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Editor: Kent Harker

## ONWARD CHRISTIAN HEALERS by Kent Harker

[The claims of supernatural healing have long been a subject of skeptical interest. Nearly all religious societies have healing rituals and concomitant stories of miraculous recovery. There is usually an effort to protect the entire practice from careful scientific analysis lest what is considered sacred be drawn into profaning scrutiny. Thus it is indeed rare for someone with scientific credentials to offer up something more than the anecdotal evidence to which we have become inured.]

I have an acquaintance who is ever on the lookout for ways to enlighten me. When people learn one is a skeptic, he or she often becomes something of a target for other's boy-have-I-got-something-to-show-you tidbit. An article, report, or a "someone said" is gleefully stuffed in our face with a taunt, "Just explain THAT!" Well, this person is a believer in Christian healing. I explained that a genuine healing would likely be a testable claim, and that in spite of innumerable anecdotes I remain unconvinced.

In July of last year he came into possession of a 1982 newspaper clipping, which he mailed to me, reporting that a San Francisco medical doctor had clinical proof that prayer aided the healing process. He was nonplused when I said I put almost no credence in newspaper stories.

"But this is a report from a medical doctor at a recognized hospital!" he insisted. "That's proof!"

My reply that I had to see the actual study before I could make any kind of judgment left him slack jawed. He vowed he would get the report. I didn't hear from him much during the ensuing five months, but I knew he was digging away, trying to trace the story down. In early December he called me. There was a touch of smugness in his voice as he told me he had finally found the doctor in question and that he had a copy of the study in his hands. I shortly had it in mine.

The report, "Positive Therapeutic Effects of Intercessory Prayer [IP] in a Coronary Care Unit Population," was published in updated version in the "Southern Medical Journal", vol. 81, No. 7, July 1988. The author and researcher, Dr. Randolph Byrd, MD (himself a fundamentalist Christian), conducted his study at San Francisco

General Medical Center in the coronary care unit over a period of 10 months (from August 1982 to May 1983).

The study drew 393 patients, 192 in the study group and 201 in the control group. The conclusions of this work apparently stirred national attention and were mentioned on Paul Harvey's broadcast.

During the ten-month period, Dr. Byrd requested all patients in the CCU to participate in the study, a total of 450. Fifty-seven (14.5%) declined for various reasons. Those who agreed to take part were told the nature and purpose of the study, and they signed mandatory informed consent agreements. A random generator then assigned each patient to either the study group (those for whom IP was to be offered in addition to regular procedures) or to the control group (those receiving only "traditional" therapies), and both the medical staff and patients were blinded as to the membership of each group.

Finally, Byrd chose "intercessors" on the following basis: They were "born-again Christians (according to the Gospel of John 3:3) with an active Christian life. . . ." These intercessors were then randomly assigned a patient in the study group for whom they were to pray daily, outside the hospital, until the patient was discharged.

I haven't taken the time to do in-depth analyses of Dr. Byrd's statistical rigor -- I will assume it is adequate. At least it seems to be upon cursory examination. (Note: At the time of this writing, other researchers with whom I have had contact are looking into Byrd's statistical presentation. Indications are that those analyses will be published in the summer issue of "Free Inquiry", journal of the Humanist Association.)

Normally "BASIS" does not enter the arena of theological questions, but conditions of Dr. Byrd's experiment merit setting that policy aside because he presents it as a scientific study with theological ramifications.

If one conducts a double-blind study on the efficacy of a drug, a direct cause-effect relationship is obtainable, and the researcher looks for consistency in that relationship. The method of application and strength of the dosage can then be modified and studied as the active agents are pinpointed.

We will see that Dr. Byrd's study purports to show that prayer results in more rapid improvement, significant in six of twenty-six coronary conditions (see Table p. 3). But there is no way to recognize the affect of prayer on a SPECIFIC condition. The first question I asked myself is why those specific six for which this study showed significance? I have little doubt that if Byrd repeated the study, some other group of conditions would be affected.

