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Ask Marilyn

BY MARILYN VOS SAVANT

You have followed in the footsteps of an ignorant algebra teacher.

Michelle in Wisconsin recently wrote: "A woman and a man (who are unrelated) each have two children. At least one of the woman's children is a boy, and the man's older child is a boy. Can you explain why the chances that the woman has two boys do not equal the chances that the man has two boys? My algebra teacher insists the probability is greater that the man has two boys, but I think the chances may be the same." You agreed with the algebra teacher.

This illustrates one of my favorite theories. I submit that woman's intuition—used successfully by Michelle—is often a process of subconscious reasoning that bypasses the right-brained analysis that is typical of male argument. I love your column; I also love to argue about it.
—Russell Bell, Palisade, Colo.

Then you'll love my reply. What selective logic! Because you believe the reader is correct, you assume she used woman's intuition to arrive at a correct answer, that I used male right-brained analysis to arrive at an incorrect answer (or that my woman's intuition took a powder) and that the algebra teacher is a typical wrong-headed male (or a woman who didn't use her woman's intuition). But you are not alone in disagreeing:

If the first child is a boy, will the second child be one too? Readers argue about logic and common sense.

For you to come up with an answer contradicting common sense suggests that you approached this problem by trying to back up the algebra teacher. It is often easier to explain a wrong answer than to find the right one. But I find an attitude in which the voice of authority outweighs common sense troubling.
—David Wetta, Norman, Okla.

It matters not whether the boy was firstborn or lastborn. Each parent had one additional child. The probability that that child is male or female has no relationship to other things mentioned. That probability is a fact of nature. Your statement is nonsense. You've disappointed many of us in a way only an apology can cure.
—Phil Wallace Payne, Lompoc, Calif.

I have a BA from Harvard, an MBA from the University of Pennsylvania Wharton School, a math SAT score of 800, and a perfect score in the Glazer-Watson critical thinking test, but I'm willing to admit I make mistakes. I hope you will have the strength of character to review your answer to this problem and admit that even a math teacher and the person with the highest IQ in the world can make a mistake from time to time.
—John Francis, Newton, Mass.

I do make mistakes from time to time—which I always announce—but this isn't one of those times! The original answer is correct. Here's further explanation:

Families with two children can be distributed in the following ways. Each way is equally likely.

1) older = boy, younger = girl
2) older = girl, younger = boy
3) older = boy, younger = boy
4) older = girl, younger = girl

In real life, 50% of these families (the first two groups) have a child of each sex, 25% (the third group) have two boys, and 25% (the fourth group) have two girls. The woman in question—with at least one boy—must be in one of the first three groups. But only one of those three groups (No. 3) has another boy. So the chances that she has another boy are 1 in 3. It's different for the man whose older child is a boy. He must be in one of only two groups—the first or the third. One of those two groups (No. 3) has another boy. So the chances that he has another boy are 1 in 2.

The answer was "The Rings of Saturn." What's the question?

"What could the best man not find on the Roman god's wedding day?"
—Joe Sinkey, Athens, Ga.

"What do you count to learn the age of the sixth planet from the Sun?"
—Jill Roberts, Tampa, Fla.

"What preceded the invention of the answering machines of Saturn?"
—George McCahey, Carol Stream, Ill.

"What were left in the tub after the sixth planet took too many long meteor showers?"—Mike D., Staten Island, N.Y.

Ready to try another one?
The answer is: "Unidentified Flying Objects."

If you have a question for Marilyn vos Savant, who is listed in the "Guinness Book of World Records" Hall of Fame for "Highest IQ," send it to: Ask Marilyn, PARADE, 711 Third Ave., New York, N.Y. 10017. Because of volume of mail, personal replies are not possible.

A PARADOX OF PROBABILITY THEORY (copyright 1996 by Chris Langan)

The following problem, posted by a reader, appeared in the "Ask Marilyn" column of the Sunday, December 1, 1996 edition of *Newsday Parade*. Before proceeding, I would like to say that Marilyn's performance as a columnist is generally sterling, and that what follows is in no way be interpreted as a depreciation of her acute intelligence. Her solution is ultimately correct; it falls short only in depth of explanation, a fact for which editorial limitations are no doubt partly responsible.

"A woman and a man, who are unrelated, each have two children. At least one of the woman's children is a boy, and the man's older child is a boy. Can you explain why the chances that the woman has two boys do not equal the chances that the man has two boys? My algebra teacher insists the probability is greater that the man has two boys, but I think the chances may be the same."

Marilyn vos Savant's Reply

In a previous column, Marilyn vos Savant, Mega member and part-time columnist, had explained why she agreed with the algebra teacher. In the face of vehement criticism, she justifies her position as follows.

Two-child families can be distributed in the following equiprobable ways:

Table 1a:

(group)	Older	Younger
1.	boy	girl
2.	girl	boy
3.	boy	boy
4.	girl	girl

Table 1b:

	Boy	Girl
1.	older	younger
2.	younger	older
3.	both	---
4.	---	both

Tables 1a and 1b are identical in content.

The woman's family must be in group 1, 2, or 3, only one of which (3) has two boys. So her chances of having two boys are 1 in 3. But the man's family, in which the elder child is known to be a boy, must be in group 1 or 3. So his chances of having two boys are 1 in 2. Thus, the probability that the man has two boys (1/2) is greater than the analogous probability for the woman (1/3).

Preliminary Remarks

The fact that Marilyn calculates two different probabilities for the man and the woman seems to indicate a false belief that the sex of a child is somehow dependent on its birth order. This, at any rate, seems to be the main point of her critics, who may be assumed to reason as follows:

1. Given that the man's elder child is a boy, the probability that he has two boys is identical to the probability that his younger child is a boy. But the sex of a younger child cannot be affected by that of the elder; it is 1/2 in any case.

2. Given that at least one of the woman's children is a boy, the probability that she has two boys is identical to the probability that her other child is a boy. Again, the sex of a child is not affected by that of its sibling. So again, the probability is 1/2.

In other words, Marilyn's critics interpret the problem like this. Let there be a deck of four cards BB, BG, GB, and GG, each of whose two faces is marked with a B or a G (for boy or girl). Suppose as well that one face of each card is marked with an O and the other with a Y (for older or younger). E.g., the face reading "B/O" corresponds to "boy, older". Now suppose that someone puts tape over all of the O's and Y's, and deals you a card whose visible face reads "B/(tape)". What is the probability that the underside of this card is also marked with a B?

There are four ways that this face could have been dealt to you. It is either the O or Y face of the BB card, or it is the B face of either the BG or the GB card. Since two of these ways correspond to the BB card, your chances are 2 in 4 = 1/2.

Now suppose that the dealer removes all of his tape, shuffles, and deals you a card whose visible face reads "B/O". This time there are two ways that this face could have been dealt to you. It is the B/O face of either the B/O-B/Y card or the B/O-G/Y card. Since one of these ways corresponds to the BB card, your chances are again 1/2.

