

# NOTARIZED AFFIDAVIT BY DECLARATION UNDER PENALTY OF PERJURY

To Whom It May Concern

I, William J. Kosmann, declare and state under penalty of perjury as follows:

In the fall of 1990 I interviewed Dr. Richard H. Battin at the Massachusetts Institute of Technology (MIT), in Cambridge, Massachusetts, regarding his early work on Gravity-Assisted trajectories. The interview lasted more than one hour. Dr. Battin stated that from 1959 to 1961 August, he discovered and computed patched conic Gravity-Assisted Earth-Venus-Mars-Earth trajectories with the following characteristics:

Launch date: 1972 June 9  
Venus Encounter date: 1972 November 14  
Mars Encounter date: 1973 April 7  
Earth Encounter date: 1973 September 13

Launch date: 1966 February 6  
Venus Encounter date: 1966 July 9  
Mars Encounter date: 1967 January 24  
Earth Encounter date: 1967 December 17

These trajectories were not integrated trajectories, but patched-conic trajectories. Dr. Battin was asked for proof of this Gravity Assist trajectory work in the 1959 to 1961 August time period. The only proof Dr. Battin offered during this interview was a copy of a set of 6 old lantern slides describing some of the characteristics of the above trajectories (Attachment #1). Dr. Battin stated that these slides were created sometime between 1960 August, and 1961 March. At no time during the interview did Dr. Battin mention or claim to possess any trajectory runs (computer printouts) related to either of the above Earth-Venus-Mars-Earth Gravity Assisted trajectories. In addition, Dr. Battin stated that the results of this work were eventually published in a paper in 1963. Dr. Battin provided the title of the paper, the title of the publication, and the name of the publisher of the paper. Dr. Battin completed the interview by stating that after the 1959-1961 Earth-Mars-Venus-Earth Gravity-Assisted trajectory work, he worked exclusively on the Apollo guidance problem for the rest of the 1960's, not returning to work on Gravity-Assisted trajectories at all.

Dr. Battin was firm about the time period of the above Gravity-Assisted trajectory work. There can be no misunderstanding or misinterpretation regarding the time period during which Dr. Battin said he did this work. Determining what early Gravity-Assisted trajectory work was done and when it was done was the entire reason this interview (as well as a related interview with Dr. Walter Hollister of MIT) was conducted. In fact, these interviews were the third and fourth of an ongoing series of interviews to attempt to determine who did what in the early history of Gravity-Assisted trajectory work.

A few weeks after the above interview with Dr. Battin, I traveled to California to spend the Christmas holidays with my family. (I was working in Reston, Virginia at the JPL Support Office of the Space Station Program Office, while my family was preparing to move to Virginia from Pasadena, California.) During this trip, I met with Dr. Minnovitch to brief him on what I had learned from my interviews with Dr. Battin and Dr. Hollister (just as I had with all my other interviews). A meeting was arranged with Dr. Minnovitch at his sister's apartment on the evening of 1991, January 5. Mr. Richard Dowling was also present at this meeting.

In order to preserve the details of the interview for the historical record, Dr. Minnovitch, summarized in his words, what he believed Dr. Battin told me in the 1990 interview. This summary took the form of a letter from Dr. Minnovitch to me, dated 1991 January 11 (Attachment 2). The letter was 8 pages long and included 14 enclosure documents, indicating that he believed that what Dr. Battin told me in the 1990 interview was not credible. Dr. Minnovich mailed the letter to me, and I received his letter, but I did not reply to it. At the time, my personal belongings (including my notes from the interview) were partially on the East coast and partially on the West coast. (I was working in Virginia, and my family and almost all of my professional library were in Pasadena. The library, including the results of this series of interviews, was in the process of being boxed up for the upcoming move to Virginia).

I attest and affirm under penalty of perjury under the laws of the State of Virginia that the preceding statements are true and correct.

William J. Kosmann  
Signature

1999 September 24  
Date

William J. Kosmann  
37523 Maple Shade Lane  
Middleburg, Virginia 20117

Attachment (1): Xerox of set of 6 lantern slides describing some characteristics of the 1966 and 1972 Earth-Venus-Mars-Earth Gravity-Assist trajectories, provided by Dr. Battin as proof of his having worked on these trajectories from 1959 to 1961.