In other words, there would be little or no correlation between IP

and which specific symptom or condition should improve. For this and other reasons I must consider the theological ramifications.

There are six references in the New Testament which indicate it is the faith OF THE PERSON BEING HEALED that is the operant (e.g., Mark 5:34). On a specific occasion, even the most powerful miracle worker, Jesus, was unable to effect a miracle because of the lack of faith of His hearers: "And He could do no miracle there. . . . And He marveled because of their unbelief. (Mark 6:5-6)"

What are the general HUMAN developments of intercessory prayer? What does it mean that God may choose to heal some members of the study group -- in an apparently random fashion if Byrd's sample is statistically valid -- for whom IP is invoked? Since the study and the control groups were randomly chosen it is just as likely that non-born-again Christians in the study group were the beneficiaries of healing.

Does the fact that born-again Christians were the intercessors have any implications about the efficacy of mainline Christian and non-Christian pleadings for God's mercy? (Jews, Moslems and Catholics assert that God heals and saves them.) Can the alleged healing effects of IP be separated from these moral questions?

Now for some problems I see in the protocol. The intercessors prayed outside the hospital and it was not established in the study what other outside contact patients had with loved ones and other support groups. What about the prayers we can imagine were offered by family and friends on behalf of some in the control group?

Many studies show that close contact of a large support group is beneficial for obvious (non-religious) reasons. Byrd's failure to monitor or consider the results of this variable is a flaw.

Looking over the data, (Dr. Byrd did a scientifically commendable job of presenting the data -- even that which does not entirely support his conclusion) there are yet other questions that greatly trouble me, again from the humane perspective. The six conditions to improve to any statistically significant degree were, in order of highest significance, (1) intubation/ventilation, (2) antibiotics, (3) cardiopulmonary arrest, (4) congestive heart failure, (5) pneumonia, and (6) diuretics.

In other words, IP was most likely to lessen patient's time on intubation devices and to reduce the amounts of antibiotics they needed. Two in the study group suffered cardiopulmonary arrest while seven of the controls did. While those in the study group were ten times more likely to enjoy the considerably less-desired results (1) and (2), I want to ask myself a flood of questions about those who benefited from condition (3).

What about the two poor buggers for whom prayer was offered but they fell anyway? How were they chosen for their fate? If a particular medication is being tested there are no moral questions

involved in partial or total failure. What did seven (randomly-chosen) misbegotten souls do wrong to find themselves in the wrong group (the controls) by the fall of the dice that resulted in their suffering condition (3)? Is it inconceivable that there were some of them for whom family and friends, NOT in the intercessors, prayed? Does this suggest that only the prayers of the designated intercessors are potent?

Why does God seem to have a preference of cures He chooses to confer (getting His subjects off antibiotics 8.5 times more likely than stopping their congestive heart failures)? The preference seems to extend to ignoring third-degree heart block, mortality(!), coronary angiograms, and fifteen of the other conditions. Why is unstable angina and the incidence of permanent pacemakers less frequent (although not statistically significant) in the control group?

Based upon the experiment, the general improvement of the study group was better. This neglects the most important aspect of the INDIVIDUAL. Again, this is a moral question, and only has validity when a moral agent -- God -- is allegedly part of the cause.

Behind these nameless statistics are the lives of human beings stricken with life-threatening maladies and the human tragedy wreaked on their own and in the lives of their families. What does this study say about them? That they are less worthy? If the randomization process was accurate, the control group should have contained the same proportion of born-again Christians as the study group; what about them?

Other controlled, replicated studies have been done, studies that eliminate some of the bias I see in Dr. Byrd's work. In one such, patients were told that a prayer group was interceding in their behalf in an adjoining room. In a statistically significant proportion, they improved as compared with a control group which was told nothing.