While Marilyn has adopted this model with respect to the man's family, she seems to have rejected it with respect to the woman's. In her view, no card has yet been dealt in the woman's case, at least not in such a way that a particular face is showing. Instead, one card - the one marked GG - has been deliberately eliminated from the deck, and you have subsequently been asked to randomly pick one of the three remaining cards. Thus, your chances of selecting the BB card are 1/3 (note that in the real universe of 2-child families, the "GG card" cannot logically be eliminated without examining one or both of the children in every family in the sample space).

In fact, the initial information in this problem is consistent with either interpretation. Because both Marilyn and her critics are right, a paradox exists. The *Parade* article, which essentially consists of nothing but stripped-down versions of these arguments, does little to resolve it. Because I personally got sucked in by this paradox - I went from agreeing with Marilyn to agreeing with her critics, and then unhappily back and forth, very nearly rushing prematurely into print with a lopsided analysis - it falls on me to resolve it. As it turns out, the paradox is as much a matter of wording as of mathematics. But then again, probability theory can be extremely deceptive on both counts, and it pays to resolve these paradoxes as they arise.

The Resolution of "Marilyn's Paradox"

First, let's establish a simple notation.

b: the event that one child of a given 2-child family is identified as a boy
g: the event that one child of a given 2-child family is identified as a girl
b_o: the event that the elder child of a given 2-child family is identified as a boy
b_y: the event that the younger child of a given 2-child family is identified as a boy
bb: the event that both children of a given 2-child family are found to be boys
not-gg: the event that group 4 in Table 1 has been eliminated as a possible description of a given 2-child family.
P(B|A): the probability that event B will occur given event A (conditional probability)
P(Z|...|A): the probability of event Z given a cumulative sequence of events from A to Z (where these chains "branch" at sets of exhaustive and disjunctive possible events, their values sum to P(A), the total probability of A)

The paradox may now be traced to the following cause:

In the woman's case, Marilyn has computed P(bb|not-gg); her critics have instead computed P(bb|b). The fact that these conditional probabilities differ is clearly attributable to the inequivalence of b and not-gg. In other words, the elimination of group 4 from Marilyn's tabulation of possibilities, while it may indeed imply that "at least one of the woman's children is a boy", does not imply that one of her children has been specifically identified as a boy *by the one doing the computing.*

This is a rather counterintuitive fact. How can we know that group 4 (g,g) has been eliminated as a possible description of the woman's family unless one of her children has been conclusively identified as a boy? Well, in any of several artifical and elaborate, but nonetheless possible, ways. For instance, we can feed census data into a computer, have the computer eliminate all (g,g) families, take the woman's family from those remaining, and erase the computer's memory to try to conceal the fact that somebody or something must have individually examined at least one of her children in order to gather the data in the first place. The point is that while a specific boy must at some point have been identified, the identification was made by someone or something *else*. As far as we are concerned, the information is "not-gg", period.

The difference between b and not-gg becomes evident when we consider that after (g,g) has been eliminated as a possible description of the woman's family, a randomly selected child still has a 1/3 chance of being a girl. That is, P(b|not-gg) = 2/3. Applied to the above anti-Marilyn argument, it changes P(bb|b) into

E.1 $$P(bb|b|\text{not-}gg) = P(bb|b) \times P(b|\text{not-}gg) = 1/2 \times 2/3 = 1/3,$$

and lowers the probability to 1/3 as required.

The independence of sex and birth order is reflected elsewhere. If we compute the probability that the man's family contains 2 boys given that both of the events not-gg and b_o have occurred, we get

E.2 $\quad P(bb|b_o|\text{not-gg}) = P(bb|b_o) \times P(b_o|\text{not-gg}) = 1/2 \times 1/3 = 1/6,$

i.e., only half the probability of P(bb|not-gg) = 1/3. This is because the chain of conditional probabilities has "branched" at the exhaustive and disjunctive set of possibilities $b = (b_o \text{ or } b_y)$, and

E.3 $\quad P(bb|b|\text{not-gg}) = P(bb|b_o|\text{not-gg}) + P(bb|b_y|\text{not-gg}) = 1/6 + 1/6 = 1/3,$

again as required.

The above probability, of course, is conditional. When all initial information is ignored, the man's and woman's probabilities are unconditional and again equal:

E.4 $\quad P(bb) = P(bb|\text{Table 1}) = 1/4$

or, where P(not-gg) = 1 - P(gg) = 1- 1/4 = 3/4,

E.5 $\quad P(bb) = P(bb|b|\text{not-gg}) \times P(\text{not-gg}) = 1/3 \times 3/4 = 1/4.$

So much for the first layer of the paradox. The next involves linguistic ambiguity stemming from the fact that when the event "not-gg" is reworded as "at least one child is a boy", specific reference has been made to "one child" and event b has thus been implied. That is, for the phrase "one child" to be semantically meaningful, it must correspond to a real child with a specific identity, and this specific child seems to be identified as a boy. But while a specific identification may indeed have been necessary to establish not-gg, the identificative event had a subunary probability lower than that of not-gg itself (indeed, this is what allowed not-gg to be inferred from it).

In the "2-boys problem", it seems that the original provider of information possesses more information than he has imparted. On the one hand, he may have deliberately omitted information to the effect that he has intentionally examined both of the woman's children and already knows their sexes. In this case, the omitted information cannot be reconstructed from the information actually imparted. But even if he has not purposely "lied by omission", he has expressed himself in an ambiguous and misleading way by referring selectively to "one child" and imparting partial information, not-gg, by which further information, b, is logically implied

Thus, logic is interfering with probability. Conventional probabilistic reasoning tells us that since any random act of identification subsequent to not-gg has a 1/3 chance of revealing a girl, P(b|not-gg) = 2/3. But logical reasoning tells us that if the woman's family is known not to consist of two girls, then one of her children must have been specifically identified as a boy: P(b|not-gg) = 1. From this viewpoint, it appears that not-gg was initially established for the woman by means of an act of identification, equivalent to randomly drawing a boy from Table 1a, of probability P(b) = 1/2. In effect, this defines two separate conditional probabilities, each to some extent justified by the initial information pertaining to the woman's case.

To reconcile these distinct conditional probabilities , we must go from the intuitive idea of "probability" to probability *measure* and invert equation E.1 as follows:

R.1 P(bb|b|not-gg) --> P(bb|not-gg|b), where

E.6 P(bb|not-gg|b) = P(bb|not-gg) x P(not-gg|b) = 1/3 x 3/2 = 1/2, and

E.7 P(bb) = P(bb|not-gg|b) x P(b) = 1/2 x 1/2 = 1/4

R.1 is a peculiar inversion. For one thing, it seems to violate the rules of deduction by making not-gg "conditional" on something of lesser generality, namely b. This violates conventional probability theory on a very basic level by generating a "probability" in excess of 1, P(not-gg|b) = 3/2 (to be read "the measure of not-gg in b is 3/2"). For another, it does not account for the initial probability of drawing a girl. After all, since the implication "not-gg" assumedly did not exist prior to b, a girl was just as likely to have been drawn: P(g) = P(b) = 1/2. To explain this, we simply observe that the chain

E.8 P(bb|not-gg|g) = P(bb|not gg) x P(not-gg|g) = 1/3 x 0 = 0

is logically "self-aborting", and therefore cannot interfere with E.6 or E.7.

