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HIGH DELUSION by John Lattanzio

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The affair is now officially over. At least, that is the position of John Maddox, editor of "Nature". "Not so", says Jacques Benveniste. Or, to be more precise: "...Facts are stubborn, and so are we.... There is more to come."

So what is this all about? It resulted from an article by Professor Benveniste (of Inserm 200 at the University of South Paris) and colleagues in "Nature" (30 June 1988, p. 816). Briefly, they describe an experiment that claims to show that human white blood cells respond to a solution of antibodies, even when the solution no longer contains a single molecule of the antibody. Further, they alleged that the biological activity fluctuates periodically with increasing dilution.

"Nature" published an editorial titled "When to Believe the Unbelievable" in the same issue as the Benveniste paper. The editorial argued that SOMETHING must almost certainly be wrong with the Benveniste experiment, although one could not determine the problem from his article. ("Nature" published a similar disclaimer with Targ and Puthoff's paper on Uri Geller, something never mentioned by Uri ["Nature", 18 Oct. 1974, p. 559]).

The Benveniste experiment does sound something like experimental verification of homeopathy, the belief that a symptom can be cured by giving vanishingly small concentrations of substances known to produce the same symptoms when taken in higher doses. The claim that therapeutic effects occur at concentrations so low that there remains no active ingredient has been the main argument against homeopathy. Experimental verification would indeed be a major discovery -- in fact, it would mean a major change in thinking about how the universe works. Of course, "Nature" knew this, so what were they to do when the paper was submitted for publication?

Well, before even considering acceptance of the paper, "Nature" insisted that other laboratories replicate the experiment, so Benveniste arranged for researchers in Israel, Italy, and Canada to do so. With reported safeguards, they obtained essentially the

same results. In spite of the apparent replication, "Nature" decided to send "independent investigators to observe repetitions of the experiments". This is when the controversy really began.

The investigating team consisted of three people: editor John Maddox, a physicist; Walter Stewart, an organic chemist and investigator of scientific fraud; and magician James Randi, familiar to "BASIS" readers. Their report appeared in the 28 July issue of "Nature". Their conclusion was that the experiments were statistically ill-controlled, included observer bias and systematic error, and were not always reproducible.

Indeed, measurements in conflict with the claim had not been reported in Benveniste's paper, although they were recorded in laboratory notebooks. Further, the "Nature" team was "dismayed to learn that the salaries of two of Dr. Benveniste's co-authors of the published article were paid under a contract between Inserm 200 and the French company Boiron et Cie., a supplier of pharmaceuticals and homeopathic medicines, as were our [the investigators'] hotel bills."

The investigating team supervised a double-blind experiment aimed at removing notable biases. Test tubes containing various dilutions were coded, and "the code itself was folded in aluminum foil, enclosed in an envelope sealed by Randi, and then taped to the laboratory ceiling for the duration of the experiment." Obviously, the conclusions quoted above were based on the negative result of the test, but Dr. Benveniste was granted a reply ("Nature", 28 July, p. 291), and it makes interesting reading.

"...A tornado of intense and constant suspicion, fear, and psychological and intellectual pressure unfit for scientific work swept our lab. Furthermore, these lesson-givers were astonishingly incompetent." His article drips with invective: "Then Stewart, with his typical know-it-all attitude...", and "The next day the hysteria was such that Maddox and I had to ask Stewart not to scream."

It certainly sounds entertaining: "Stewart imposed a deadly silence in the counting room, yet loud laughter was heard where he was filling chambers. There, during this critical process, was Randi playing tricks, distracting the technician in charge of its supervision!" When he calms down, Benveniste makes the point: "It will now be clear what a mockery of scientific inquiry this was." But his calmness does not last. His final paragraph says: "This kind of inquiry must immediately be stopped. Salem witch hunts or McCarthy-like prosecutions will kill science. Science flourishes only in freedom. We must not let, at any price, fear, blackmail, anonymous accusation, libel, and deceit nest in our labs. Our colleagues are overwhelmingly utmost decent people, not criminals. To them, I say: never, but never, let anything like this happen - never let these people get in your lab."

So what are we to make of all this? There are two points: 1. the

effect on science, 2. the perception of the public.

