January 11, 1991

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Dear William:

Now that I have had some time to reflect upon Battin's claimed gravity-propelled Earth-Venus-Mars-Earth trajectory research project in 1956-57, I want to put my response on paper for the record. As you indicated during our meeting with Dowling on January 5, Battin is now claiming (after 33 years of silence) that in 1956 he and several co-workers were engaged in research involving the numerical determination of one-way interplanetary trajectories to Venus. One of these researchers indicated that if a free-fall vehicle approaches Venus in a certain way, the resulting gravitational perturbation would cause the vehicle's orbital energy with respect to the sun to increase, and that this perturbation would cause the vehicle's orbit to pass beyond the orbit of Mars. You indicated that Battin said that he then utilized this effect to formulate analytically gravity-propelled multiplanet Earth-Venus-Mars-Earth trajectory profiles and subsequently discovered the existence of such trajectories for 1966 and 1972 launch windows.

In particular, you indicated that, according to Battin's statements, he carried out this research project by himself during the 1956-57 time period and performed the following work:

1. The analytical formulation of gravity-propelled multiplanet patched conic Earth-Venus-Mars-Earth trajectories in three-dimensional space;

2. Programming this analysis on a large IBM digital computer for numerical processing; and

3. Conducting a numerical investigation of gravity-propelled Earth-Venus-Mars-Earth trajectories, which resulted in his discovery of the 1966 and 1972 launch opportunities.

My comments are as follows:

1. If this story were true, why didn't Battin publish this research or mention it (or some of the results) in subsequent publications? Moreover, a serious research project on this scale -- which involved three-dimensional trajectory analysis, programming this analysis for computer processing, and using a large IBM digital computer -- required a considerable effort, not to mention funding to support the use of a large digital computer. (Any trajectory engineer who was doing work during that time period will tell you that conducting such analysis, programming it on a computer with a planetary ephermeris, and then debugging it would take months.) It is highly unlikely that a professional

researcher would conduct such research with the expenditure of that amount of effort and not report it in some publication, even if the research led to negative or unimportant results. But in this case, Battin's claimed research project led to significant discoveries. The 1972 Earth-Venus-Mars-Earth trajectories are only 460 days long, required relatively little launch energy, and offered the truly remarkable possibility of reconnoitering two different planets during one mission. These trajectories were very significant and had a tremendous impact on the possibility of carrying out manned interplanetary missions. They became known in the literature as the "Venus swingby mode for manned missions to Mars." (See refs. 1-4.) What is also unusual about his claim is the time frame. At that time free-fall multiplanet trajectories were, for all practical purposes, unknown in the technical literature.

2. In 1959 (about one year after completing his claimed Earth-Venus-Mars-Earth gravity-propelled trajectory research), Battin did publish a lengthy paper on simple round-trip free-fall trajectories to Mars of the form Earth-Mars-Earth (ref. 5). If Battin's claimed Earth-Venus-Mars-Earth research project were true, it is inconceivable that Battin could have written this 1959 paper describing the very long trip times required for Earth-Mars-Earth trajectories (exceeding 1,100 days) as unavoidable because of the basic dynamics of the Solar System, while not mentioning the fact that these trip times could be reduced by more than one-half by employing low launch energy multiplanet trajectories of the form Earth-Venus-Mars-Earth. (See Table 1, page 558 and page 566 of ref. 5.) Moreover, unlike the simple Earth-Mars-Earth round-trip trajectories, the multiplanet Earth-Venus-Mars-Earth trajectories offer the remarkable possibility of passing two different planets in a single mission instead of only one. Do you think that he didn't mention these remarkable free-fall round trip multiplanet trajectories in his 1959 paper because he wanted to keep them a secret?