Thus, while Marilyn computed the woman's probability as in E.1, others - who have logically reconstructed the identification of one of the woman's children as a boy - prefer to compute their probability as in E.6. Both are "right" within a certain interpretation of the initial information which has been provided. If "at least one child is a boy" is interpreted as coming from a random identificative event, the critics are right. If it is interpreted as coming from a deliberate examination of one or both children with subsequent loss or concealment of up to half of the relevant information, then Marilyn is right. Marilyn's computation has the important advantage of following directly from the more general of the two events b and not-gg, but has the cosmetic disadvantage of seeming at first glance to violate the independence of sex and birth order.

The resolution of this paradox resides in the reconciliation of equations E.1 and E.6, which describe two distinct conditional probabilities, by the numerical equivalence of E.5 and E.7, which describe convergent unconditional extensions of E.1 and E.6. This amounts to a "relativization" of likelihood to two distinct conditional probability chains corresponding to two distinct, but justifiable, interpretations of initial information, and its subsequent "absolutization" by regressively extending the chains until they reach a common numerical value (1/4).

The true depth of the paradox is now obvious. While initial information is generally treated as "given" in the computation of conditional probabilities, its acquisition actually has a nonunary probability, and this probability must be accounted for in any probabilistic computation. But in problems like the one at hand, it seems to vanish in the wording. It is inadvertently lost in the act of communication between providers and recipients of initial information, only to be *logically* reconstructed without regard for its "measure" in the set of possibilities which has been literally provided. Moreover, when its measure is accounted for, superunary probabilities may be generated.

Before leaving this section, it may be worthwhile to mention an interesting extension of Table 1a which applies where initial information is interpreted as "randomly dealt".

Table 2:	Older	Younger	
1.	b_k	g_u	Information common to both the man's and woman's
2.	b_u	g_k	cases, P(one child male) = 1, narrows this subjectively
3.	g_k	b_u	symmetrized table to groups 1, 4, 5 and 6. Birth-order
4.	g_u	b_k	information then eliminates groups 4 and 6 in the man's
5.	b_k	b_u	case, equalizing his and her chances at 1 in 2 and 2 in
6.	b_u	b_k	4 respectively. These conditional probabilities can be
7.	g_k	g_u	reconciled with others by accounting for the probability
8.	g_u	g_k	measures of different knowledge configurations.

Table 2 can be regarded as a "multiplication" of Table 1a by the subjective distinction "known, unknown" in the form of indices k and u. Concisely, a problem caused by an asymmetry in subjective knowledge of ordinality has been obviated by symmetrizing the ordinal knowledge embodied in the table. This approach to subjective probability - i.e., explicit allowance in the table of initial possibilities for the cognitive states of subjects - constitutes a *metalinguistic* identification of an "object language", that of subjective cognition, with its "object universe", that of the real 2-child families to which it refers.

A Related Controversy

The foregoing paradox involves some thorny logical issues. One is reference to a set in terms of an element; the description of a whole family in terms of one child requires an *existential quantifier* whose translation into and out of ordinary language can be tricky. In the 2-boys problem, initial information about the woman's family can be interpreted in either this way, or a more conventional way in which one specific child is described. Another (related) issue involves the fact that each interpretation implies a different distribution of 2-child familes, and without knowing or assuming the distribution, we cannot *a priori* determine a probability "conditional" upon it.

This is not the first time that mention of the latter issue has been made in *Noesis*. As recently as *Noesis*123, Chris Cole mentioned a controversial Ronald Hoeflin test problem involving a boxed set of 10 marbles from which sampling with replacement has yielded 10 white marbles in a row: what is the probability that all 10 marbles in the box are white? It was Chris's contention that the problem is insoluble without information on how the set was constructed. Concisely, he maintained - indeed, has maintained for the past six or seven years - that the table of possibilities used to initialize Bayesian inference regarding this problem requires information prior to the trials themselves. Although he has never gone into much detail regarding his thesis, he seems to feel that the frequency with which a white marble is selected depends on the specific colors of the other marbles in the box, or at least on the way marbles were selected for insertion.

That's hogwash, but my meager literary and mathematical talent has not sufficed to convince anybody. So without further ado, I present that comic book superhero of the Big Top, the Undisputed Don of Con, the All-Gotham Master of Mirth, the incomparable *Jojo Einsteiiiin!!!* (Naturally, any resemblance to real persons living or dead is strictly coincidental. We rejoin Jojo precisely where we left off in the last episode.)

More News from Times Square

Staring balefully at the Hi-Q snotrag at his feet, Jojo reflected on the hard facts of life. Here on the mean streets of New York City, certain lessons couldn't be ducked. The kind of lesson that a surf-happy California boy had trouble fitting in between umbrella cocktails, beach bunnies, and UV overdoses. For instance, you learned to tell an Uzi from a squirtgun, and a hard rock from a gasbag. A fair split was always better than zip-a-dee-doodah, and if a guy was the real deal, it was always smarter to cooperate. People had learned that about Jojo as soon as he had hit the bricks! But if you were gonna be a hard-nose, a *tough guy*...well, then, if the dude whose size 33-Z floppies you were parking on was the genuine article, you better count on a *problem*. A problem who, as lesson one of a refresher course on "sharing", was willing to part free of charge with a big hunk of hurt feelings in your direction. And you were lucky if that was all!

So what on earth was anybody doing pulling *Chris Langan's* chain, when everybody and his pooch knew that he was under the personal protection of the biggest, meanest street clown anybody ever saw? It made so little sense that Jojo's outsized head, red rubber nose, and curly orange wig started to ache in unison.

The clown was already wishing that he were back inside the Bijou, gorging on day-old popcorn and watching a certain kult klassic for the hundred-and-first time, when a most dashing figure caught his eye. Slim and devilishly handsome, the stranger approached like a male supermodel on a spotlit Armani runway, moving with the athletic grace of a panther on DHEA. In his magnetic eyes was an unmistakable glint of preternatural intelligence that one could only call...well, *raptorial*, for want of a better word. What but a killing dive could have brought such a magnificent creature so far below his lofty penthouse aerie? Except maybe the weight of that cash-packed wallet that Jojo's x-ray vision had already discerned in the hip pocket of his razor-creased slacks?

By sheer coincidence, it was at that very moment that the elegant stranger glimpsed what appeared to be a crisp new $100 bill on the sidewalk just ahead of him. Small change, really, but at least it would almost cover lunch. Distractedly, he bent down to retrieve it. But just as it was about to stick of its own free will to his beautifully manicured fingertips, it was caught by a stray breeze instead! That was funny; the wind hadn't stirred all day. Annoyed, the stranger advanced to its new location and stooped down a second time. But again, the wayward greenback fluttered away just as he was about to take possession! With rising anger and embarrassment, he leaped boldly after it...and very nearly followed it right up the hairy, paisley-clad leg of some tasteless character with enormous feet and an ultralight fishing reel in his white-gloved hands.

"Just as I suspected," said the clown in his best W.C. Fields twang, dropping the fishing reel into a convenient pocket. "A gentleman who knows the value of money!"

The stranger couldn't believe it. There in front of him, like a spontaneous free-form delusion come to life, was what appeared to be a cross between Howdy Doody and a death-metal rock star. And even worse, its tailor should have been shot!