From the scientific viewpoint, there were flaws in the experiment (despite its replication at other institutes). But rather than just show one double-blind test that fails, a more satisfactory (although possibly impractical) approach is to explain the observations quoted by Benveniste and collaborators. A broader question, however, and one raised in many letters to Nature as well as the editorial of 4 August, is whether the journal should have published the paper in the first place.

I believe that "Nature" did the correct thing by publishing the article. A scientific journal exists to publicize and distribute ideas and research, subject to reasonable standards. If every care was taken, as was claimed in the paper, to remove various biases, then the paper was publishable. The journal cannot be expected to find all caveats for each paper that it publishes.

I know others who argue that, given the extraordinary claims of the paper, some extraordinary evidence would need to be presented. Well, the claims were "verified" by independent laboratories. It now appears that they too were in error, but the journal's editor (and the appointed referees) can only be expected to check for gross errors. It is up to others to reproduce (or otherwise) the research once the journal has made it known to the community.

The ongoing process of science determines the lasting veracity and value of a publication. "Nature" took all reasonable steps, and then published. Although debate continues concerning whether the paper should have been published, there seems to be agreement that it was inappropriate to send an investigating team, or "ghostbusters", as the English magazine "New Scientist" called them.

Maddox defends this action ("Nature", 27 October 1988, p. 760), but fails to convince. He states that "Journals do not normally undertake investigations of contributors' laboratories, and for good reason: they have neither the resources nor the skill." To which I would add that neither have they the right, nor is it their responsibility. His summary makes interesting reading, and is highly recommended.

Likewise is a perceptive letter ("Nature", 8 Sept.) from G.A. Petsko, a chemist from MIT, who warns that in today's scientific community, there is so much competition that publishing a "wrong interpretation of data, EVEN IF THE DATA ARE ACCURATE AND ADMIT OF MORE THAN ONE INTERPRETATION" can be extremely harmful to one's reputation. Science is so competitive that honest mistakes can end a career. Petsko argues that fraud is more likely to occur in a climate where mistakes are treated too harshly. He argues for "decriminalization of error", and rightly stresses that "detection of error, and its correction" is the real goal.

Which brings us to the final point. What are we to make of this

fiasco? They see Randi, the editor of "Nature", and a specialist in scientific misconduct off to investigate a laboratory. The clear implication is that fraud is involved. A grossly unfair, albeit unstated, allegation. The whole idea of an investigating team is offensive to me, and I believe Randi would have been wise not to have taken part.

In any event, the public has a very poor understanding of how science corrects itself. The "Nature" fiasco is unique, but who will remember that? Perhaps the last word should go to P.J. Lipowicz, who, in a letter to "Nature" (8 Sept.) states: "I submit that it would be easier to prove an incredible result like Benveniste's to scientists than it would be to disprove it to homeopaths."

DEGREES OF FOLLY: PART III by William Bennetta

[Parts I and II of this article ran in our February and March issues, respectively. Here is a summary:]

By law, no unaccredited post-secondary school in California can issue degrees unless the school has been approved by the superintendent of public instruction (the chief of the State Department of Education). In 1981, when Wilson Riles was superintendent, the Department approved the granting of MS degrees in biology, geology, "astro/geophysics", and science education by the ICR Graduate School (ICRGS), an arm of the Institute for Creation Research. The ICR is not a scientific institution, but a religious ministry promoting "creation-science", a pseudoscience based on literal readings of the Bible. The president of the ICR and the ICRGS is Henry Morris, a preacher and former engineer.

In 1987, after the superintendency of the Department had passed to Bill Honig, the ICR applied for renewed approval. By then, "creation-science" and the men who purveyed it had been repeatedly discredited. Nobody could have inquired into "creation-science" or the ICR without finding that both were fakes.

In August 1988, the Department sent a committee of five to assess the ICR's degree programs. The five were: Robert L. Kopach, professor of geophysics at Stanford; Stuart H. Hurlbert, professor of Biology at San Diego State; G. Edwin Miller, vice-president for administration at United States International University; James A. Woodhead, professor of geology at Occidental College; and George F. Howe, professor of biology at The Master's College, a religious school. (Howe -- who had been nominated for a place on the committee by Henry Morris -- would emerge as the ICR's advocate.) The committee was managed by the man who had assembled it: Roy W. Steeves, of the Department's Private Postsecondary Education Division (PPED).