3. According to your description of Battin's claimed Earth-Venus-Mars-Earth trajectory research project, this project was itself based upon the claim that during the course of an earlier research project in 1956, it was noted that the orbital energy of a free-fall space vehicle can be changed without rocket propulsion relative to the sun by a close planetary encounter. If this story were true, why did Battin express surprise when he noted a change in orbital energy in his computed round-trip Earth-Mars-Earth trajectories in 1959? He described this change as a "curious fact". (See paragraph 2, page 566, ref. 5.) Clearly, this statement can be logically explained only if Battin believed that orbital energy should remain a constant regardless of any planetary encounter.

4. If Battin was aware that it is possible to send a vehicle beyond the orbit of Mars via gravitational perturbations resulting from an indirect transfer to Venus that requires less launch energy, why did he regard the classical direct-transfer Hohmann trajectories as representing the absolute minimum energy trajectories for interplanetary space travel and supported this belief with detailed mathematical demonstrations? (See pages 554-555, and Fig. 2-3 of ref. 5.)

5. Battin's claimed Earth-Venus-Mars-Earth research project in 1956-57

involved the use of gravitational perturbations to reduce the launch energy requirements and was therefore unlike Crocco's Earth-Mars-Venus-Earth constant elliptical path multiplanet trajectories, which did not employ gravitational perturbations to reduce the launch energy requirements. Therefore, if Battin's claimed research project was true, it represented the first research project involving gravity-propelled trajectory designs. But Battin himself credits Crocco for originating the concept of gravity-propelled trajectory design in 1956, only one year before his claimed work. (See ref. 6.) Why did Battin credit Crocco in 1978 as originating the concept of gravity-propelled trajectory design, which was not true, in light of his claimed 1956-57 research project?

6. Battin acted as a technical advisor to Walter Hollister for his dissertation research in 1963. (See page iii of ref. 7.) According to Hollister, the Earth-Venus-Mars-Earth multiplanet trajectory played a key role in his dissertation. (See pages i - ii, ref. 7.) If Battin's claims are to be believed, he surely would have advised Hollister on the existence of Earth-Venus-Mars-Earth trajectories with launch dates in 1972 that he claims to have discovered during his 1956-57 research project. But Hollister states in his dissertation that, after the 1970 launch opportunities, such multiplanet trajectories would not be practical again until after the turn of the century. But the 1972 trajectories are more practical than the 1970 trajectories because they have much shorter trip times. (The required launch energies are approximately equal for the 1970 and 1972 launch windows.) It is obvious that Battin didn't tell Hollister about the 1972 trajectories because he didn't know about them. Otherwise, he would have been withholding important technical information from his own student. If he didn't know about those 1972 trajectories, the story he told you is untrue.

It is possible to identify many other instances that lack reasonable explanation if one regards Battin's claim as being true. Consequently, I therefore submit that Battin was clearly not truthful during your interview. The evidence he presented -- that single slide -- does not, and clearly cannot, logically negate the collected weight of all of the evidnece cited above. This evidence indicates such a research project, with the claimed results, cannot reasonable be assumed to have existed. How many inconsistent instances are required before one can honestly conclude that his claimed research project is more likely to be not true than true?

If his claims are true, one must ask the question: What did he do about it? According to what you say, he didn't do anything about it. He didn't publish, nor did he tell anyone about it. Well, what can one conclude from this? Was he just plain stupid or just a liar?

Battin knows that he could never publish his claimed research project in a professional journal because it is simply not creditable in view of his 1959 paper. But he may feel that other people like yourself may publish it and thereby give it creditability.

You asked me to comment on Battin's possible motivation for telling you that lie, assuming that it is a lie. I can think of several reasons. The first may be a simple case of petty professional jealousy. Somebody who is not a professional in the field creates a uniquely novel concept, and if it later turns out to have a revolutionary impact, many of those professionals will try to claim credit or belittle it as "an obvious idea that we were just about to investigate." However, in Battin's case, the motivation is deeper because he is connected with the early history.