He eyed Jojo as though being forced to examine a recently squashed garden slug. "Is this supposed to be some kind of *joke?*", he grated.

"Not a joke, kind sir, but a wager", earnestly explained the clown, positioning himself so as to impede the stranger's further progress. "A wager I daresay is right up your alley!"

The stranger's lip curled in a sneer. "And what could an idiot like <u>you</u> possibly have that someone like <u>me</u> might want?" This was not the kind of guy you just *interrupted*.

"Well," answered Jojo, reverting to his native Hell's Kitchen subdialect of Brooklynese, "to start with, there's that C-note you were just trying to snag." In fact, the hundred was so bogus that it might as well have been a 3-spot. "Or should I say 'rescue'?" You could never underestimate the value of a little well-placed verbal delicacy.

The stranger snorted disdainfully. "Well...as an *appetizer*, perhaps." The flawless diamond in his Harvard University tiepin glinted brightly, telegraphing the ease with which he could up the ante until the poverty-stricken clown cried uncle. Law, stocks, corporate software...who knew what kind of lucrative bean-counting this guy was into?

"Tip of the iceberg," lied the clown. "But first things first, so lemme explain the wager." From the inner recesses of his jumpsuit, he extracted a box. "In this box are 4 marbles. 1 of them is white. The rest of them are nonwhite; just to keep things simple, I've made 'em gray. First you shut your eyes and randomly pick a marble out of the box. Then you examine and replace it. If you do that say 100 times, how many times will you come up with the white marble?" Jojo proferred the box. "Here, I'll even let you *try* it a few times."

"You fool," scoffed the stranger. "I don't have to pick a single marble to tell you the answer to *that*. Since at any time 1 of the 4 marbles in the box is white, the chance for each trial is 1/4. So I'd pick the white marble approximately 100 x 1/4 = 25 times. If anything else were to happen, I'd know that you were nothing but a cheap street crook. Which, I add, wouldn't *surprise* me. Now reel my money out of those awful pajamas."

"Gee Willikers!" gushed Jojo admiringly. "You're *good*. But unfortunately, that wasn't the bet. Say I take all 3 gray marbles out of the box, paint them different nonwhite colors, and put 'em back. *Now* how many times will you come up with a white marble?"

"What an incredible moron!" marveled the stranger out loud, unconsciously fingering his tiepin as if to maintain his socioeconomic distance from this greasepainted dunce. "You can paint until the cows come home! The answer is still approximately 25."

Jojo smiled imperceptibly. "You don't say! Now why is *that?"*

The stranger shook his head in disbelief. "Because, you dolt, there's still just 1 white marble in the box, regardless of what specific nonwhite colors you painted the others."

"How right you are!" congratulated the clown. "But sad to say, that still ain't the bet! Now suppose we visit a certain marble manufacturer, give him a few gross of empty boxes, and instruct him to put 4 marbles in each box according to a secret rule or set of rules that he gets to choose. The rule or rules in question simply say how marbles are to be selected for any given box or group of boxes...for example, that they're to be taken randomly from a grab-bag containing a certain number of marbles of each of a certain number of specific colors, including white. You follow?"

The stranger sniffed, nodded curtly, and glanced pointedly at his chunky gold Rolex Oyster wristwatch. Jojo's nimble fingers tingled with temptation.

"OK, *here's the bet*. Suppose we take one box at random and you sample marbles out of it, just like before. Say you come up with a white marble about 25 times out of 100."

Jojo paused dramatically. *"Now, how many white marbles are in the box?"*

An exultant smirk bloomed on the stranger's lips. If it wasn't for winning, some people would have no luck at all, heh heh! "To a high degree of certainty, the answer is 1!"

The clown dropped his jaw like an anchor, scratching his wig in evident befuddlement (where was the Denorex when you needed it?). "But...but how can you know *that?* I mean, don't you have to know the exact rule by which the box was *filled?"*

"Of course not!" snapped the stranger impatiently. "Since the frequency of white trials is 1/4, 1 out of 4 marbles must be white! The frequency equals the proportion. So pay up!"

Jojo, seemingly on the verge of tears, gulped air like a boated fish. "But...but...then you're saying that the relationship between frequency and proportion doesn't *depend* on the rule? That the only things directly affecting the frequency of white observations are the numbers of white and nonwhite marbles actually *in the box*, and not the color-specific process by which the box was originally filled?" His rubbery mug contorted spastically as he feigned a pea-brained inability to absorb this terrible realization.

"*Right*, genius." Never had so smug a face begged so needily for a banana cream pie.

The clown grinned wolfishly. "Thanks, pal! That's all I wanted to know." And then he simply vanished, leaving not so much as an afterimage (or gold Rolex) behind him.

And Now, Back to our Host

Where q is a quality applicable to the individual elements of a set, Jojo has just shown that the frequency of q in random observations depends only on the numbers of objects with (exhaustive and disjunctive) qualities q and not-q in the closed and finite set being observed. Where q is quantified by the number of objects it describes, the frequency of q in repetitive trials converges, by the law of large numbers, on the measure of q in the sample space - i.e., to the ratio of |q| to (|q| + |not-q|) (where |q| and |not-q| denote the numbers of q objects and not-q objects in the set being sampled). On this simple and fundamental relationship rests the virtual entirety of probability theory. Inexplicably, it is the very relationship with which Chris Cole is still arguing.

E.9 expected observational frequency of q = |q| / (|q| + |not-q|) in sample space

To anyone who knows anything about probability theory, it is very clear where Chris is coming from. There is in fact a well-known class of probability-theoretic paradoxes, written of by Rudolph Carnap among others, centering on different ways to apply the so-called "principle of indifference". For example, should it be applied to the possible combinatorial distributions of balls inside an urn before sampling begins, or to each ball drawn from the urn *without replacement?* This decision makes the difference between assuming a combinatorial or permutative initial distribution. However, as Carnap notes himself, only the former method assigns probabilities to future events according to the frequency of their past occurrence. That is, only the former is consistent with E.9 and thus with the all-important connection between frequency and probability measure. Furthermore, as I've explained before, it is mandated by the rules of predicate logic.

Before proceeding, I'd like to say that I don't mind engaging in rational discussions of the fine points of probability and decision theory. What I *do* mind, and have become increasingly disturbed about, is Chris Cole's "invulnerability" to being proven wrong. Specifically, it seems that whenever Chris senses that he is about to lose an argument, he simply clams up or changes the subject. Unfortunately for the intellectual progress of the Mega Society, our members seem slavishly willing to let him employ this tactic.

In *Noesis* 123, Chris himself once again brings up the 10-marbles problem. The only sensible conclusion I can reach is that he still believes he's right about it. So let's see if we can finally bring this whole purulent controversy to a belated head.

For Publisher Cole: A Prediction...and a Bet

Publisher Cole and I apparently subscribe to two different conceptions of probability. In the version he seems to espouse, the observed frequencies of observations tell us nothing about the future, not even with respect to closed and finite spaces like Ron's box of marbles. He can try to deny or qualify this, but it is directly implied by his past statements and can no more be hedged than a woman can be "half-pregnant". Even if he concedes that frequency depends on distribution in this kind of problem, his chronic insistence that we "cannot know" the nature of this dependence still implies that the past tells us virtually nothing about the future.

