + |cat alive, dog dead> * a_2 + |cat dead, dog alive> * a_3 + |cat dead, dog dead> * a_4

This is sometimes described by saying that the states of the cat and dog have become entangled. It is fairly trivial to define the state of the cat and the dog with respect to each other. For instance we could re-express the above expansion with respect to the cat's two states as:
|dog+cat> = |cat alive>x(|dog alive> * a_1 + |dog dead> * a_2) + |cat dead>x(|dog alive> * a_3 + |dog dead> * a_4)

We term the state of the dog the *relative state* (Everett invented this terminology) with respect to the cat, specifying which cat state (alive or dead) we are interested in. This thus the dog's relative state with respect to the cat alive state is:
(|dog alive> * a_1 + |dog dead> * a_2)/sqrt(|a_1|^2 + |a_2|^2)
where the sqrt term has been added to normalise the relative state.

A Classical Approach to Newcomb's Paradox

by Robert Low

In a recent Noesis, Chris Langan commented that he had provided a resolution of Newcomb's paradox involving a new concept: I'd like to present a discussion purely in terms of standard ideas, just for comparison.

First, let's recall the nature of the paradox.

A being who has been extremely good at predicting your behaviour to date has placed 1,000 dollars in box A. In box B, he has placed 1,000,000 if he predicts that you will open just box B, and nothing at all if he predicts that you will open both boxes.

Argument for opening just box B: judging by past experience, the being will correctly predict my actions. If I open just box B, I will almost certainly get 1,000,000 dollars; if I open both boxes, I will equally certainly get just 1,000.

Argument for opening both boxes: the money is already there. If I open box A and B, I get either 1,000,000 dollars or 1,001,000 dollars, depending on the contents of B. In either case, I get more money than if I just open B. So I should open both.

I don't think that the latter argument holds water, because it does not take into account the fact that a perfect predictor will foresee that argument being used. However, an analysis in these terms gets us into swampy "I know that you know that I know that...." territory (and the 'Princess Bride' solution is unavailable). So, to cut through that Gordian knot, I shall wield the sword of probability.

Let us suppose that the predictor is correct with probability P; and by this I mean that whatever action I take, the predictor predicted (with probability P) that I would do that.

Case 1: I open just box B. Then the amount of money I get is 1,000,000 dollars with probability P, and 0 dollars with probability 1-P. My expected amount of money is therefore P*1,000,000

Case 2: I open both boxes. Then my expected amount is 1,000 + (1-P)*1,000,000.

I want to maximise the amount of money I expect to get. I therefore want to choose the larger of P* 1,000,000 and 1,000+(1-P)*1,000,000. Now,

P*1,000,000 > 1,000 + (1-P)*1,000,000

is (by elementary algebra) equivalent to

P > 0.5005

so that if I believe my actions are going to be predicted correctly with a probability significantly greater than 0.5, I should open only box B---since the predictor is assumed to be very reliable, box B is the rational choice *if I wish to maximise my expected amount*.

There is, however, still a rational argument for opening both boxes. The above argument gives the case for the rational choice of opening B only, if one wishes to maximise one's *expected* amount of money. If, on the other hand, one wishes to maximise the *minimum* amount of money obtained, it is rational to open both boxes. The reason is simple: there is a small, but finite probability that the being's prediction will be incorrect. If this is the case, then by opening box B I may conceivably get no money at all. By opening both boxes, I get at least 1,000 dollars. Hence, if I wish to maximise the minimum amount of money I can get, rather than maximising the expected amount, I should open both boxes.

The fact that I have rational reasons for each choice is now simply a reflection of the fact that I have two different bases from which to reason, and provides no paradox whatever.

--- Robert Low, email(JANET): RobLow@cov.ac.uk Home Address: 1A Stoney Road, Cheylesmore, Coventry CV1 2NP, England "A foolish consistency is the hobgoblin of little minds." - Emerson.

Comments on the Society
by Chris Cole

There has been considerable controversy over the editing of *Noesis*. As the publisher, I have even been asked by one member to switch editors. I am not going to do that, and here is why.

My biggest concern is that the Mega Society not become embroiled in the kind of petty politics that frequently destroy organizations. I want peace and tolerance amongst the members. And I know that the first requirement of peace and tolerance is to refrain completely from any exercise of unilateral decision-making. For example, even the relatively benign suggestion that members must submit ten pages of material has been attacked by Richard May and Kevin Langdon, and I beat a hasty retreat. So, anything as authoritarian as unilaterally switching editors is completely out.

I'd like to discuss what I've learned that *Noesis* is (as opposed to what I wanted it to be). What *Noesis* is is a reflection of what the Mega Society is, and the Mega Society consists of people with extraordinary mental gifts, many of whom have not succeeded in life even by their own standards. There are many reasons for this; perhaps the most common is summed up by the saying, "the nail that stands out gets hammered down." As a result, material in *Noesis* is brilliant, idiosyncratic, and bitter.

I had hoped that the Mega Society would be a forum for changing the world. This now seems hopelessly optimistic, as indeed many of you realized from the start. My ambitions were based on the hope that people capable of passing the Mega Test would have much in common. While it seems that we do have much in common, it is clear to me now that we do not have enough in common to speak with a single voice.

Why is this? Well, first of all, *de gustibus non est disputandum* -- there is no disputing about tastes. While we all may have been born with roughly equivalent gifts, our histories are radically different. These different histories have bred different demons. Some of these demons seem so overwhelming that fighting them consumes all of our energy, indeed, it seems that for some of us the possibility of losing to the demon is so painful that the world must be reorganized to make that impossible.

One demon that we all fight is the need to be considered smart, indeed, to be a "genius." I can postulate that we all share this need because of a selection bias: we all spent the enormous effort required to pass the Mega Test (or an equivalent). This is a very tawdry kind of need, and I am embarrassed by it personally. I seek to suppress it within myself. I don't know why I have it and I wish it would go away. I have talked to one subscriber to this journal who is so obsessed by this need that he cannot get a job because he cannot pass up an opportunity during a job interview to explain that he is a genius.

Noesis is a means for bettering our minds and solving hard problems. Sometimes these are the same thing.

In closing, I'd like to relate two incidents from Feynman's life that may be relevant. When he was a child, Feynman discovered trigonometric identities before he had a course in trigonometry in school. He developed his own notation to express these identities, and as one might expect his notation was much more concise and sensible than the usual "sin," "cos," and "tan" of historical accident. Nonetheless, when he eventually was taught trigonometry, he realized that in order to communicate his ideas, he was going to have to adopt the ugly historical notation. So, regretfully, he put away his childish things.

The second incident occurred when Feynman was a graduate student. By this time he had invented the perturbation expansion of the equations of quantum electrodynamics that later became called Feynman diagrams and for which he shared the Nobel Prize. However, no one other than Feynman knew of this discovery, and when Feynman tried to explain it to others (many of whom were people of equal and greater intellect to his own), he encountered only blank stares. This is because these people were busy with their own theories. So, in desperation, he asked people what they were working on. They told him. He went away and came back the next day with an answer that, in many cases, had taken these people months to calculate. This got people's attention and, as they say, "the rest is history."