Before I go into this history it is important for you to understand certain fundamental characteristics regarding manned missions to Mars. By the end of the 1950s, it was an accepted fact (believed without question) that low-energy manned landing missions to Mars using near Hohmann transfer trajectories required very long trip times. Non-stop free-fall round-trip reconnaissance trajectories (Earth-Mars-Earth) with near Hohmann legs also required long trip times. These facts were known prior to Battin's 1959 paper from co-planar calculations based on the relative motion between Earth and Mars. Battin's 1959 paper was important because it set more accurate lower bounds on the required trip times for round-trip non-stop manned missions to Mars (1,100 days). This information was crucial because it was needed for basic mission planning (sizing the required launch vehicle, etc., costing billions of dollars). If you had a clear historical understanding of the importance of these facts you would see that Battin's claim is a joke.

In June 1962 when I extended my UCLA trajectory research by using the JPL computers, I had a long meeting with Victor Clarke. During the course of that meeting, I gave him more details regarding my low launch energy gravity-propelled Earth-Venus-Mars-Earth trajectories with 1970 launch dates. Since these round-trip reconnaissance trajectories were several hundred days shorter than Battin's Earth-Mars-Earth trajectories, required very little launch energy, and enabled the reconnaissance of two different planets instead of one -- a possibility totally new to Clarke and everyone else I met at JPL at that time -- Clarke was very excited about them. Clarke indicated that two people would be very interested in receiving this information. Stanley Ross at Lockheed and Richard Battin at MIT. It was clear that Clarke would, in fact, send this information to those people. (He also knew that these trajectories were actually solutions to the N - body problem because Gene Bollman performed detailed integration tests on them in April.)

A few weeks after that meeting, I was told by a secretary that Clarke did indeed send Battin this information. I learned many years later, however, that Clarke not only informed Ross of the existence of my Earth-Venus-Mars-Earth trajectories, but he also sent him a duplicate copy of my UCLA gravity-propelled trajectory program that I used to calculate them (ref. 8). Ross introduced my duplicated UCLA multiplanet gravity-propelled trajectory program into the Lockheed System, where it was used for many years to do "swingby" trajectory calculations (refs. 1 - 4). You once told me that this unauthorized transfer amounted to a "theft." I hope that you still have the same opinion. (The effects went way beyond any form of common plagiarism.)

Ross used the information and computer program to do trajectory research. But what did Battin do with his information? As I told you during the interviews conducted in the summer of 1990, Battin passed the information to a student named Hollister, and Hollister used this information as the basis for his 1963 dissertation at MIT. This represented high-level academic misconduct which, if made known to appropriate officials at MIT, could have resulted in Battin's dismissal from the university. But I was not aware of these events in 1962-63, and even if I were, there was little I could do about them.

In 1966, a paper was published by Ross wherein the credit for discovering the technique of gravity-propelled trajectory design was given to Hollister. (See pages 4-5, ref. 9). It is highly likely that that erroneous story published by Ross was not done in an off-handed manner, but rather was the result of systematic planning with Clarke and Battin, operating behind the scenes in complete collusion, that had its roots in that meeting with Clarke during that summer of 1962.

Battin may now be sensing some danger of exposure because, with the publication of our first IAF paper, people are beginning to make independent investigations and, in his eyes, the "established history" may begin to unravel. Thus, he may feel that by manufacturing a story <u>suggesting</u> that he led Hollister to those multiplanet Earth-Venus-Mars-Earth trajectories via his own prior research instead of receiving information on them from Clarke at JPL, the act of serious academic misconduct can be covered up (or at least made more difficult to expose). But he may find that his new story may lead to bigger problems for him in the future.

In order to call attention to the obvious questions that future historians and investigators may ask, I cite the following: As I pointed out above, Ross credited Hollister for originating the concept of gravity-propelled trajectory design. But Ross and his Lockheed cc-workers Gillespie and Ragsac, published papers (see pages 44-45, ref. 10 and ref. 11) on low launch energy multiplanet Earth-Venus-Mars-Earth gravity-propelled trajectories six months before Hollister did! In view of this fact, it is natural to ask the question: Why did Ross credit Hollister with a fundamentally new and important innovation in astrodynamics when his publications were the first and preceeded Hollister's by six months? It is because that little rascal was using my UCLA program to do his multiplanet "swingby" trajectory calculations and, since many people from Lockheed and some from JPL knew about this fact, he couldn't take the credit, (he would have been laughed at). Consequently, Hollister and his dissertation were an unwitting "set up" specifically designed to take the credit. Thus, the three main culprits in the early history of gravity-propelled trajectory design are Clarke, Ross, and Battin. It is unfortunate that those 40 hours of tape-recorded interviews were ruined becaue I went into this part of the early history with great detail.

