An Introduction to the Mathematics and Methods of Astrodynamics, Revised Edition

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October 4, 1957 and the Aftermath

Like so many other Americans, the first half of October 1957 found me standing in my yard in the cold but clear early morning hours watching and waiting for the Russian Sputnik to pass overhead. I had been away from MIT for just one year exploring new and different career opportunities in the alleged greener pastures of industry. A few months later during one of my infrequent telephone conversations with Hal Laning, I learned that he had a simulation of the solar system running on the IBM 650 and was "flying" round trips to Mars.

It didn't take long to wind up my affairs and head back to the Instrumentation Lab. My return practically coincided with the publication of a laboratory report[11] on the technical feasibility of an unmanned photographic reconnaissance flight to the planet Mars. It was asserted by the authors that a research and development program to that end could reasonably be expected to lead to the launching of such a vehicle within the next five to seven years. (It is interesting that the study and report had been sponsored by the Ballistic Missile Division of the U.S. Air Force.)

A small group was forming to flesh out the system proposal for the Mars mission. Hal and I were responsible for the trajectory determination, as well as the mathematical development of a suitable navigation and guidance technique. The project culminated a year or so later in a three volume report,[12] and a full-scale model of the spacecraft.

To my surprise, it quickly became evident that we did not really know how to compute trajectories for the simple two-body, two-point boundary-value problem! How could that be possible after all the work on ballistic missile trajectories only a few years earlier? As I reviewed those equations in the Q-system report, the difficulty (but not the solution) was apparent. We had, indeed, developed expressions involving the correlated velocity vector but they were all implicit—$\mathbf{v}_c$ never appeared explicitly. These equations were fine for calculating the Q matrix by implicit differentiation but in no way did it seem possible to isolate the velocity vector. (Hal had been calculating round-trip Martian trajectories by "trial and error"—adjusting and readjusting the spacecraft initial conditions and determining the orbit by numerically solving the equations of motion. There had to be a better way!)

I found the clue in the classical treatise on dynamics by Whittaker[13]:

> *"Lambert in 1761 shewed (sic) that in elliptic motion under the Newtonian law, the time occupied in describing any arc depends only on the major axis, the sum of the distances from the center of force to the initial and final points, and the length of the chord joining these points: so that if these three elements are given, the time is determinate, whatever the form of the ellipse."*

The proof followed, and the section ended with a neat analytical expression for time of flight as an explicit function of the problem geometry and the semimajor axis a of the orbit. Given the geometry and the time of flight, then a could be determined—not directly but by iteration.

It was the footnote that gave me pause:

> "*It will be noticed that owing to the presence of the radicals, Lambert's theorem is not free from ambiguity of sign. The reader will be able to determine without difficulty the interpretation of sign corresponding to any given position of the initial and final points.*"

By no means was it obvious to me how to resolve the ambiguity or, more to the point, how to instruct a computer to choose unerringly from among the several alternatives. Whittaker's only reference was to Lagrange (Oeuvres de Lagrange, IV, p. 559) who also failed to address my concerns; but going to the original source did pay dividends. Instead of proceeding immediately to his proof of Lambert's theorem, Lagrange first chatted about the problem from different perspectives† —one of which led me to transform the problem to rectilinear motion. The ambiguity then ceased to exist.

A nontrivial problem remained—to obtain the initial velocity vector in terms of the semimajor axis a. An intense effort produced finally a delightfully elegant expression. We were now able to generate interplanetary trajectories with great aplomb. (My first trajectory program suffered from an annoying deficiency. Time of flight is a double-valued function of the semimajor axis a with infinite slope for the minimum-energy trajectory—far from ideal for a Newton-Raphson iteration. The difficulty was resolved by a different choice of independent variable against which the time of flight is a monotonic function. This small, but necessary, wrinkle was first reported in an appendix to Ref. 9, and practically eliminated the audible vulgarisms that so frequently accompanied the use of the original program.)

With some trepidation, I presented this method[14] of trajectory determination in New York on January 28, 1959 at the annual meeting of the Institute of the Aeronautical Sciences. My scant background in celestial mechanics did little to inspire self-confidence in the novelty of the technique. But, as I later learned, Rollin Gillespie and Stan Ross were in the audience, and had carried a preprint back home to their associate John Breakwell at the Lockheed Missiles and Space Division. They, too, had been grappling with the trajectory problem and (according to Rollin) this was the "breakthrough" they also needed.

† Lagrange's paper would never appear in the *Journal of Guidance, Control, and Dynamics*, or in any other modern archival publication, without strong protestations from the editor—"Needs at least a 50% reduction!"

The method became the basis of the major orbit-determination programs of the Jet Propulsion Laboratory for its series of unmanned interplanetary probes, and of the Navy and Air Force for targeting ballistic missiles. Indeed, in the early sixties, JPL used this technique to generate an enormous set of volumes—similar to the Airline Guide—in which were tabulated daily launch conditions for Venus and Mars missions extending many years into the future.

To support the Mars reconnaissance study project, we confined our attention to trajectories whose flight times were of the order of three years, and which had launch dates in the years 1962–1963. These missions, for which the space vehicle makes two circuits about the sun while the earth makes three, seemed to provide the greatest flexibility in launch window and passing distance at Mars without placing unreasonable requirements on launch system capabilities. Later we investigated round-trip missions to Venus, which could be accomplished with flight times of only a year and a quarter.

One day, when plotting a few of these Venusian reconnaissance trajectories, I was impressed by the proximity of the spacecraft orbit and the Martian orbit resulting from the increased velocity induced during the Venusian flyby. The interesting possibility of a dual contact with both planets seemed feasible—a kind of celestial game of billiards. The infrequency of proper planetary configurations would, of course, severely limit the practicality of such a mission if, indeed, one existed at all.