On the other hand, I say that where a sample space is closed and finite - a stipulation designed to neutralize certain infinitary paradoxes of confirmation theory - and where we are in a position to randomize our observations of it, the past frequencies of distinct kinds of observation imply future frequencies with a degree of confidence determined by Bernoulli's (or Chebyshev's) *law of large numbers*. I.e., if an event q occurs k times in n identical independent trials, then as n rises, k/n inexorably draws closer to the probability P(q) of q, which in the 10-marbles problem is identical to the proportion of q marbles to the sum of q and not-q marbles in the box. The law of large numbers is just a way of establishing that frequencies can be used to estimate probabilities and the compositions of sets. This is ultimately a *logical* relationship that applies even when the number of trials is small, albeit with a lower level of precision (which is not the key issue here). Since the 10-marbles problem comes complete with unambiguous data on both frequency and composition for the initialization of Bayes' theorem, we need make none of the troublesome initial assumptions that have caused difficulty elsewhere

Since Publisher Cole has requested in *Noesis*123 that I predict the outcome of an experiment that cannot be predicted by theories to which he already subscribes - including his theory of "Bayesian regression" as previously described in *Noesis* - I propose that the following statistical experiment be conducted on neutral ground.

1. Compose a large number of statistical rules for constructing 10-element sets of 1-10 colors each. Some of these rules should permit the construction of all-white sets.
2. Feed these rules into a computer programmed to construct sets accordingly and construct equal numbers of sets using each rule. Continue until a significant number of all-white sets have been constructed.
3. Program the computer to randomly sample the elements of these sets at 10 samples per set. Loop this procedure until each set has been sampled numerous times and a large number of all-white runs have been generated.
4. Tabulate every all-white run according to the composition of the corresponding set.

I hereby predict that, given enough constructive variety, randomness, and computer time, the fraction of all-white runs involving all-white sets will converge on a limit of approximately .67, as computed from a straightforward measure-theoretic initialization of Bayes' theorem, at a rate determined by the law of large numbers. Constructive distinctions will have nothing to do with it. And furthermore, Jojo and I hereby bet the Brooklyn Bridge, Queens-Midtown Tunnel, and - why not? - the Empire State Building and Twin Towers that we're right! But only, of course, against commensurate stakes.

This experiment is doable by anyone with decent computative facilities and a modicum of programming ability. So if it doesn't get done, I don't want to hear any more about it. It should be evident to anybody who read Jojo's entertaining bit several paragraphs ago that an experiment isn't necessary to confirm my viewpoint, and logical implication is superior to speculative prediction in any event. Whether we're talking about ten white marbles or one, the fundamental probabilistic considerations are the same; it's a simple matter of the basic logical relationship between frequency and proportion.

ADDENDUM TO "ON NEWCOMB'S PARADOX" (copyright 1996 by Chris Langan)

While housecleaning, I found an old article by Robert Nozick on Newcomb's paradox. In it, he discussed the results of a *Scientific American* survey he undertook years ago with the venerated *Games* columnist Martin Gardner. After reading it again, it occurs to me that in my haste to make a point, I left something out of my review.

Incidentally, Nozick himself considers the problem insoluble. Gardner, on the other hand, concurred with a small minority of *Scientific American* readers who believed that a perfect predictor is "impossible" because its existence would lead to "a logical contradiction". Proper translation: as reality is self-consistent, it can contain no real inconsistency. Unfortunately, without a logical framework of the NST (or CTMU) variety, nobody was able to say, in terms of the structure of reality as opposed to mere game-theoretic argumentation, what this inconsistency would have been, given that a predictor is not required to make predictions that involve him in logical contradictions. The matter was thus reduced wholly to belief, and specifically to belief in free will. However, just as belief or disbelief in Santa Claus has no bearing on the fundamental nature of reality, neither does belief in undefined "real" contradictions.

In place of a real contradiction, Nozick substituted a contradiction from game theory. The *expected utility agument* says that you should look only at the overwhelming empirical evidence of the predictor's success; the *dominance argument* says that since the money is already in the boxes, you can lose nothing by taking both of them. Expected utility implies that you should (play the game as though you) believe in the predictor, whereas dominance implies that you should not. But this is not the kind of contradiction that can invalidate an hypothesis about the fundamental nature of reality. More probably, it points to a weakness basic to game theory itself, or to a weakness that game theory has unwittingly "borrowed" from some other field or worldview. For example, a close look at the dominance argument, which Gardner calls "flawless", reveals an unsubstantiated assumption to the effect that time is always linear...an assumption which modern physics has called seriously into doubt.

In my last contribution I re-introduced the NST (Nested Simulation Tableau), a physical and computational analogue of the theory of metalanguages, likening it for the purpose at hand to a computer within a computer...within a computer which is running a simulation within a simulation...within a simulation. ND, the "omniscient being" in Newcomb's paradox, is directly or indirectly identified with a "programmer" residing in a higher NST level. While I pointed out one NST-illustrated possibility - namely, that free will is illusory and that our decisions may be determined by undecidable higher-level programming - trimming the entire 3-part contribution to an even eleven pages caused me to omit another possibility that I had in mind when I wrote the original paper.

This other possibility incorporates both free will and a certain NST potential regarding the nature of time. Specifically, time becomes "random-access"; the programmer can wait for you to make your decision freely, freeze time, randomly access the moment when the boxes "were" filled, cause ND to fill them, and then return to your "present" and unfreeze time. Since the box-filling event was hidden from you in the first place, there can arise no logical contradiction with any memory you might otherwise have. Indeed, since the event was hidden from everything and everybody on your level except ND, and the money he wagers was either spontaneously manufactured or gathered in advance and secreted on his person, no detectable contradiction *of any kind* can arise, except perhaps one of the sort to which we are already accustomed in connection with the collapse of the quantum wavefunction (e.g., "Schrodinger's cat")...or, of course, an absolute contradiction at the metaphysical (CTMU) level, which neither Nozick nor anybody but me is presently able to specify.

Remember, *The Resolution of Newcomb's Paradox* asks you to "believe in" nothing, not even the NST (or its more complete and powerful outgrowth, the CTMU). It doesn't have to, because the paradox has already given you unlimited empirical confirmation in its stead (Nozick, in his article *Reflections on Newcomb's Paradox*, describes ND as having "already correctly predicted your choices in many other situations and the choices of many other people in the situation to be described"). The model's intrinsic credibility, no matter how low it may be, is completely irrelevant. It has the nature of a logical possibility, is vulnerable only on logical grounds, and needs only empirical confirmation for its probability to rise. Since the paradox provides such confirmation in great hypothetical abundance, it remains only for the NST to provide a tool for logical analysis of your position in the "game".

Again, what was needed to resolve Newcomb's paradox was just a logical framework within which omniscience, and the subject's own decision-theoretic situation, could be represented. Because the NST qualifies as such a framework - and indeed, as the minimal and most general such framework - it fills the bill. Technically, this framework constitutes a *model* for Newcomb's problem, and thus for the resolution of the paradox associated with that problem. Logic, of course, does not require advance belief.

