## **Brouwer vs Vinti**

"If it disagrees with experiment it is wrong. In that simple statement is the key to science. It does not make any difference how beautiful your guess is. It does not make any difference how smart you are, [or] who made the guess... if it disagrees with experiment it is wrong. That is all there is to it."

Richard P. Feynman

## **Historical Notes**

John Vinti was one of the few surviving figures from the American Golden Age of Science that began during the 1930's. He received a S.B. degree in mathematics, and a D. Sc. degree in physics both from MIT. Physicist Philip Morse, who was his advisor, suggested the approach of finding a "solvable problem" in Hamilton-Jacobi Theory. Then, as now, the Hamilton-Jacobi equation was regarded by most physicists as only a point of departure for quantum mechanics. Years later, he became the first to apply the Hamilton-Jacobi Theory effectively to the general perturbations of orbital and celestial mechanics – the analytic methods of orbit prediction.

In the early 1940's, while working on interior ballistics of rockets, Vinti met Boris Garfinkel, an astronomer, and Joel Brenner, a mathematician at the Aberdeen Proving Ground in Maryland. Garfinkel helped direct his efforts in celestial mechanics, while Brenner reinvigorated his focus in finding a solvable solution of the Hamilton-Jacobi equation in orbital and celestial mechanics. It was also during his stay at Aberdeen that he developed a close association with giants such as John

von Neumann, Martin Schwarzchild, Subramanyan Chandrasekhar, and Josef and Maria Goeppert-Maier.

In 1959, Vinti produced his first series of papers on the motion of a close Earth, drag-free satellite by means of a separable Hamiltonian. By introducing a gravitational potential in oblate spheroidal coordinates, Vinti was able to simultaneously separate the Hamilton-Jacobi equation and satisfy Laplace's equation. Since the assumed potential is very close to that of the Earth, the resulting analytic solution to the equations of motion yields very accurate and rapid results. Until that time, standard general perturbation methods were both computationally intensive and relatively low in accuracy for use in orbit prediction. In a single brilliant effort this changed overnight. Scientists and engineers especially in the British, Soviet, French, Japanese and Chinese Space communities were quick to recognize this work and adapt it to their needs in both research and applications. For various reasons, the United States Space communities have forgotten the powerful Vinti method of general perturbations.

All the equations of Vinti's method were published for over 40 years, but despite several attempts by government agencies and defense contractors, all the Vinti computer programs were not accessible to the world community. Most of these Vinti computer programs are based on the classical orbital element formulation and not proven to be free of singularities. An analytic Vinti computer program that has no singularities and developed with the use of universal variables, was finally presented in the 1998 Vinti's book "Orbital and Celestial Mechanics" by American Institute of Aeronautics and Astronautics. Here, a further improved version is presented in the vinti computer programs of *DerAstrodynamics*.

The United States Space surveillance and tracking systems, in general, use the Simplified General Perturbation (SGP) and its derivatives and extensions, which are essentially derived from the Brouwer theory. The problems of tracking satellites at the zero and critical inclinations still present difficulties for any Brouwer type

method. It is impossible to fix the SGP programs that are not designed to compute these types of satellite orbits because of singularities associated with the Brouwer theory. In a letter dated December 23, 1959, Professor Brouwer wrote to Vinti to express his displeasure and criticism that Vinti asserted to have solved the critical inclination singularity problem in the National Bureau of Standards Report, STR-2434. This report and the letters of their dialogue, which are obtained from the family of Vinti, are presented in the following. One interesting quote extracted from a letter of Dr. D.G. King-Hele (January 1, 1960) is: "The eminence of Brouwer's position doesn't prove that he is right!"

# **TECHNICAL NEWS**

From

U. S. DEPARTMENT OF COMMERCE

NATIONAL BUREAU OF STANDARDS

WASHINGTON 25, D. C.

SUMMARY TECHNICAL REPORT STR-2434

December 1959

FOR FURTHER INFORMATION CONTACT
NBS OFFICE OF TECHNICAL INFORMATION
EM 2-4040 - Ext 7142

#### RESEARCH ON SATELLITE ORBITS

Summary: The National Bureau of Standards has devised a simple, direct approach for determining the effects of the earth's oblateness on the orbit described by an artificial satellite. This method is shorter and less complicated than the perturbation method usually applied to the problem. It offers the additional advantage of eliminating the need for special considerations at the critical orbital inclination. The solution provided by the Bureau's procedure can also be adopted as a starting point for using perturbation theory to find the effects of factors other than oblateness.