The committee's report was farcical. It omitted or obscured anything about the real nature or aims of the ICR and the ICRGS, and it promoted the fiction that the ICR did scientific work; then it recommended "by a vote of 3 to 2 that full institutional approval be granted." Its last page bore the signatures of the committee members, who were denoted by name only. There was nothing to suggest their professions, affiliations, titles, or qualifications.

Later in August, the truth got out. The two men who had voted against approval -- Woodhead and Hurlbert -- furnished Honig with separate accounts of what they had seen. Hurlbert wrote that he had had little influence on the committee's report and was not an author of it. Then he exposed the ICR's operations and misrepresentations in detail, providing many examples and quotations.

On 10 November, Honig met in Sacramento with Woodhead, Hurlbert, and Howe. (Kovach and Miller had been invited, but could not attend.) Howe brought a disingenuous document, written mostly by Henry Morris, that purported to rebut Hurlbert's account. The meeting was inconclusive. Honig, who evidently did not want to take part in a sham or scam, judged that he might resolve the case by turning to Kovach. Kovach already had seen Hurlbert's dissent; and in late November, the department sent him other information that had not been considered during the committee's doings in August.

PART III

George Howe and Henry Morris have been working together for many years. In the 1970s, for example, each was an officer and a director of the Creation Research Society -- a fundamentalist group whose members must subscribe to a creed that begins with: "1. The Bible is the written Word of God, and because we believe it to be inspired throughout, all of its assertions are historically and scientifically true in all of the original autographs. To the student of nature, this means that the account of origins in Genesis is a factual presentation of simple historical truths."(1)

Morris was the Society's president early in the decade, and Howe was the editor of its quarterly. In 1977, Howe became its president.

Since 1982, Howe and Morris have been linked in a fundamentalist "legal defense" organization that, according to its president, seeks to "blow evolution out of the public schools." (I shall tell more about this next month.)

So when Roy Steeves, in the summer of 1988, named Howe to the committee that would examine the ICR, he furnished Howe with a chance to do a big favor for an old pal. And Howe evidently made the most of it, according to accounts that Hurlbert and Woodhead gave to me during telephone interviews. Hurlbert said that Howe had succeeded in turning the entire assessment into nonsense by

frustrating any consideration of the obvious and crucial question:
Was the ICR teaching anything that could be called science?

Woodhead told me: "With our committee constituted as it was, there was no possibility that we could have written a decent report. There was one person there, Howe, who would not have voted against those people [the operators of the ICRGS] even if their whole thing was a sham -- which is how, I think, it turned out."

Just how WAS the committee constituted? It evidently was constituted in defiance of the education code and the PPED's own "Guidelines for the Approval of Degree Granting Institutions Pursuant to California Education Code Section 94310.2", a document issued in May 1987. The code clearly called for an assessment of "each degree program offered by the institution", and page 26 of "Guidelines" said: "Visiting Committees for first-time applicants will consist of a minimum of five technically qualified educators for each program offered. Reapproval Visiting Committees will consist of three and may, if designees prescribe, consist of five or more technically qualified educators for each program offered.(2,3)

But Steeves, for assessing the ICRGS's program in biology, enlisted not three "technically qualified educators" but two: Hurlbert and Howe. For geology, he had only one: Woodhead. For "astro/geophysics", he had only the geophysicist Kovach. And for science education, he had nobody.

On 15 February 1989, in a letter, I asked Steeves some questions about the composition of the committee. One question dealt with the absence of a science-education expert. In his reply, sent on the 17th, Steeves asserted that the committee HAD had such an expert: George Howe.

"It is true", he wrote, "that Dr. Howe received his training in the field of biology, but he is the Chairperson of the Division of Natural Sciences at The Master's College. I have enclosed the appropriate pages of the catalog for your perusal. His professional assignment ideally prepared him for the review of the Science Education program at ICR."

This was just a wild bluff, for the catalog pages lent no support to Steeves's assertion. Howe's division at The Master's College(4) did not offer any program in education, did not offer even one course in the theory or practice of education, and had nothing corresponding to any of the education courses claimed by the ICRGS.(5) (Howe taught in the division's four-man Department of Biological Sciences, Physical Sciences, and Mathematics, which "seeks to promote a broad understanding of scientific facts and principles and exposes the unwarranted interpretations of scientific evidence that have damaged the cause of Christ.")

So: For assessing the ICRGS's program in science education, Roy Steeves's committee had had nobody at all. That program had

received a free ride.