In fact, there had been no such intercessory effort. When the test was reversed, i.e., an intercessory group prayed away in an adjoining room AND THE PATIENTS KNEW NOTHING ABOUT IT, there was no significant difference in the patients' conditions. This study seems to show that intercessory prayer works, but not for the reasons that Dr. Byrd might like to think, I'm sure.

I am unwilling to accept the oft-quoted reason -- no, excuse -- "God works in mysterious ways."

It is well established that the most nearly universal drug is the placebo. Roughly 35% of all patients respond to a placebo, be it psychic surgery, faith healing, sugar pills, hypnosis, or an unimaginable host of other preparations, nostrums and incantations.

How did Dr. Byrd control for the placebo effect?

TABLE

Study Variable	St.	Cn.	P
Days in CCU after entry	2.0	2.4	-
Days in hospital after entry	7.6	7.6	-
Number of discharge medications	3.7	4.0	-

Problems After Entry	No.	No.	P
Antianginal agents	21	19	-
Antiarrhythmics	17	27	-
Antibiotics	3	17	.005
Arterial pressure monitoring	7	15	-
Cardiopulmonary arrest	3	14	.02
Central pressure monitoring	6	15	-
Congestive heart failure	8	20	.03
Coronary angiography	17	21	-
Diuretics	5	15	.05
Extension of infarction	3	6	-
Gastrointestinal bleeding	1	3	-
Hypotension	3	7	-
Inotropic agents	8	16	-
Intubation/ventilation	0	12	.002
Major surgery before discharge	5	14	-
Mortality	13	17	-
Permanent pacemaker	3	1	-
Pneumonia	3	13	.03
Readmissions to CCU	14	14	-
Sepsis	4	7	-
Supraventricular tachyarrhythmia	8	15	-
Temporary pacemaker	4	1	-
Third-degree heart block	3	2	-
Unstable angina	20	18	-
Vasodilators	8	12	-
Ventricular fibrillation	14	17	-

(P= Significance, St. = Study group; Cn. = Control group; No. = number of patients, "-" denotes not statistically significant. P is the probability of chance occurrence: P <= .05 is significant.)

KNOWING WHEN NOT TO BELIEVE THE UNBELIEVABLE  
by Wallace Sampson, MD

In early July, 1988, a "Newsweek" reporter me called to write an analysis of the "Nature" article titled "When To Believe the Unbelievable," in which Benveniste and associates claimed to have demonstrated the validity of homeopathy.

The data in the Benveniste report were strange, and the conclusions were indeed unbelievable. Dilutions of an antibody to concentrations of 10 to the minus 60 and of 10 to the minus 120 had the same effect on basophil (a type of white blood cell) degranulation as did the optimal concentration of about 10 to the

minus 3.

The conclusions seemed to be consistent with homeopathic theory and practice, two principles of which are: 1) The principle of "Similia similibus curantur", or "like cures like", in which one uses substances that induce symptoms of the illness rather than to use drugs that counter those symptoms, and 2) the principle of dilution, according to which the paper states that "solutions of a substance so dilute that no molecules of it likely remains, are effective remedies for numerous disorders."

If no molecules remain, what is left to impart activity? The authors admitted that at the dilution of their antibody solution (a molar concentration of  $2.2 \times 10^{-20}$  M) no molecule of the substance is likely to be present in the small amount tested. Chemical laws state that the more dilute the solution of a substance, the less the solution's activity, with certain exceptions (enzymes and their substrates often have optimal concentrations that may be specific for each set, determined by experiment).

But they explained their results by the usual homeopathic argument that dilution and a ritual of shaking imparted potency to the water: Water "remembered" the quality and imparted activity to the substrate -- in this case, basophil granules -- which released histamine on contact with the water. The authors claimed that the experiments were authentic, and that they were repeated in four separate institutions in four countries.

As I analyzed the data I was bothered by periodic peaks and troughs of activity that appeared with increasing dilutions of the test material; I learned that these were typical of homeopathy experiments. These peaks and troughs should have represented random variation and experimental error, but they seemed to be too regular.