The National Bureau of Standards, in work done for the Air Force, has devised a simple, direct approach for including the effects of the earth's oblateness, or flattening at the poles, in the calculation of satellite orbits. \frac{1}{2} Based on an analysis by J. P. Vinti of the Bureau's mathematical physics group, this approach applies in cases where drag is negligible. Although results are still being worked out, it promises to eliminate many of the difficulties encountered in applying perturbation theory to the problem of calculating the effects of oblateness. Furthermore, it can be used even when the orbital inclination approaches the critical value of 63.4 degrees, where special considerations might otherwise be necessary.

### YALE UNIVERSITY OBSERVATORY

BOX 2023, YALE STATION NEW HAVEN, CONNECTICUT

135 PROSPECT STREET

December 23, 1959

Dr. J. P. Vinti National Bureau of Standards Washington 25, D. C.

Dear Dr. Vinti:

A short time ago I received a copy of the Summary Technical Report STR-2434 on Research on Satellites. I am convinced that there was no intent to mislead the reader, but, after having read the communication several times, I have the uneasy feeling that only a reader who is fully informed on the subject will come to the correct conclusion. Those who are less familiar with the problem are likely to conclude that you have accomplished the impossible: a solution of the artificial satellite problem in which the difficulty of the critical inclination has vanished.

In your particular potential,

$$V = \frac{A}{r} \left[ 1 + \frac{B_2 P_2 (\sin \beta)}{r^2} + \frac{B_4 P_4 (\sin \beta)}{r^4} \right]$$

the ratio  $B_4/B_2^2 = 1$ . The Summary Technical Report on page 3 states that this ratio agrees, within the limits of observational error, with the most recent and most reliable value—that found by King-Hele and Merson.

O'Keefe, Eckels and Squires (AJ 64, 245, 1959) find for  $\rm B_4/\rm B_2^{\ 2}=1.46$ . This result appears to be more trustworthy than that by King-Hele and Merson. Hence it seems pretty well established that the coefficient of the fourth harmonic is larger by some 40 to 50 per cent than in your model.

These remarks do not intend to minimize the value of your contribution. I merely am unhappy about the manner in which it was presented in the Report.

With all good wishes for the Christmas Season and the New Year.

Sincerely yours

Dirk Brouwer

DB/bid

#### 28 December 1959

11.4

Dr. D. G. King-Hele Royal Aircraft Establishment Farnsborough, Hants England

## Dear Dr. King-Hele:

As you will perceive from the enclosed letter, I have got myself in trouble with Professor Brouwer. As you know, he is the editor of the Astronomical Journal and one of the leading dynamical astronomers in the world, so I do not wish to remain in this trouble.

I had known of the work of Jeffreys and of O'Keefe et al when I wrote the material for STR-2434, but disregarded it when I wrote "the most reliable value". This was on three counts:

- (1) Jeffrey's value for  ${\bf J}_4$  depends on the assumption that cross-sections of the earth are ellipses
- (2) O'Keefe had not yet committed himself in a published paper on the value of  ${\bf J}_4$
- (3) According to Cornford, King-Hele, and Merson, "Recent Studies of Satellite Orbits", IAS Paper No. 59-141, 7th Anglo-American Aeronautical Conference, your value for  $J_4/J_2^{\ 2}$  was still coming down, closer to the value corresponding to my potential. The latest decrease had its source in corrections for the third harmonic, the rotation of the atmosphere, and lunar-solar perturbations.

These remarks will constitute the major part of my reply to Professor Brouwer, but I should appreciate the favor if you can give me any further information about the reliability of your value for  $J_4/J_2^2$ , as compared with that of O'Keefe et al. As a partial return for such a favor, I promise to keep you out of any controversy, by

not quoting you directly.

Sincerely yours,

John P. Vinti

P.S. Brouwer's remark about the critical inclination has to do with the disappearance of singularities in the perturbation theory when  $J_4 + J_2^2 = 0$ .

The angle 63.4° remains a critical inclination, however, in the sense that when I expand my integrals in powers of  $k = (I - I_t)/MR^2$ , that

I find the mean motion of the line of apsides relative to the line of nodes vanishes, to the first order in k.

JPV/mc Encl.

## BY AIR MAIL



Guided Weapons Department, Royal Aircraft Establishment, South Farnborough, Hants, England.

1st January, 1960.

Dear Dr. Vinti,

Thank you for your letter of 28 December. I'm sorry to hear that you have received such an undeserved rebuke from Professor Brouwer. The eminence of his position doesn't prove that he is right!