In my letter of 15 February, I also asked Steeves about the absence of an astrophysicist. His answer was: "We did have a professor of geophysics [i.e. Kovach] who advised us that in the field there is no real distinction between the study of astrophysics and geophysics. As a matter of fact, Dr. Kovach also has received training in astrophysics."

How the committee operated cannot be reconstructed fully, for its members have some conflicting recollections. All, however, seem to agree on these points:

=> THE PPED DID NOT FURNISH THE COMMITTEE MEMBERS WITH ANY SIGNIFICANT INFORMATION ABOUT THE ICR, OTHER THAN THE ICRGS'S APPLICATION FOR APPROVAL, UNTIL THE COMMITTEE MET AT THE ICR ON 3 AUGUST. Steeves admits this, and defends it as standard practice. "To depart would have possibly raised due-process questions", he says. (This presumably is why the PPED denied Hurlbert's request for copies of the ICR men's curricula vitae.) Steves says that all the committee members knew that the ICR was clouded in controversy. Kovach disagrees. He did not know what he was getting into, he says, and he later "was surprised that it turned out to be so emotional and controversial".

=> THE COMMITTEE'S CHAIRMAN WAS KOVACH. This was not told in the committee's report, Kovach says, because there was an explicit agreement that the chairman would not be identified.

=> STEEVES INSISTED THAT THE COMMITTEE'S REPORT HAD TO BE SHORT AND HAD TO AVOID DETAIL. Steeves confirms this. If he had allowed elaboration, he says, we would have had a much longer report but no conclusion. As an administrative task, we had to get closure. We were trying to accomplish a purpose -- making a recommendation."

=> THE REPORT WAS DRAFTED BY KOVACH FROM PIECES THAT THE MEMBERS, WORKING SEPARATELY, HAD WRITTEN. THERE WAS NO SIGNIFICANT REWRITING BEFORE THE REPORT WAS PRESENTED FOR THE MEMBERS' SIGNATURES.

=> STEEVES EMPHATICALLY PRECLUDED ANY PROTRACTED DELIBERATION, AND INSISTED THAT THE REPORT HAD TO BE TYPED AND SIGNED BY THE EVENING OF 5 AUGUST. Woodhead says: "Steeves was in charge, and he vetoed the idea of taking [Kovach's draft] home for pondering." Kovach says: "Steeves set the theme. It had to be done then and there, not later. What he said amounted to 'You are not getting out of this motel room until we get this report finished and signed.'"

All of this, if infer, represents the PPED's standard practice as well as the PPED's version of due process.

I infer, too, that the PPED's regular practice includes a patently meaningless vote like the one in which the examination of the ICR culminated. There is no evidence that the committee made a discrete, identifiable assessment of each of the ICR's degree

programs; but if such work was done, it was then negated. In the end, the committee voted on only one question: Should the ICRGS as a whole -- including its financial and administrative structure, as well as its four degree programs -- be approved?

In effect, then, everyone voted on everything. Kovach, a geophysicist, voted on the biology program; Hurlbert, a biologist, voted on financial practices; Miller, an expert in finance and administration, voted on all four degree programs, even though he apparently did not claim expertise in any of the related disciplines; and so forth. Why had Steeves bothered to recruit any experts at all?

My inquiry into the ICR case has convinced me that the PPED acted with foolish insouciance and with only one objective: to create a nominal report by filling some sheets of paper with words. I do not think that the PPED took the examination seriously or cared about getting a valid result, even if (as things turned out) some individuals in the committee DID care. I see no sign that the PPED had any qualm about producing a farcical document, even if this would create a fierce dilemma for Bill Honig.

A question remains: Given that the report was incompetent, false, and misleading, why did the members of the committee sign it?

Woodhead says that he signed because he had promised to take part in a job and had been led to understand that the job included finishing and signing a report by the evening of 5 August. "What my signature means", he explains, "is that I was there".

Hurlbert says: "I signed as a statement that I was present and had participated. I did not think that it was a valid report. There were too many omissions and too much wrong information."

Kovach says that he signed because "It was a competently prepared report for the committee in the time that we had to prepare it." Miller thought that "it was a reasonably representative view of what we saw during our two- or three-day stay there." Howe "felt it was a very good report and said what we wanted to say."