I could not determine with certainty what was wrong with the methods, so I speculated on how such results might have been obtained. When I spoke with the "Newsweek" reporter again, I told her that there were four possibilities, two natural, one unnatural, and one supernatural.

One natural explanation is that the experiments were authentic, and that the results occurred by chance. The likelihood of just one experiment showing such activity by water alone is unrealistic. The chances of four sets of experiments showing the same results are too small to imagine.

The second natural explanation is one of a systematic or human error in method, unknown to the investigator, that was repeated by all four institutions. Perhaps the same person performed the experiments or a crucial part of them in each. (I subsequently found out that the same person did perform the experiments at least in France and Israel.)

It is possible for the same pipettes to be used repeatedly, and, depending on the way the dilutions were set up, for there to be contamination of successive tubes in a periodic fashion. There could be contamination of successive tubes in a periodic fashion. There could be conscious or unconscious bias in setting up and performing the experiments or in their analyses.

There might have been error in reading the samples for degranulation. The method is not quantitative and depends on personal interpretation. Sometimes basophils degranulate partially. Staining varies from day to day because of changes in pH, temperature, etc. (The authors should have selected a more quantitative test with automatic recording devices such as the uptake or discharge of a radioactive labeled substance. There are hundreds of such systems. Instead, they selected the basophil degranulation method, which, if not tightly controlled, could be misinterpreted.) At any rate, I included these possibilities under systematic errors.

The unnatural explanation was that the experiments were faked. Someone could have spiked active material into the wells in which the cells were incubated, with or without the knowledge of the main author. Perhaps someone toyed with the raw data and fudged and plotted them according to a preconceived notion. Such things have happened before. The supernatural explanation was that the whole thing was true, so we have a new universe to deal with.

When the "Newsweek" article appeared my skeptical comments were not included. The article ended with a statement that perhaps Benveniste was really on to something and that the medical world should pay attention to the possible validity of homeopathy. However, a three-member team from "Nature" would go to Paris to observe the researchers perform the experiments.

The team's report was published in "Nature" a month later. They found a combination of loose or non-existent controls, possible equipment contamination, data manipulation, and data selection (keeping positive results and rejecting the negative). When the experiment was run under their strictly monitored controls, the results were negative. A series of letters to "Nature" was published with critiques, and with negative results from other labs.

This question remained: by a critical reading of the original paper, could one show the claims to be invalid? Analysis of the study shows that even if the experiments and results were authentic, 1) they are unreproducible, thus of no use to homeopathic practice and, 2) the results suggest that homeopathy is more likely to worsen a patient's condition than to heal.

The first clue to unreproducibility is in the third paragraph of the Benveniste report: ". . . similar results were obtained AT ONE OR THE OTHER PART of the high dilution scale in the participating

laboratories." If the experiment were reproducible, the specific dilutions would have been consistent at all parts of the scale from one lab to another. This hints that the peak activities reported might be random.

Later in the paragraph is the statement: "The repetitive waves of anti-IgE-induced degranulation COULD SHIFT BY ONE OR TWO DILUTIONS WITH EVERY FRESH SEQUENTIAL DILUTION OF ANTI-IgE AND DEPENDED ON THE BLOOD SAMPLE." Each dilution in homeopathy is usually ten-fold. The mean number of dilutions between each peak and the adjacent trough in the paper's Fig. 1 was 3.94 dilutions.

If the peaks and troughs of activity could vary by one or two dilutions either way, the peak value of activity in one run could shift half way toward a trough value of the next. Since the authors imply that the variation pertains to the same solution in sequential runs, predictability from any single solution is impossible.

In other words, a homeopath might "prove" a specific dilution to be effective in a patient once, but could not be certain of the same effect at the time for the next treatment. One would have no way to determine what dilution to use from one time to the next. The problem was not recognized or was ignored by the authors.