Jeffreys would certainly agree that his value of  $J_{i}$  in "The Earth" is purely a conventional one: he has told me that he accepts our values of  $J_{i}$  and  $J_{i}$ .

I am afraid I have not made any further refinements of our calculations since my paper for the International Astronautical Congress, which I wrote in July. (This was almost identical to the paper which Cornford gave at New York in October). We still stand by those values and see no reason to revise them.

I think that (if we haven't made any mistakes!) our results ought to be more accurate than those of O'Keefe et al because we have used the nodal motion of two satellites, whereas they have used the motion of node and perigee of one satellite. Now it is very difficult to define the perigee position of a satellite accurately, in the presence of perturbations, and almost impossible to determine it observationally. For example, with Vanguard 1, an angular travel of 1° at perigee changes the height by only 200 ft., a distance which is quite undetectable with the relatively crude Minitrack observations from which the orbit of Vanguard is determined.

Dr. J. P. Vinti, National Bureau of Standards, Washington 25, D.C., U.S.A. /The



The Minitrack observations have, I believe, an accuracy of about 3 minutes of arc, which corresponds to about 3000 ft. in height. This accuracy is however quite good enough for determining the node, since the ground track can be estimated correct to about 3000 ft. too, or 0.01° in longitude.

Best wishes for the New Year,

Yours sincerely,

D. G. King-Hele

(D. G. KING-HELE)

January 6, 1960

Professor Birk Brouwer Director, Yale University Observatory Yale University New Haven, Connecticut

Dear Professor Brouwer:

Your letter of December 23 has given me much concern, so that I feel I must reply to you in some detail.

The material in STR-2434 was prepared by me and rewritten by our Office of Technical Information, newspaper style, for a forth-coming Technical News Bulletin of N. B. S., describing the work of the Applied Mathematics Division. In deference to your opinion, I have gone over the material with Dr. Cannon and we have decided on certain changes, to be made in this forthcoming bulletin. I am saclosing a copy of STR-2434, in which these changes are indicated in pencil on page 3. (The Summary Technical Report was a sort of preprint.)

When I prepared this material (about October 12), I was aware of the published work of Jeffreys in "The Earth", Ed. 3, with the value  $R \equiv |J_4|J_2^{-2} = 2.05$ , of Lecar, Sorenson, and Eckels, J. Geophys. Res. 64, 209 (1939), with R=2.02, of Ring-Hele and Herson, with R=1.17, and of Cornford, King-Hele, and Herson, with R=1.11. The published values of Jeffreys and of Lecar et al depend strongly on conventional assumptions about the figure of the earth, so I dismissed them from consideration.

The reliable <u>published</u> values of R at that time were thus only those of King-Hele et al. It is true that I had some inkling of work by O'Keefe, which he had told me about over the telephone.

He had, however, not yet published this work, so could not be held responsible for the value of R. He quoted values for J and D, from memory as I recall, which indicated a value for R intermediate between those of Jeffreys and of King-Hele. I handed in my write-up long before November 19, the date when the Bureau received the September number of the Astronomical Journal, containing O'Escée's paper.

Although I have mentioned these dates, I do not wish to rest my case on any such technicality as O'Keefe's lack of formal publication at the time. Instead I plead truth, with the qualification that this is an opinion. A bit of the history of my own thinking is pertinent here. The statements in my MBS paper are somewhat more conservative than those in STR-2434. My thinking changed after I weat to a New York meeting on October 5, when I heard Cornford present the work of Cornford, King-Hele, and Merson. In this talk it turned out that when they made further corrections, viz. for the third harmonic, for atmospheric rotation, and for lunar-solar perturbations, their value for & came down from 1.17 to 1.11, the change being opposite in direction from O'Keefe's value 1.46. Moreover their work is based solely on nodel motions (of two satellites), rather than on model and perigee motious of one satellite, as is O'Keefe's. Data concerning action of the periges appear to be less reliable than data concerning nodal motion. Thus my opinion remains unchanged about the relative degrees of reliability of the two determinations.

Until much more work is done, however, this disagreement will remain a matter of opinion. Since your opinion is certainly worthy of great respect, I have, as I have already indicated, deferred to it and modified the claims that will appear in the forthcoming Technical News Bulletin.

A Happy New Year to you, Sincerely,

John P. Vinti Mathematical Physics Section

## YALE UNIVERSITY OBSERVATORY

BOX 2023, YALE STATION NEW HAVEN, CONNECTICUT

135 PROSPECT STREET

January 11, 1960

Dr. John P. Vinti Mathematical Physics Section National Bureau of Standards Washington 25, D. C.