I do not know how much of this history was known to Bill Honig in November, when he started to clean up the mess that the PPED had made. But I suspect that, after his meeting on 10 November with Woodhead, Hurlbert, and Howe, he understood that the committee's proceedings had included much sham and that at least two signatures on the committee's report did not mean what readers would surely imagine them to mean.

Early in December, after the Department had sent additional information about the ICR case to Robert Kovach, and after Kovach had examined that information, Honig called him. Kovach later gave me this account of the conversation:

"[Honig] did not ask me to change my vote. He asked, 'Given this

[new information], what would you do?' My answer was 'I would concur with what the new material said.' So, in effect, I changed my vote. IF WE [THE COMMITTEE] HAD HAD ALL THAT INFORMATION AVAILABLE TO US IN A TIMELY MANNER, I WOULDN'T HAVE VOTED FOR APPROVAL TO BEGIN WITH."(6)

On 8 December, in a story by Sandra Blakeslee, the "New York Times" told that Honig had barred the ICR from granting science degrees. Honig was quoted thus: "No one is stopping the [ICR] from granting degrees in religion or creation. But they are holding their people out to have science degrees, which they don't. The vast bulk of what they learn is not science."

Blakeslee recounted that a committee had visited ICR and had voted 3-to-2 for approval, and that Honig had asked the committee to reconsider. She quoted Honig again: "They had grave reservations about the science, but did not want their recommendation to put the school out of business. We then made the institute an offer. We will recommend approval and all you need to do is come up with a new name. Just don't call it science."

The ICR had refused, Blakeslee wrote; and Kovach, after discussion with Honig, had switched his vote.

On the same day when Blakeslee's story appeared, the director of the PPED, Joseph Barankin, sent a letter to Henry Morris. It said that the PPED had decided to deny approval and that the case would be reviewed on 10 January by the Council on Private Postsecondary Educational Institutions. (This is a state agency, separate from the Department of Education. It had no authority over approvals, but it can hear appeals and advise the superintendent.)

Early in January, however, things changed abruptly. Honig's Department drew back from the decision to deny approval, and the PPED began to negotiate with the ICR. On 6 January, functionaries of the Department -- in conversations with me and with others who had heard rumors of a deal -- said that the Department and the ICR had completed an agreement, and that the ICR case was no longer on the council's agenda. They would not tell the agreement's substance.

On 10 January, Barankin told me that an agreement was being wrought, and he listed some terms that he expected it to have, but he denied that it actually had been completed and signed.

What was going on? I shall try to answer that question next month.

End of Part III

NOTES:

(1) Later parts of the creed endorse the doctrine of organic "kinds", the worldwide extent and effect of Noah's Flood, and the special creation of a man named Adam and a woman named Eve. I have not yet seen the Society's report of the research by which those

names were discovered.

(2) "Designees" evidently means the director and other functionaries of the PPED, who act for the superintendent of public instruction.

(3) During inquiries to the PPED, I have found no suggestion that the May 1987 rules have been changed or superseded. As far as I know, they were in force during the examination of the ICR and are in force now. For a copy of "Guidelines", write to Joseph P. Barankin, Director, Private Postsecondary Education Division, State Department of Education, P.O. Box 944272, Sacramento, CA 94244.

(4) Until 1985, the school's name had been Los Angeles Baptist College.

(5) According to the ICRGS's dummy catalog, the core of the ICRGS's science-education program included courses called Curriculum Design in Science, Curriculum Implementation in Science, and Instructional Design and Production.

(6) Emphasis added.

[Editor's note: This is the last of our long installments about the ICR case, but we will continue to report on it. Next month, Bennetta will tell about the Department's putative plan to send a new committee to make a new assessment of the ICR.]

SIDEBAR: THE GURU SAYS NO

Are the "creation-scientists at the Institute for Creation Research really doing creation research? According to Henry Morris, the ICR's president and guru, the answer is a flat NO. Morris has pronounced that no such research is possible.

Look, for example, at his book *THE TWILIGHT OF EVOLUTION* (1963; twenty-fourth printing in 1986; available today by mail from the ICR's publishing arm, Master Books). On his page 56, Morris says: "[S]ince nothing in the world has been created since the end of the creation period, everything must THEN have been created by means of processes that are no longer in operation and that we therefore cannot study by any of the means or methods of science. We are limited exclusively to divine revelation as to the date of creation, the duration of creation, and method of creation, and every other question concerning the creation."

So much for "creation-science" and creation research. -- W.B.