The second problem is the conclusion that these results support the theory and practice of homeopathy. The authors state: "These results may be related to the recent double-blind clinical study of Reilly et al. which showed a significant reduction of symptoms in hay-fever patients treated with a high dilution (10 to the 60th) of grass pollen vs. placebo. . ."

However, the results show that very dilute "solutions" (actually, water only) seem to produce the same effect as solutions of optimal concentration of material. If the water still shows effects quantitatively the same as those of concentrated solutions, it should reproduce quantitatively the symptom of the illness.

In this case, the water causes just as much histamine release from basophil granules as an optimal amount of an allergy-causing substance. Hence the treatment solution would cause just as much asthma or hay fever as that produced by maximum stimulation by the allergen material. One must conclude that the results paradoxically support the view that if homeopathic treatment "works," it must worsen or prolong the illness.

On a practical level: Because homeopathy has never been proven, analysis of the Benveniste paper supports the skeptical view that homeopathic solutions are in reality ineffective, that the results of these experiments probably have other explanations such as equipment contamination or misinterpretation of data, and that any improvement in symptoms from homeopathic solutions is probably from placebo effect or suggestion.

## CAUTION: PSYCHICS AT WORK

by William Bennetta

[Among psychics, pseudoscientists and other purveyors of magic, few things are coveted more keenly than the respectability that such people may get by linking themselves, however briefly or tenuously, to respectable institutions. This article tells of a recent coup by a fortune-teller named Robert Willhite, who succeeded in exploiting the California Academy of Sciences for dignifying and propagating his claims about "clairvoyant abilities." Our author, William Bennetta, is an advisor to BAS, a fellow of the Academy and a research associate of the Academy's best-known division, the Steinhart Aquarium.]

Robert Willhite seems to be an ordinary fortune-teller. His routine is built on the reading of rune-stones -- small stones decorated with various abstruse symbols -- but his essential methods are evidently the same ones used by palm-readers, astrologers, crystal-gazers and the other practitioners of divination.

After looking at some stones and then sinking into a "trance", Willhite can tell a client that "many times, as a child, you had to ask permission as to what you could have," and that "you have a tendency to want to get things just right." He can even sense that a client's adolescent son "has a tendency to like to have people give to him," and that client and son have "known each other before, in past lives." In short, his routine is both pedestrian and transparent.

In one way, however, Willhite is unusual, and he can point to an extraordinary commercial achievement: About two months ago, he ran a public fortune-telling session at the California Academy of Sciences, used the Academy for disseminating his claims to clairvoyant powers, and even used the Academy for advertising a commercial organization that steers prospective clients to astrologers, numerologists, channelers, psychic healers and the like.

Willhite's achievement was an embarrassment to the Academy, and I take little pleasure in describing it here. I think, however, that my report may be useful to other academic and scientific institutions that offer public programs, and may help them to avoid comparable embarrassment.

For about six years, the Academy's Department of Anthropology has had a Traditional Arts operation that sponsors public demonstrations of folk arts and crafts, such as vocal music, instrumental music, dance, story-telling, wood-working and cooking. Several of these programs are presented each month, with emphasis on arts and crafts from sources outside of conventional Western culture.

Whether the participants are professionals or amateurs, they usually are evaluated by a representative of the Department of Anthropology, who appraises not only the content but also the cultural significance of the proposed performances.

In the case of Robert Willhite, the usual procedures evidently did not work. I cannot now reconstruct all the steps by which Willhite got onto the Traditional Arts schedule, but this much is certain: He was recommended to the Academy by a functionary of DeJa Vu Hotline.

In retrospect, it seems obvious that the very name of that organization was a warning to stay away -- or, at least, a warning that the Academy would have to conduct an especially careful evaluation of Willhite and of the "art" that he wanted to demonstrate. Alas, the warning was not recognized.

Deja Vu Hotline is a "psychic referral service." Its advertising handbill offers "psychic readings in person or by telephone" and then shows a somewhat confusing list of the specialties with which Hotline psychics deal: "clairvoyant, business tarot, past life, astrology relationship, body weight, pet career, numerology, and more!" There are also "channelers" and "healers."