Dear Dr. Vinti:

I thank you for your letter of January 6, and am unhappy that I gave you so much concern with my letter of December 23, 1959. It still seems to be that we are looking at two different aspects of the same problem.

What was lacking in S.T.R.-2434 was a clear statement that your solution is a special case,  $J_2/J_2=1$ , of the general artificial satellite problem. It is in this particular case alone that the critical inclination features of the motion vanish. But for this particular ratio they also vanish in other theories, for example in Garfinkel's and my own.

Thus the statement that your theory "promises to eliminate many of the difficulties encountered in applying perturbation theory to the problem of calculating the effects of oblateness" is unfortunate because it omits that precisely these difficulties are not present in the particular case  $J_4/J_2^2=1$  in any theory. This was my principal complaint about the Report.

You concentrate your attention on the particular ratio that applies to the earth. On the basis of what you write, I am willing to accept that is entirely conceivable that the ratio is closer to unity than O'Keefe's result, and that therefore the solution with  $\mathbf{J_4}/\mathbf{J_2}^2=1$  is a reasonably close approximation for the earth's gravitational field. The results obtained so far are rather provisional, and do not want to argue in favor of a particular value.

Let me conclude by saying that at no time did I believe that you were deliberately misrepresenting the situation, but that I merely wanted to express my disappointment that the Report, in my opinion, failed to take advantage of the opportunity to present a clear and unambiguous statement on the relation between your solution and other solutions.

Sincerely yours,

Dirk Brouwer

DB/bjd

January 14, 1960

Professor Dirk Brouwer Director, Yale University Observatory Yale University New Haven, Connecticut

#### Dear Professor Brouwer:

This is in reply to your letter of January 11. I think you will agree that it would be unfair to ask me to answer your objections at this time in any detail, since the perturbation theories to which you refer have not yet been published. Instead let me present my own point of view. As before, let  $\left|J_A\right|J_2^{-2}=R$  and also let i be the angle of inclination.

I believe you will accept the physical postulate that no singularity can physically exist for any value of i; that is, physically all quantities must vary smoothly right through the inclination  $i=63.4^{\circ}$ , even if  $R \neq 1$ . In the unlikely event that you do not agree, it would be difficult to continue the discussion, but I should then have to remark that asserting the existence of a singularity is a serious matter for which one should be able to give a physical reason. I have seen no physical reason given by anyone. In case you are now impatient with me, just realize that I am here deliberately emphasizing the physics.

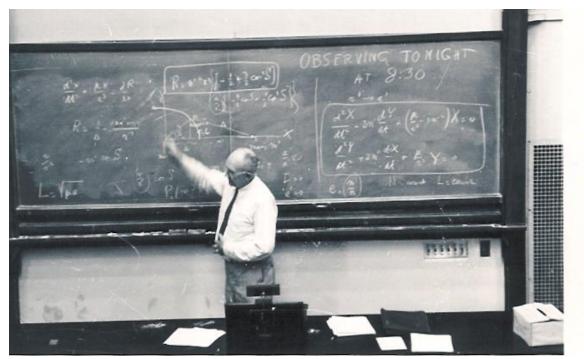
Another way of stating the physical requirement is that if, for given initial conditions, we imagine changes in R, the observable quantities must vary smoothly through R=1, regardless of the value of i. Thus if one has a mathematical solution correct for R=1, as is mine, it must be a rather good approximation if R actually equals  $1+\epsilon$ , where  $\epsilon$  is sufficiently small.

This is the way in which I have thought about the validity of my method as an adequate description of the effects of the even harmonics. It also explains why I felt it sufficient, in answering your letter of December 23, to confine my remarks to the possible values of  $\epsilon$ .

You may come back, however, with the objection that the validity of such considerations may depend strongly on the value of  $\epsilon$ , even if  $\epsilon$  is small. Such an objection would be founded on the existence of the singularity in the perturbation theories, which you may consider indicative of a physical sensitivity even if physically there is no singularity. That is, you may argue that quantities must vary rapidly with changes in  $\epsilon$  at  $\epsilon = 63.4^{\circ}$  and  $\epsilon = 1$  even if they don't become infinite. But can you give me any clear physical reason why they should? If you can, I'll try to stop the presses on the Technical News Bulletin. In any event, if these letters of ours lead to a better physical understanding of the problem, they will have been worth while.

Sincerely yours,

John P. Vinti Mathematical Physics Section



Professor Brouwer lecturing on the motion of the Moon



Professor Vinti lecturing on Potential Theory