SIDEBAR: THE OLD MILL STREAM

On 26 January, United Press International distributed a report about Joseph Barankin, the director of the PPED. Written by Teresa Simons, of UPI's Sacramento bureau, it told that Barankin was conducting a "romantic relationship" with Catherine Sizemore, the chief lobbyist for the trade association that represents many schools that the PPED regulates.

Articles based on the UPI dispatch ran in the "San Diego Union" on 29 January and in the "San Francisco Examiner" on 12 February. (The "Examiner" had printed on 5 February a general story, also based on Simons's reporting, about dubious schools and their doings. It included a luminous quotation in which Catherine Sizemore equated classroom instruction with suffering.)

From the UPI report, I learned of the book "Diploma Mills: Degrees of Fraud", by David W. Stewart and Henry A. Spille. I now have read it, and I recommend it to anyone who is interested in the ICR case and who wants to understand the regulatory environment that makes such a spectacle possible. Indeed, I recommend it to anyone who is interested in the integrity of education.

The authors say that California is the nation's main haven for diploma mills, and -- in their chapter called "California: A Very Special Case" -- they tell some reasons for this.

To order a copy of "Diploma Mills", send \$20 to the Mail-Order Dep't, Macmillan Publishing Co., Front and Brown Streets, Riverside, New Jersey 08075. The price includes the shipping cost. -- W.B.

A NOTE ON SCIENTISM

by Yves Barbero

Skeptics, especially those organized into groups such as BAS, are often accused of "scientism". When the term is used, it is usually misused to mean that we make a religion of science.

My "Random House Dictionary" defines "scientism" as 1. Often disparaging; the style, assumptions, techniques, practices, etc., typifying or regarded as typifying scientists. 2. the belief that the assumptions, methods of research, etc., of the physical sciences are equally appropriate and essential to all other disciplines, including the humanities and the social sciences. 3. Scientific or pseudoscientific language. [scient(ist) + ism]

The curious thing is that there are people who dislike us because of this alleged "religious" practice, and others who, on discovering that we're not structured to unquestioningly obey whatever their notion of science is, walk away in disappointment.

In recent weeks, an astrologer accused CSICOP of worshiping science in the body of a letter ("Noe Valley Voice", Dec. 1988) written in

answer to one I wrote criticizing the community newspaper for unquestioningly accepting the astrologer's statements.

In another case, a caller to the BAS electronic bulletin board was angry that we had left a bulletin on the opening screen of the Catholic Information Network BBS promoting B. Premanand's recent talk in San Francisco about the state of skepticism in India. The caller didn't think we should traffic with "miracle mongers". It struck me that he thought we were somehow anointed and that we were soiling our purity by talking to a religious group.

Much of the problem has to do with our culture, which has many carryovers from a time when it was acceptable to lay down a foundation of a premise, ideology, or religious dogma before even attacking a problem. To the novice, sound, scientific methodology is sometimes mistaken for absolute and rigid rules when it should simply mean insuring that personal prejudice doesn't interfere with research (this can require some pretty involved and precise procedures and still not need a foundation of blindly accepted premises).

It is perfectly true that skeptics "borrow" from scientific methodology, and it is unfortunately true that without proper scientific training, it's easy to fall into the trap of making skepticism an ideology or a set of dogmas closely resembling religion. Add to that the passion that often shows itself when a group is expressing a minority viewpoint, and a potent brew can develop.

Indeed, there are fanatics expressing, as dogma, what mainline skeptics only hold as tentative conclusions. Few of us, for example, think astrology or telepathy will ever be proven scientifically. But we have to recognize that our prejudice against these notions cannot stand in the way of our analysis of such claims. We are therefore careful in the way we design experiments.

Undoubtedly, more than a few people get involved with us because they want to have like-minded people to talk to (I certainly came in that way), and few of these people have formal scientific training (I didn't). They soon discover that Bay Area Skeptics is not a club as such (although friendships are made), and there is a low tolerance of any dogmatic proclamations (even those that "agree" with skepticism). It calls for a lot of self-education and self-discipline. This is too much for some, and they go on to other, more club-like organizations that comfort more than they educate.

The real goal is to understand the nature of things, to clear away the clutter of culturally imposed assumptions about the world, and to appreciate the raw beauty of nature. Mixed with an appreciation of the arts and history, this approach beats the hell out of any dogma, scientism included.