The Hotline is one of several enterprises, all having "Deja Vu" in their names, that are headquartered in San Rafael and are linked to the Berkeley Psychic Institute (BPI), a unit of the Church of Divine Man (CDM). Graduates of BPI typically call themselves "Reverend" and depict their operations as religious activities.

The central figures in BPI, in CDM and in the DeJa Vu businesses seem to be Lewis S. Bostwick and Susan Hull Bostwick; in some of their roles, these two use the titles "Very Right Reverend Doctor" and "Right Reverend," respectively. Besides the Hotline, the DeJa Vu businesses include DeJa Vu Publishing Company (which issues a monthly tabloid called the "Psychic Reader"), DeJa Vu Tours (a travel agency selling "worldwide adventures for psychics") and DeJa Vu Wedding Services.

Willhite's appearance at the Academy was set for the afternoon of 29 April (a Saturday) and was announced in the April issue of the "Academy Newsletter", a bulletin sent to all the Academy's members. The announcement said: "Robert Willhite demonstrates his clairvoyant abilities in interpreting runes -- ancient Scandinavian symbols that predate Christianity and are used as a spiritual communication tool. Willhite will also discuss the history of these mystical signs and their secret powers."

The performance, which drew some 100 people, took an hour and had three major parts. For the first 35 minutes or so, Willhite gave an incoherent, quasi-historical talk about magical beliefs and symbols. His "history" was fatuous, and his style seemed not merely credulous but promotional: He seemed to be trying to validate soothsaying and magic by conveying the idea that if something is

old, and has been revered by ancient peoples, it must be right and true.

The second part of his program, taking some 20 minutes, was his "demonstration" of rune-reading and of his psychic powers, which he exercised on behalf of two people from the audience. One of these, a woman, was the poor creature who (as Willhite magically detected) had suffered a childhood in which she had had to seek permission before having things. The other was the father whose young son (as Willhite uncannily sensed) liked to have people give stuff to him. Marvels indeed! The audience applauded vigorously.

The last part of the show was advertising. After inviting the audience to contact him for "professional readings", Willhite introduced Pat King, the woman who runs Deja Vu Hotline. King promoted the Hotline and its services, including magical healing "for yourself or for a relative, and, believe it or not, even your pets." Then she invited the audience to take the Deja Vu handbills and business cards that were stacked nearby.

This ended the program. There was no time allowed for questions or comments from the audience.

I have discussed the Willhite incident with officials of the Academy, and I believe that their views can be summarized in four statements. First: The Academy did not investigate Willhite properly and did not know that his "art" consisted of fortune-telling. Second: The Academy has seen no evidence that Willhite or anyone else has "clairvoyant abilities," and no evidence that symbols have "secret powers."

Third: The Academy does not endorse, and never has endorsed, Willhite or his claims or his business. Fourth: The Academy intends to ensure that its Traditional Arts program will not again give a platform to a pseudoscientist, a soothsayer or a sorcerer. This applies to the commercial soothsayers who now are rampant in our own culture, and it applies to people who may try to sell occult rituals from exotic sources.

## DEGREES OF FOLLY: PART V

by William Bennetta

The first four parts of this article ran in "BASIS" in February, March, April and May. They told how the Private Postsecondary Education Division (PPED) of the California State Department of Education, in August 1988, staged an "assessment" of the ICR Graduate School (ICRGS). The school is an arm of the Institute for Creation Research, a fundamentalist organization that disseminates the pseudoscience called "creation-science."

The assessment was made by a five-man committee, chosen and managed by a PPED officer named Roy Steeves, that included two ringers --

two men who had had close associations with the ICR or with the ICR's president, Henry Morris. The committee wrote a false, misleading report saying that the Department's chief, Bill Honig, should approve the ICR as a source of masters' degrees in science and in science education.