The bottom line is this. If you wish to refute *The Resolution of Newcomb's Paradox*, you must produce an unavoidable logical contradiction of the kind that Gardner and Nozick apparently had in mind. Since that will take you a long time, you currently have no grounds for dispute. If confronted right now by ND and his boxes, you would have to take both him and his wager seriously on a combination of rational and empirical grounds. So, in your subjective calculations of personal utility given your current state of knowledge, you can dismiss neither the paradox itself nor its NST resolution. And since this is all that the resolution itself says, refutation is impossible in any event.

I almost wish I could give some of you a way out...a way to explain why, after seven years, you still don't understand. But I can't. All I can do is hope that you'll use the brains that God gave you (even if some of them *did* come from California!).

TO CHRIS COLE, ON HIS REMARKS AT THE END OF *NOESIS* 123

First, let me say that I feel kind of badly about having to add insult to injury like this. If you don't know what I mean, Chris, then you'd better take the time to read what I've written above. I've given you chance after chance to communicate intelligently with me, and it's nobody's fault but your own that you've never responded acceptably.

Although you introduced Newcomb's paradox to our readership, you again refuse to discuss my resolution of it in detail. Although you don't want anyone to find out why, the reason is obvious: your own position on the problem is logically untenable (more on that above and below). Even if the members of our little group aren't as smart as they're cracked up to be, they're not as dumb as you play them for...except, that is, when they fall for one of your well-timed and hard-spun "Calls for Votes".

Again, I'm forced to repeat myself. **My stance** on Newcomb's paradox requires **no belief** in anything whatsoever, but merely the **introduction of a possibility** not accounted for by Nozick's "dominance argument", which incorporates the unproven assumption that time is strictly linear. Possibilities are not assumptions; they are logical in nature, intrinsically neutral, and acquire likelihood only through confirmation My stance centers on the **logical stage** of analysis, and constitutes an application of the branch of logic known formally as **model theory.** By virtually "parallelizing" time, this model **suspends the unwarranted assumption** that time can only be linear .

Your original stance, on the other hand, was based on **dominance** and thus requires **unquestioning belief** in temporal linearity. Thus, it fixates on the **credential stage** of analysis. In other words, the **metaphysical assumptions** were all yours. Accordingly, your stance was **logically inferior** to mine, and your protestations to the contrary are 180 degrees out of kilter. If you now wish to **change your stance** to one of absolute skepticism, you **still cannot accuse me of making assumptions.** If you keep it up, you will only confirm a sullen ignorance of the logic involved in this kind of problem.

I know you're a busy and important man, but you simply must stop trying to speed-read deep material if you wish to serve credibly as Publisher of *Noesis*. I still respect your intelligence, but my respect for your intellectual *modus operandi* is low and sinking fast. Do you really want it to bottom out for good?

Just in case you haven't read the rest of this contribution, I've made a prediction for you. It is exactly what you asked for on page 24, *Noesis* 123. You ask, I give. Now what do *you* have to give in return?

As a successful businessman, you know that before you get to run up big bills, you must establish credit by paying little ones. You have a bunch of little bills with me - in fact, a piece of some big ones - but you haven't paid a dime's worth of recognition on them. That's one reason I've been so tight with my predictions. So naturally, I'm curious to see what you'll do now that I've upped the stakes at your request.

Incidentally, would you mind telling everybody what you mean by "current orthodox metaphysics"? A lot of us have been laboring under the disturbing impression that this refers to a dark gaping hole that predicts nothing but its own inadequacies, and doesn't even indicate which of the eight or so interpretations of quantum mechanics is correct. You'd probably have been better off invoking "current orthodox cryptozoology".

Good luck. I'd say you're going to need it.

ON CHRIS LANGAN'S PAPER AND NEWCOMB'S PARADOX by Rick Rosner

I just read Chris's paper, and followed about 20% of his arguments. (It's a well-written, convincing and entertaining paper, but I'm lazy. (In fact, I just formed a company with two other guys. We named the company Stinky Boys, because all three of us, being lazy and feckless, stink.)) Chris took a fairly straightforward probability problem and did a good job of showing that the orthodox solution rests on some not-completely-reasonable assumptions. The small amount of reading I've done about Bayesian theory makes me think that the whole shiny structure of statistics and probability is a glib assembly of efficient assumptions resting in a theoretical swamp.

Which means that supposedly culture-fair probability problems actually require some hidden indoctrination on how to make clean, orthodox probabilistic assumptions (though probability problems are nicely amenable to self-indoctrination--many bright people reinvent basic orthodox probability on their own). However, I'm still happy to consider the marble problem insoluble.

But what I really want to talk about is an aspect of Newcomb's Paradox that may have been overlooked up to now, which is its fashionability and how the problem's trendiness has influenced people's attempts to solve it. I spoke with Chris Langan and agree with him (and I think I understood him correctly, at least on this one point) that the virtual reality perspective is the most efficient framework in which to think about Newcomb's Paradox.

VR is a hot concept, and the smarter one is, the longer one has been aware of the possibility (and future near-certainty) of simulations of reality which are indistinguishable from actuality. In other words, people are increasingly aware that, at some time in the next century, people will be subject to fictional or otherwise artificial situations which are not easily distinguished from real life. Most people are so aware of it that the previous sentence, which might have been startling and exciting a generation ago, is now just a boring premise for a Keanu Reeves movie.

Given the current awareness of virtual reality, magical situations such as one involving Newcomb's perfect predictor are more readily-considered; it's easy to imagine being jacked into a VR situation with Newcomb's genie, and it's easy to see that in VR, Newcomb's genie could really be a perfect predictor, able to violate rules of normal reality in order to maintain 100% accuracy.

But for us, today, Newcomb's genie can't exist. None of us has any direct evidence of systematic violations of the tacit rules of everyday existence. Were any of us to encounter Newcomb's genie in the next few years, we'd have to conclude that the rules of normal reality had been breached, and that anything could happen (or that we were the victims of a sleazy con). Were I to run into Newcomb's genie, I'd believe his/her claims--hey, the genie's gonna be more at home in breached reality than I am. I'd take only the million dollars, but I wouldn't expect to be able to spend it.

Dear Rick:

This letter is a response to Chris Langan's remarks addressed to me in issue 121, page 2 onward.

First, it is true that my intelligence tests were not based on a clear-cut theory or definition of intelligence as it interfaces with reality. However, my philosophical theory, which is essentially a theory of categories, has in recent years yielded very clear and precise interpretations of virtually every major theory of intelligence that I am aware of, including the two-factor theory of Cattell (fluid versus crystallized intelligence), the three-factor theory of Sternberg, the five-factor theory of John Dewey, the seven-factor theory of Howard Gardner (which has an eight-factor variant), and the ten-factor theory of L. L. Thurstone (see his book, The Nature of Intelligence).

My theory does have a direct relationship to the notion of intelligence. In fact, in his book, Concept and Quality (page 17), Stephen Pepper explicitly states that the purposive act, which he proposes to use as his metaphysical "root metaphor" in that book, is "the act associated with intelligence." We can see this by noticing that to solve a problem (= intelligence) is to achieve a purpose.