Using trusty "cut and try" methods, I found that ideal circumstances did prevail on June 9, 1972. On that date, a vehicle in a parking orbit launched from Cape Canaveral on a 110° launch azimuth course could be injected into just such a trajectory at the geographical location of 5° W and 18° S and with an injection velocity relative to the earth of 15,000 ft/sec. The first planet encountered would be Venus after 0.4308 year. The vehicle would pass 4426 miles above the surface of the planet and would, thereby, receive from the Venusian gravity field alone a velocity impulse sending it in the direction of Mars. The second leg of the journey would consume 0.3949 year and the spacecraft would then contact Mars, passing 1538 miles above the surface. The trip from Mars back to earth would last 0.4348 year so that the vehicle would return on September 13, 1973. This truly remarkable orbit is illustrated in Fig. 6. (At the time, the launch date seemed incredibly far off—twelve whole years! But the day finally came and, sad though it may seem, passed without fanfare or even a comment.)

Although this was the first realistic multiple flyby mission ever designed, it was not the first ever conceived. That distinction goes to General Gaetano Arturo Crocco who was Director of Research of the Air Ministry and a Professor of Aeronautics at the University of Rome, Italy. His paper[15] described an earth to Mars to Venus to earth mission of one year duration. The orbits were all coplanar; the velocity requirements were

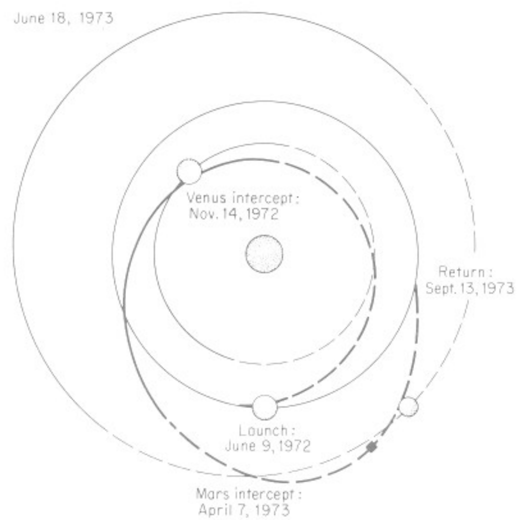

Fig. 6: Double-reconnaissance trajectory (from Ref. 10).

enormous; and the reversed itinerary prevented the best utilization of the gravity assist maneuvers. But it was published in 1956—one year before Sputnik. (AIAA members might appreciate knowing that General Crocco was a founding member of the Institute of the Aeronautical Sciences—one of our parent organizations.)

The Mars reconnaissance preliminary design was ready for customer review in the summer of 1959. The Air Force had been our sponsor, and it was there that we expected to turn for authorization to proceed. We were ready to go—"Mars or bust!"—with an enthusiasm that was exceeded only by our naiveté. While we had been busy nailing down the myriad of technical problems one by one, the political climate was changing. A new government agency called the "National Aeronautics and Space Administration," not the Air Force, would control the destiny of the Mars probe.

With view-graphs, reports, and a wooden spacecraft model, we headed for Washington instead of Los Angeles, and arrived there on the same day as Chairman Khrushchev. Although our presentation was well received, the high-level NASA audience we had expected (including Hugh Dryden, the Deputy Administrator) was attending to the necessary protocol mandated by the Russian visit. We were sent home with a pat on the head and the promise of some future study money. As our dreams of instant glory in interplanetary space began to fade, we secretly took perverted pleasure in having Nikita Khrushchev himself as a ready-made scapegoat. The Russians were formidable opponents indeed!

The advantages of the new formulation became evident. An easy calculation showed that the contribution of the term $\Delta \mathbf{g}$ is generally much smaller than that of $\mathbf{Q}\mathbf{v}_g$. Furthermore, $\Delta \mathbf{g}$ approaches zero at a rate proportional to v_g^2, while the $\mathbf{Q}\mathbf{v}_g$ term, on the other hand, vanishes like v_g to the first power. Simulations verified that $\Delta \mathbf{g}$ is so small for short maneuvers that a nearly constant attitude can be obtained by merely steering the vehicle so as to align the thrust vector along v_g. Velocity-to-be-gained, under these circumstances, is particularly easy to compute—the accelerometer-sensed velocity change is subtracted from the previous value of $\mathbf{v}_g$ on each computer guidance cycle.

Tim's technique[33] works well, even for long duration maneuvers, if we periodically create a new coasting-flight trajectory. A suitable approximation for $\Delta \mathbf{r} = \mathbf{r}' - \mathbf{r}$ is found to be

$$\Delta \mathbf{r} = -\frac{v_g}{2a_T}\mathbf{v}_g \tag{27}$$

which, when added to current vehicle position, produces the position vector $\mathbf{r}'$. Knowing $\mathbf{r}'$ and the target $\mathbf{r}_T$, together with the time of flight, permits a new Lambert solution—hence a new $\mathbf{v}_r$ and a new coasting trajectory. Subtracting the current vehicle velocity provides an updated value of $\mathbf{v}_g$ with which to begin anew.

If none of these ideas seem familiar, you have forgotten the Convair legacy. What has just been described is essentially what the Convair engineers were advocating those many years ago. I must confess that I did not make the connection between Tim's new technique and the old Convair proposal until I began rummaging through my memorabilia in preparation for this paper. Obviously, Tim knew nothing of this—he was only about ten years old at the time.

Of course, the Tim Brand or the Convair scheme would have been impractical for an onboard implementation to guide the early ballistic missiles. It was feasible only after the small airborne digital computer replaced all those servos, amplifiers, potentiometers, and other analog devices of the good old days.

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