Later, however, two of the committee's legitimate members told the truth about the ICR; and Honig -- at least in statements that he gave to the newspapers last December -- refused the approval. But in January the Department drew back from that decision and began to negotiate with the ICR.

I shall describe here the results of those negotiations, after I make some final comments about the antics of Roy Steeves. I assume that my readers have seen all the earlier parts of this article.

-- W.B., 11 June

## MORE ABOUT MILLER

In Part IV, I told a little about G. Edwin Miller, one of the ringers whom Roy Steeves had named to the committee; and I wondered whether Miller had been recommended by the ICR. Here is why this seemed important: The Department already had admitted that the other ringer, George Howe, was "ICR's nomination"; but the Department also had said that its "standard policy" allowed only "one" such nomination by a school undergoing assessment.

I now know that Miller, too, was an ICR "nomination." Like Howe, he was one the people whom Morris had recommended in a letter sent to the Department on 7 June 1988.

## PROMO BY MEMO

I saw Morris's letter a few days ago, when I went to Sacramento and examined the PPED's whole file on the ICR case. It had many engaging documents, but none more engaging than the memoranda in which Steeves -- writing to the PPED's director, Joseph Barankin - - seemed to promote the ICR, the ICR's positions, and the ICR's pseudoscience. For example:

- On 23 May 1988, before he began to recruit the committee, Steeves sent a memo in which he summarily declared that "they [the ICR men] ARE scientists" and then said: "this group believes that the universe is decaying from an original creating event. That cosmology is remarkably similar to what they are saying at Cal Tech. In the Biology program the underlying religious belief is that mutation is occurring away from an original creation. At the same time evolutionary theory is generally accepted in the biological sciences, so is devolutionary [sic] theory accepted and particularly in the study of mutations, which seems to be one of their [WHOSE?] specialties."

- On 8 August, three days after the committee had written its report, Steeves sent a memo that urged approval. Two days later,

he sent another. The second memo warned Barankin that Stuart Hurlbert would be submitting "a letter which in his opinion is a minority and dissenting opinion to the visit report."

- On 29 August he wrote: "There is nothing in Dr. Hurlbert's report that I can see that was not discussed at one time or another during the course of the Committee meetings at the school site." (He did not suggest why so great a mass of material, if it had been "discussed" by the committee, was not acknowledged in the committee's own report.) Then he accused Hurlbert of "creating a series of straw men."

- On 1 September he wrote that he was "appalled" by a comment in which (he inferred) Hurlbert had questioned the motives of G. Edwin Miller. Then he said: "Dr. Miller was not there as an expert on science curriculum. He was there as an expert on school finance." (Steeves did not tell that Miller, whatever the reason for his presence on the committee, had VOTED on the ICR's "science" curriculum.)

Later Steeves announced that "These [ICR men] are quite capable of teaching science and they do so." (He did not disclose how he had learned that.) Finally he declared: "This thing is a dispute between theists and atheists. . . ."

So there it was: the ICR's "two- model" stuff, neat and pure. People who saw the ICR's charlatanry for what it was, and who objected to the state's certifying it as science, were categorically "atheists."

Did Steeves really believe what he wrote? Did he really think that Caltech professors were teaching a cosmology in which modern physics was summarily rejected and in which the universe was only 6,000 years old? Did he really think that modern biology had a "devolutionary theory" that figured in "the study of mutations"? I do not know.

## STILL AT IT

Roy Steeves is still on the Department's payroll, and -- as I told in Part IV -- the Department has undertaken a cover-up that includes an effort to justify Steeves's conduct. Right now, the chief element of the cover-up seems to be a plain refusal to answer mail. During the past two months, several people who are following the ICR case have sent inquiries to Bill Honig, including inquiries about the matter of G. Edwin Miller, but to no avail. One such letter was dispatched on 6 April and still has not been acknowledged.