Langan's mention of Cantor's paradox is interesting because elsewhere in this issue I offer a letter (in response to Kevin Langdon) in which I analyze Cantor's 1895 definition of a set in terms of the phases of a purposive act. Unfortunately, Cantor's paradox was discovered in 1897, two years after his final and most sophisticated definition of a set, and it and the other paradoxes discovered shortly thereafter, such as the Burali-Forti paradox of 1899 and Russell's paradox of 1901, all evidently require some more sophisticated definition of a set. Some believe that the axioms of set theory proposed by Zermelo in 1908 (supplemented by Fraenkel and Skolem in 1921 and 1922, working independently of one another, and by von Neumann in 1925) constitute a sort of indirect way of "defining" what we mean by a set. My book does in fact offer an analysis of the nine axioms of this system, which are listed by Fraenkel in his article on "Set Theory" in The Encyclopedia of Philosophy, volume 7, pages 424-426.

But surely we have learned from Godel that no set of axioms can be regarded as absolutely complete and definitive for all possible mathematical systems. We may be satisfied with some system of axioms like those of Zermelo and his followers, just as the Greeks were satisfied to try to solve all geometrical problems with a straightedge and compass. But eventually some fundamental new innovation tends to emerge such as the analytic geometry of Descartes, that enables us to go beyond past approaches.

It may be that Langan's CTMU formulates an adequate approach to logic, cognition, metaphysics, etc, for all time to come, at least in a general way. It may be that Langan's CTMU says the same things I am saying, but in a mathematically more sophisticated way that can be grasped only by those who are deeply immersed in the logico-mathematical mode of expression.

Far from being one who tries to exclude opposing theories as inane, my own approach to philosophy is highly inclusive. I try to show the central commonalities of a huge range of theories (my book currently has 500 sections, almost every one devoted to a distinct way of looking at the world). So my natural inclination would be to incorporate the CTMU system within my own system of thought and point out its commonalities with so many hundreds of other systems and structures.

If I can understand Cantor's definition of a set in terms of my categories, as well as several other relatively sophisticated logico-mathematical structures such as Zermelo's axioms (my present analysis of which is superior to the one I initially gave a few years ago), then i probably could give an interpretation of CTMU in terms of my categories if it were presented in a sufficiently piecemeal and clear fashion, such as in terms of a definite set of axioms. langan has perhaps written up such a list of axioms or basic concepts, clearly numbered, in one of his many pages on this subject, but i don't recall having seen such. So my suggestion to him, if he really wants to be appreciated by me, is to put forth all of his basic concepts and axioms in clearly enumerated lists which i can ponder piecemeal and then put together into an organized whole in terms of my own mode of thought--namely, in terms of the phases of a purposive act. he need not make the purposive interpretation of his concepts or axioms himself; that would be my own contribution, once i see his system concisely presented.

If Mr. Langan cannot simplify his ideas as I have just suggested, then I do not see why I am under any obligation either to approve or to disapprove of them. He advises me at the top of page 8: "If the verdict of posterity means anything to you...then you will either produce a sound reason why the CTmu can't work, or publicly change your attitude regarding it." This is about like saying that if I cannot disprove Wiles' proof of Fermat's last theorem, then I have an obligation to accept his proof. Surely that is absurd, black-and-white thinking. A three-valued logic would permit the option that I currently choose, the "I don't know" option. I don't know if CTNU has merit because it has not been broken down into easily digestible pieces--either a complete list of basic concepts or a complete list of basic axioms, each one numbered and explained, preferably with simple examples, if possible.

The key advantage of my own theory, simple-minded though it may be, is that it can be used to give clear interpretations of hundreds of systems of thought, such as my analysis of Cantor's definition of a set elsewhere in this issue. Langan has not blessed us with a similar display of how his system would interpret hundreds of different systems, so that its impact on traditional philosophy is left unclarified. It's possible that Langan is chiefly interested in dealing with mathematical problems, in which case it should be addressed to mathematicians rather than to a philosopher. If he does want his theory understood by me, then I have stated above the simple means by which this can be achieved: a simple, complete list of basic concepts or axioms.

Ron Hoeflin

Dear Rick:

This is a reply to Kevin Langdon's remarks on free will in issue 122, page 6, titled "Reply to Ron Hoeflin on Free Will."

I found Kevin's most pregnant remark to be "Consciousness is passive; thought is active; will mediates between them." In terms of my analysis of a purposive act, these three factors correspond to what I label QD, DA, and A, respectively. But these are just three of the eight main phases of a purposive act. The full set of phases can be labeled D, DA, A, AG, G, GQ, Q, and QD, where D stands for drive, A for anticipatory set, G for goal object, and Q for quiescence, as in desire for water (D), reaching for it (A), grasping the glass (G), and drinking it (Q), with DA, AG, GO, and QD being the interconnecting links between the foregoing phases.

One can discern the eight phases in many structures, ranging from quantum mechanics to mysticism. See for example the Eightfold Path of Buddhism, and the eight kinds of quantum reality outlined by Nick Herbert in his book titled Quantum Reality. But a particularly clear and simple illustration is provided by Georg Cantor's definition of a set as given in The Encyclopedia of Philosophy, volume 7, page 420, column 2, paragraph 2:

> A set [Menge] is a collection into a whole [Zusammenfassung zu einem Ganzen] of definite, distinct objects of our intuition or our thought.

The correlations of the key words in this definition with the eight phases of an eight-phase analysis of purpose would look like this:

Phase	Key word in Cantor's definition
D	our
DA	thought
A	collection
AG	definite
G	objects
GQ	distinct
Q	whole
QD	intuition

The word "our" refers to the embodied drive, the agent, who does the collecting of objects into a whole.

The word "thought" designates the commencement of the reaching out of the drive or agent for a plan, strategy, anticipatory set for achieving its satisfaction, which in this case would be the formulation of a plan for what sorts of objects to collect into a whole.

The word "collection" indicates the completed plan or strategy or anticipatory set, as embodied in the actual process of collecting the appropriate objects into the desired whole.

The word "definite" indicates the ability of this collecting plan or process to reach out and select definite objects, such as birds, that can become part of the whole we refer to by the word "birds."

The word "objects" refers to those specific entities that the collection process picks out for inclusion in the set it is creating.

The word "distinct" refers to the fact that the foregoing objects must each be distinct from one another, as indicated by their unique quiescent properties, e.g., this bird will have a little spot on its beak, Q, that no other bird, G, has.

The word "whole" indicates the quiescent properties that bind a given set of objects together so as to form a clear-cut whole, such as the property of having bird-like properties that all birds share in common and do not share with other sorts of objects such as elephants.

Finally, the word "intuition" indicates the input of the bird-like property, Q, back to the organizing agent or embodied drive, D, by means of which it recognizes that it has sorted things out properly.

What Kevin calls "consciousness" would evidently correspond to what Cantor calls "intuition"; what Kevin calls "thought" corresponds to what Cantor calls "thought"; and what Kevin calls "will" corresponds to Cantor's word "our," meaning the agent or embodied drive.

I will just emphasize two points: (1) Kevin has focused on just three of the eight phases of a purposive act; and (2) this eight-phase structure has been reached by philosophers working from entirely different perspectives. You don't have to be a mystic or have a guru to arrive at this analysis, since similar structures can be found in practically every type of philosophical orientation.