SCIENTIFIC THEORY FOR ASTROLOGY

In early January, Ms. Joan Quigley, astrologer to the Reagans, was the guest on KCBS radio in San Francisco. It was a call-in format where Ms. Quigley fed the sheep as they breathlessly waited to hear her augury.

Not all were waiting breathlessly.

I had heard earlier in the week she was going to be on, so I set the time aside and called the station even before the hour to make sure I could get in. These astrologers are popular when they hit the mike, even when they are not of Quigley's renown.

When my turn came, I read from the S.F. "Chronicle" wherein she was quoted as saying she is a "scientific astrologer", and I asked her if she could confirm that. She did, and then I asked her what, as a scientific astrologer, is the basis of astrology.

"Please tell the listening audience what scientific principle makes astrology work", I requested.

I had to pose the question three times, interrupting her tangential bilge each time. It was clear she didn't quite understand my question. I told the host that for some very simple reasons of physics, it couldn't be gravitation or tidal forces, for example. (Few astrologers assert these anymore.) Quigley agreed with that, and then she understood what I wanted.

"It's photons", she said.

I was so astonished I could not collect myself for a few seconds. "Photons?" I asked in stunned disbelief.

"Yes, photons", came the unequivocal affirmation.

I could feel the host's finger on the line switch -- he had already spent a fair amount of time on my call. I wanted so much to make her follow some of the consequences of such preposterous drivel. Before the fickle finger of the engineer flicked the switch, I was only able to remind her that the planets are not emitters of photons, but only reflectors; and very poor ones at that, especially in the case of the astrologically powerful Pluto.

At least I had been successful in getting her to take a specific stance. If there is a theory -- any theory -- for some phenomenon, there is something to bite one's teeth into. For example, the most powerful source of photons during a birth is likely to be the bright lights in the delivery room. There are as many other reasons why a photon theory is absurd as one can imagine in only five minutes of reflection.

It might be interesting to hear other astrologers defend this

proposition of one of their more illustrious colleagues, particularly since Joan is a scientific (she uses a computer) astrologer. Ask your local zodiacal wonder about Joan's photon theory and let us know what happens. -- Ed.

RAMPARTS

["Ramparts" is a regular feature of "BASIS", and your participation is urged. Clip, snip, and tear bits of irrationality from your local scene and send them to the Editor. If you want to add some comment with the submission, please do so.]

Haven't you always wondered why the government is covering up its UFO investigations? Barry Greenwood, author of "Clear Intent: The Government Coverup of the UFO Experience", has plenty of good reasons, according to a report in the "Malden News".

"When you are charged with protecting the U.S., and you have these things flying around, it's very difficult to shoot at them", said Greenwood. "When you cannot deal with the phenomenon, the last thing you want to do is to admit it. It could cause a full-scale investigation by Congress into why the Armed Forces aren't equipped to deal with this kind of thing. It saves a lot of embarrassment to debunk the phenomenon."

Thus, CSICOP and its minions are merely tools of the government conspiracy. Greenwood has more. The feds are learning a lot of technical stuff about these alien ships, and the best way to keep it out of enemy hands is to deny that the whole thing exists. The idea is that what the Russkies don't know will hurt them. He even suggests that our stealth bomber technology was pirated from studying UFOs. The aeronautical engineers who expended all that effort in R&D and production would probably like to thank the aliens for the work it saved them.

Barry feigns a scientific approach to UFOs when he says, "Only after you have exhausted all possibilities can you call it a UFO." It is obvious to Barry that we have exhausted all the possibilities, and must resort to an extraterrestrial explanation.

The "Malden" article took a rare but welcome turn as it mentions CSICOP and quotes from a response by UFO expert Phil Klass, in which he raises a very interesting proposition: If someone is abducted, what is standard operating procedure? First, go to the police. The police then contact the FBI, being as this is their domain, and they MUST investigate. But if the FBI finds that the person has filed a false abduction report, the person can go to jail for up to five years and pay a fine of up to \$10,000.

"The reason", says Klass, "that no one has ever reported a UFO abduction case to the FBI is that they are afraid they'll end up going to jail. If the Hezbollah came into your house, abducted and

impregnated your daughter, you certainly would report it to the authorities", concluded Klass.

Opinions expressed in "BASIS" are those of the authors and do not necessarily reflect those of BAS, its board or its advisors.

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