## WHAT WILL HAPPEN NEXT?

After the Department, in January, abandoned its announced decision to deny approval, Joseph Barankin made a kind of agreement with the ICR's lawyer, Wendell Bird. I say "kind of" because the agreement

is so burdened by undefined terms, unspecified conditions and unanswered questions that it does not seem to be a respectable effort. It is embodied in two letters -- one sent to the Department by Bird on 10 January, the other sent by Barankin to Bird on 3 March.

In April, after studying the letters, I sent to Barankin some questions about their content. He has not replied. Here is my own, unaided reading of the major points in his deal with the ICR:

- The ICR says that it will revise its "science courses" and "science curriculum," conforming them to science courses and curricula at accredited schools. During this effort, "ICRGS's interpretations" will be removed from all courses that will carry credit toward science degrees. "Interpretations" will be confined to courses or activities that will not count toward degrees.

Barankin, then, has accepted two bizarre propositions. One is that natural sciences, and science courses at accredited schools, exist as mere piles of information, unsullied by interpretation or thought. The other is that the ICRGS, which is explicitly a creationist "ministry," really intends to excise creationist "interpretations" from its degree programs and intends to relegate creationism to some peripheral diversions.

- To learn whether the ICR has made the contemplated revisions, the Department will dispatch a new committee. One member will be selected (not merely suggested or recommended) by the ICR. The total number of members is not specified.

- The new committee will examine the ICR's programs in biology, geology and "astro/geophysics," but not the program in science education. That program evidently will get another free ride, like the one that was awarded to it, last August, by Roy Steeves.

In my April query to Barankin, I asked whether, in making the agreement, he had had advice from anyone who knew about science. I also asked: If the Department were to approve the ICRGS's interpretation-less courses, how much would the Department have to spend annually to monitor the courses and to ensure that no "interpretations" were creeping in? I am sorry that Barankin refused to answer.

The ICR has asked the Department to conduct the new examination by early August. I do not know whether the Department has yet picked a date or has chosen any members for the new committee.

#### SIDEBAR: CATHY AND JOEY AND S.B. 190

S.B. 190, State Senator Becky Morgan's bill that would reform the regulation of unaccredited schools operating in California, has been endorsed by both the Senate Education Committee and the Senate

Appropriations Committee. The bill would create a new agency for controlling unaccredited colleges and vocational schools, would remove that function from the Department of Education, and would abolish the PPED.

The Education Committee approved S.B. 190 on 3 May, by a vote of 9 to 0, after a brief hearing. A report of the committee's proceeding, written by Diane Curtis, ran in the "San Francisco Chronicle" on 4 May, under the headline "'Diploma Mill' Bill Advances." Here is an excerpt:

Catherine Sizemore, lobbyist for the California Association of Private Postsecondary Schools, led the opposition, which was joined by 17 leaders of unaccredited schools.

Sizemore said her organization shared Morgan's concerns, but disagreed that the best way to achieve reform was to take authority away from the present regulator, the [PPED], and create a new state agency.

Sizemore, who has made no secret of her live-in relationship with [the PPED's] director, Joseph Barankin, said the regulators have been hamstrung by lack of staff and money to oversee the schools. Rather than create a new agency, she said, [the PPED] should be given a chance to implement reforms approved by the Legislature in the past five years.

I find irony in Sizemore's effort, for I think that she herself - through her relationship with Barankin, and its insistent suggestion of a conflict of interest -- has done much to promote the legislation in question. Bill Honig has known for months about Barankin's affair with Sizemore, who represents many schools that the PPED presumably oversees; and by tolerating the appearance of ethical conflict, Honig seems to have said that the Department of Education has no interest in straightening the PPED out. I speculate, then, that some senators will see S.B. 190 as the only practical way to achieve reforms.

A representative of the Department observed the Education Committee's hearing but did not testify. Later in May, the Department began active opposition to the bill.

The Senate Appropriations Committee approved S.B. 190 on 12 June, by consent -- that is, without a debate or a vote. (This was possible because the bill, if enacted into law, would have no significant effect on the state's general fund.) S.B. 190 now will be considered by the full Senate. -- W.B.

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