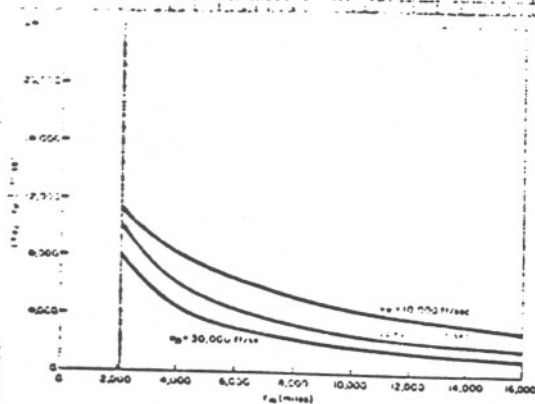
Attachment (2): Letter from Dr. Minnovitch to William Kosmann dated January 11, 1991.

State of VA  
County of Loudoun

Sworn to and subscribed before me  
this 24th day of September  
19 99 by William J. Kosmann  
Paul R. Penwell

NOTARY PUBLIC

My commission expires 12-31-00



VELOCITY CHANGE vs MINIMUM PASSING DISTANCE AT MARS

MIT Instrumentation Laboratory  
May, 1960

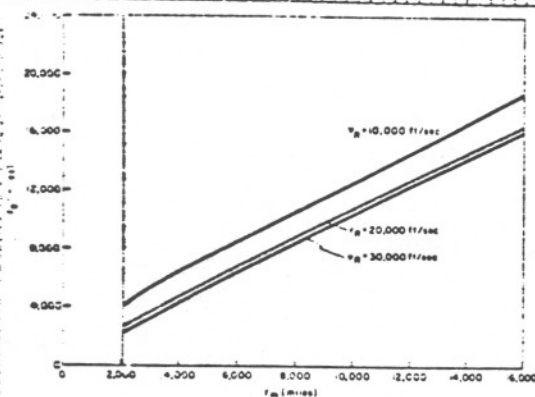
# MARS TRAJECTORY I

TIME OF FIX	ANGLE BETWEEN	RMS POS. ERROR IN MILES
0.002	MOON AND CAPELLA	26
	MOON AND ARCTURUS	
	EARTH AND CAPELLA	
	SUN AND MOON	21
	EARTH AND VEGA	15
	SUN AND EARTH	14

M.I.T. INSTRUMENTATION LAB.

AUGUST 1960

18071-1



POINT OF AIM vs MINIMUM PASSING DISTANCE AT MARS

MIT Instrumentation Laboratory  
May, 1960

# MARS TRAJECTORY I

TIME OF FIX	ANGLE BETWEEN	RMS POS. ERROR IN MILES
0.375	MARS AND RIGEL	4178
	MARS AND BETA CENTAURI	
	EARTH AND CAPELLA	
	SUN AND MARS	3515
	EARTH AND ACTURUS	2723
	SUN AND EARTH	2486

M.I.T. INSTRUMENTATION LAB.

AUGUST 1960

18067-1

## ASSUMED RMS MEASUREMENT ERRORS

OPTICAL MEASUREMENTS - 0.05 MILLIRADIANS

CLOCK DRIFT RATE -  $10^{-5}$

INJECTION VELOCITY - 40 FEET PER SECOND

APPLIED VELOCITY CORRECTION - 1%

M.I.T. INSTRUMENTATION LAB.

AUGUST 1960

# MARS TRAJECTORY I

TIME OF FIX	ANGLE BETWEEN	RMS POS. ERROR IN MILES
0.775	VENUS AND PROCYON	7252
	VENUS AND ACHERNAR	
	MARS AND CAPELLA	
	SUN AND VENUS	5844
	MARS AND VEGA	4044
	SUN AND MARS	3612

M.I.T. INSTRUMENTATION LAB.

AUGUST 1960

18069-1

January 11, 1991

William Kosmann  
752 N. Holliston St.  
Pasadena, CA 91104

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 ephemeris, and then debugging it would take months.) It is highly unlikely that a professional

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 evidence cited above. This evidence indicates such a research project, with the claimed results, cannot reasonably 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 suggesting 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 co-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 preceded 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 because 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

William Kosmann  
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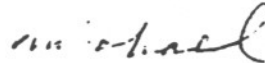
will be untrue because you will be interviewing people involved in perpetuating (or having a vested interest in perpetuating) the erroneous accounts as they have been propagated for the past 25 years. The situation may be like going to a hospital to cure a sickness when the doctors in that hospital were 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.