I have taken the time to write you this letter to give you a broader perspective in which to evaluate Battin's story and to show you how inconsistent it is with the published record. It is my feeling that you should be prepared for all types of stories when conducting the interviews. Most of these stories

will be untrue because you will be interviewing people involved in perpetuating (or having a vested interest in perpetuating) the erroneous accounts as they have been propagated for the past 25 years. The situation may be like going to a hospital to cure a sickness when the doctors in that hospital were responsible for causing the sickness in the first place. (The problems all started with the unauthorized transfer of my UCLA gravity-propelled trajectory program to Lockheed in 1962.)

I hope that our future publications will remain on a very high plane. Dowling's policy of low-key standard operating procedure is the best way to go. Eventually, people will recognize the powerful and exciting story of research we have told, which is uniquely American. I want it to lift the aspirations of all young graduate students. My ideas about space travel were radically different from those early Russian and German pioneers. This country has a right to this heritage. But there are still a lot of wolves out there who are hell-bent to keep it all covered up or to make it unrecognizable.

I enjoyed seeing you during the Christmas holidays. I appreciate your concern about moving forward with our papers. We have a good start with the first paper and the others should be just as good.

Regards to Cheryl and the children.

Sincerely,

Dr. Michael A. Minovitch

MAM/dk

Enclosures (11):

1. Alibrando, A.P., "Manned Venus-Mars Fly-by in 1970 Studied", Aviation Week & Space Technology, March 4, 1963, p. 56.

2. Gillespie, R.W., Ragsac, R.V., and Ross, S., "Prospects For Early Manned Interplanetary Flights", Astronautics and Aerospace Engineering, Aug. 1963, pp. 16-32.

3. Deerwester, J.M., "Initial Mass Savings Associated With The Venus Swingby Mode Of Mars Round Trips", AIAA 2nd Aerospace Sciences Meeting, New York, Jan. 25-27, 1965, AIAA Paper No. 65-89.

4. Gillespie, R.W. and Ross, S., "Venus-Swingby Mission Mode and Its Role in the Manned Exploration of Mars", J. of Spacecraft and Rockets, Vol. 4, No. 2, Feb. 1967, pp. 170-175.

5. Battin, R.H., "The Determination of Round-Trip Planetary Reconnaissance Trajectories", J. of the Aero-Space Sciences, Vol. 26, No. 9, Sept. 1959, pp. 545-567.

6. Battin, R.H., "Astrodynamics: Highlights 1978", Astronautics & Aeronautics, Vol. 16, No. 12, Dec. 1978, p. 36.

7. Hollister, W.M., The Mission For A Manned Expedition To Mars, Dissertation for the Doctor of Science in Instrumentation, MIT, May 1963.

8. Letter from Minovitch to Roth, May 16, 1986.

9. Ross, S., "Trajectory Design For Planetary Mission Analysis", Recent Developments in Space Flight Mechanics, AAS Science and Technology Series, Vol. 9, ed. Richards, P.B., 1966, pp. 3-43.

10. Ross, R., "A Systematic Approach to the Study of Nonstop Interplanetary Round Trips", 9th Annual Meeting of the American Astronautical Society, Los Angeles, Calif., Jan. 15-17, 1963.

11. Gillespie, R.W., Ragsac, R.V., and Ross, S., "Prospects For Earth Manned Interplanetary Flights", 31st Annual Meeting of the Institute of the Aerospace Sciences, New York, Jan. 1963.