A third point is also worth mentioning: (3) the purposive act can be divided into any number of phases from one to ten. Higher-dimensional structures, such as Kant's twelve categories, seem to be compound structures based on the lower-dimensional structures.

A ten-dimensional structure, for example, would include categories corresponding to the "spokes" of the purposive wheel, namely DG and AQ. In Nick Herbert's discussion of quantum realities, for example, one can discern these two supplementary approaches somewhat vaguely described but not given distinct numberings by Herbert towards the end of his book.

I have successfully analyzed Alfred North Whitehead's 47 categories in Process and Reality in terms of purposive structures and I devote an entire chapter of my 16-chapter book, Decoding Philosophy, to this analysis. What makes this point germane here is that Whitehead seems to be associating determinism with the AQ phase and freedom with the DG phase (the ninth and tenth "phases" of a purposive act), because A brings about Q (as when a bullet, A, produces death, Q) whereas D and G are causally independent of one another, when considered as simultaneous entities, since light waves or other messages or actions cannt bridge the gap between them instantaneously (as illustrated by the gap between the would-be murderer, D, and the person he intends to murder, G, which cannot be bridged instantaneously and hence gives each party a momentary freedom vis-a-vis the other.

Ron Hoeflin

THOUGHTS
Chris Cole

Three negative thoughts and one positive thought and why I've been thinking them: First, I don't think we need a constitution, elected officials, etc. Second, I don't think we should admit people on the basis of low-range IQ tests. Third, I don't think Chris Langan has answered my challenge to provide a falsifiable test of CTMU. Fourth, congratulations to our Editor.

While Kevin Langdon's proposals in the previous issue to simplify the old Mega Society's by-laws for adoption by the current Society are a decided improvement, they do not go far enough. Let's take a look at what really goes on in the Mega Society. People take a test; they apply for admission; they get a newsletter. That's it. There doesn't seem to be much call for representational democracy. Pure democracy is more the rule than the exception for such simple organizations, ranging from boards of directors to small towns. My assertion is that the KISS principle should be applied here, and that, in fact, more structure will engender controversy. There is an old joke that in academics the politics are vicious because the stakes are low. Let's just keep it simple and vote when there is a dispute.

A couple of legalistic points: Can we change the by-laws? A majority vote can overrule the by-laws, even if the old by-laws do not allow it, because the old by-laws have questionable validity. If the old by-laws had been voted on and approved by a majority of the current members, there would be an argument that we all bound ourselves to them. But since that is not the case, we are now in the position of choosing our mode of self-governance, and as such simple majority vote is all that is required.

Also, a clarification: I am NOT proposing that we vote on the membership application of every candidate. The old Mega Society (whether intentional or not) had the same entrance requirements as the current one, namely, a score at the one-in-a-million level on suitably normed high-range tests. (I ignore for the moment Kevin Langdon's argument in the previous issue that the one-in-a-million level is not where we think it is. This could be a matter for a future vote.) Several members have been admitted under this criterion in the past few years. The procedure has been to send the proof of qualification to Jeff Ward. Jeff consults with the test author for verification, and assuming all is in order, the candidate is admitted. I do not propose to alter this procedure. What I do propose is that we vote on whether we are going to admit people on the basis of low-range tests. I am not interested, in the case of Paul Maxim, in knowing the particulars of his test scores or even the particulars of the tests he took, other than the range of the tests. It is enough for me to know that the authors of the tests do not claim that they can be used to distinguish at the one-in-a-million level. I think we should believe them.

This leads to my second thought for the day. I was going to spend some time in this issue examining the concept of "range" in testing, and arguing that it is bad science to use an instrument designed with a certain range outside of that range. However, it occurs to me after reading Kevin Langdon's previous issue that this must be commonplace knowledge to the members of the Society. If this conclusion is wrong, please let me know and I'll write more about it in future issues. At any rate, it makes no sense to use low-range tests as a basis for admission to the Mega Society, and I urge members to vote to exclude their use.

Lastly, let me respond to Chris Langan. I asked Chris to provide some falsifiable evidence for CTMU, and in this issue he responds in an oblique way. His response is not really what I was asking for, but from other oblique comments, I gather that he doesn't want to reveal too much at this time. So we are very close to where we've been for a long time.

However, maybe we can make a little progress. One page 14 of this issue Chris makes what he thinks is a falsifiable prediction. Please read his four-step "statistical experiment" now and return here.

I will now propose a large number of statistical rules that satisfy the conditions of step 1 of the experiment, and yet which will not yield in the limit 2/3 as the ratio of all-white sets given 10 white selections in a row. In fact, I'll propose infinitely many such statistical rules. My first rule is this: generate a random number between 0 and 1. If the number is less than 1/100, put ten white colors in the set. Otherwise, put nine white colors in the set, and one black. My second rule is to change the 1/100 to 1/10,000. My third rule is to change it to 1/1,000,000. And my nth rule is to change it to 1/100^n.

It should be clear to all readers that in sampling (with replacement) sets produced by this infinite set of statistical rules, we will quite often get 10 white selections in a row and yet in very few cases will the set be all white. And the limiting ratio of all-white sets given 10 white selections in a row will be closer to 0 than it is to 2/3.

I do not think this will satisfy Chris. Even though I believe that I have obeyed the conditions of Chris' test, I suspect that he will not think so. I think he will complain that my statistical rules do not exhibit enough "constructive variety." My rejoinder is that Chris is simply pushing the problem back a step, from the distribution of colors to the distribution of statistical rules.

My guess is that Chris has an intuition that because of the symmetry of the set of 10 colors (i.e., because no one color is distinguishable from any other), there must be in the limit of a large number of rules an equal likelihood of producing 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, or 10 white colors in the set. My guess is that this is what he means by "measure-theoretic initialization of Bayes' theorem." And, indeed, Chris is in good company, at least historically, because intuitions like this lead to the formulation of the so-called "principle of indifference." This principle is not generally accepted these days, for a variety of reasons. I'll discuss one practical reason below.

The practical reason for rejecting the principle of indifference is that it does not accomplish its purpose, which is to assign probabilities in situations where there is no information to distinguish between alternatives. The principle dictates that we should make all such alternatives equiprobable. The problem is that there is more than one way to partition reality into alternatives. For example, what is the average length of a chord on a circle? Assuming that all chord lengths are equally likely the answer is the radius: the average of the shortest chord (0) and the longest chord (the diameter). Assuming that all points are equally likely the answer is 4/3 times the radius: the average of the chords from any point to the opposite point. Which answer is correct?

Finally, I don't understand the connection between the marble problem and CTMU. Does CTMU assume the principle of indifference? How is Chris' "statistical experiment" a test of CTMU?

Congratulations to Rick for breaking into the TV writing business. I saw two specials recently with Rick's name on them (although the actual name listed was "Rick G. Rosner" – let me guess: "G" for "Gilligan", right?). One was Our Favorite Christmas Specials, and the other was World's Funniest Outtakes. The latter had a great bit that was vintage Rosner: outtakes that had been faked and how you could tell. Never try to fool the King of Faked I.D.s!