Corrections and Clarifications to Vallado Second Edition (2001)

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These are the errors, clarifications, etc. that I have discovered or been told of in the second edition of Vallado. I will update this as the semester progresses, so check back for updates periodically.

p. 23 (1–14) is typeset oddly, with "$\ddot{\boldsymbol{r}} =$" too high.

p. 23 "For two-body motion, these assumptions are adequate…" These assumptions *define* two-body motion.

p. 40 Sundman's name is misspelled.

p. 41 It should be pointed out that, although its called the mass ratio, μ^* is *not* actually the mass ratio; $\mu^*/(1-\mu^*)$ is.

pp. 44–46 In Figures 2–9 through 2–11, the labels for L_1 and L_2 are switched (L_1 is between the two massive bodies).

p. 94 The unnumbered Taylor series expansion of $\boldsymbol{r}(\tau)$ is garbled. Either the polynomial times should be $(\tau - t)^n$ instead of τ^n, and then the expansion is about an arbitrary t,

$$\boldsymbol{r}(\tau) = \boldsymbol{r}(t) + \dot{\boldsymbol{r}}(t)(\tau - t) + \frac{\ddot{\boldsymbol{r}}(t)(\tau - t)^2}{2!} + \dots$$

or expansion is about $t = 0$ as the text preceeding states, and the occurences of t as arguments to the derivatives should be replaced with 0,

$$\boldsymbol{r}(\tau) = \boldsymbol{r}(0) + \dot{\boldsymbol{r}}(t)(\tau) + \frac{\ddot{\boldsymbol{r}}(0)\tau^2}{2!} + \dots$$

p. 107 Second paragraph: the term is the *right ascension of the ascending node*. It is a mouthful, but that is the proper term. "Longitude" is not correct as that refers to the earth sphere, not the celestial sphere.

pp. 107–111 Below each of the equations (2–82), (2–83), (2–84), (2–85), (2–87), (2–90), the conditions for the angle being in the lower halfplane are incorrectly stated. Replace each statement of the form $x = 360° - x$, which implies that $x = 180°$, with $180° < x < 360°$, which states that the arccosine is to be taken in the lower halfplane.

p. 111 In the description of the true longitude, delete "that's sometimes used."

p. 111 The true longitude may be approximated as $\lambda_{\text{true}} \approx \Omega + \omega + \nu$, when the inclination is small (with the equation holding exactly when the inclination is zero). The last sentence of the true longitude paragraph garbles this concept.

p. 111 The definition of mean longitude as "the location of the satellite from periapsis" is difficult to interpret and misleading, and the statement "analogous to the true anomaly" is mystifying.

pp. 114–115 Customarily, ballistic coefficient is $C_D A/m$, not the reciprocal, as he defines BC, in a circuitous way. The statement "Here, ρ_0 is the atmospheric density at perigee of the orbit (assumed to be 2.461×10^{-5} kg/m^2/ER)..." is a mystifying statement. What is *the* orbit? B^* should have units of reciprocal length, but these are not given on p. 115.

p. 118 Equation (2–99) is not correct:

$$
\begin{aligned}
g_p &= \sqrt{2\sqrt{\mu a}(1 - \sqrt{1 - e^2})} \cos(\omega + \Omega) \\
h_p &= \sqrt{-2\sqrt{\mu a(1 - e^2)}(\cos i - 1)} \cos(\Omega) \\
G_p &= \sqrt{2\sqrt{\mu a}(1 - \sqrt{1 - e^2})} \sin(\omega + \Omega) \\
H_p &= \sqrt{-2\sqrt{\mu a(1 - e^2)}(\cos i - 1)} \sin(\Omega)
\end{aligned}
$$

the other expressions are correct.

pp. 125–126 Values given at the start of the example are slightly different when placed in the equations ($e = 0.83285$ vs. $e = 0.83284$, $\nu = 92.335$ vs. $\nu = 92.336$). The numerical results presented are not exactly correct for either set of values.

p. 133 $m = r_0 r(1 + \cos \Delta\nu)$, i.e., remove the plus sign.

p. 134 In problem 1, it isn't a "hint" that $a = 8000$km, it's a piece of information necessary to find the universal variable.

p. 146 It is not made clear (until the end) that the equations below (3–10) apply for $h_{\text{ellp}} = 0$, and the comment below (3–11) is not correct; the equations may not be valid even if "on" the earth's surface, and they may be valid when not "on" the earth's surface, because what counts is being on (or off) the earth ellipsoid which is different from the surface. See Figure 3–5.

p. 162 "The S axis points in the direction of the velocity vector and is *perpendicular* to the radius vector..." This is a confusing statement; except for circular orbits, it cannot do both, if one interprets "direction of" as meaning "parallel to," as many might.

p. 163 The definitions of pitch and yaw are switched; yaw is rotation about a vertical axis (like steering), so the axis is the radial vector, and pitch is pointing the nose up or down, so the axis is the angular momentum vector.

p. 173 It's a little odd to use the subscript "IJK" on $\boldsymbol{\rho}$ and $\dot{\boldsymbol{\rho}}$ which implies the geocentric equatorial system, as there are few observers at the center of the earth. I would call this system TCE, or topocentric equatorial.

p. 173 $\dot{\boldsymbol{\rho}}_{\mathrm{IJK}}$ in (3–26) has not been given a meaning. One might be tempted to conclude that it's the time derivative of $\boldsymbol{\rho}$, but it's not, because that would need to include a term for the time rate of change of the first transformation; θ_{LST} is dependent on time. Instead, the earth rotation is accounted for in the cross product $\boldsymbol{\omega}_{♁} \times \boldsymbol{r}_{IJK}$ added below. This formula is repeated on pages 406 and 409.

p. 186 Despite its proclaimed applicability to all time systems, Algorithm 14, and the computation of C in the Meeus algorithm on p. 187, is not correct for days with leap seconds; for example, it will give the same answer for 1999-01-01T00:00:00UTC and for 1998-12-31T23:59:60UTC. A better algorithm would be to replace the last term in Algorithm 14, or C in the Meeus algorithm, with

$$\frac{3600h + 60min + s}{secday}$$

where *secday* is the number of seconds in the given day, usually 86400, but sometimes 86401.

p. 244 In Table 4–4, it seems suspicious that all the biases are positive.

pp. 255–257 The azimuth-elevation to right ascension - declination transformation, culminating in Algorithm 28 uses geocentric latitude. The *kind* of latitude should not be specified, because that needs to be supplied with the observations: azimuth and elevation are measured to a particular plane. Typically, this is the local horizontal which translates to *astronomical* latitude which is close to geodetic latitude, so if anything were specified it would be more appropriate to say geodetic latitude here. See Figure 3–12. (Clarification by John Seago.)

p. 257 In Algorithm 28, there is a trigonometric simplification of cos(LHA) and sin(LHA) such that the numerator no longer depends on the decliantion. See http://scienceworld.wolfram.com/astronomy/EquatorialCoordinates.html and note that in (2) and (3), there is no declination on the right hand sides of the equations. The advantage to this is that the LHA can be computed using a two-argument arctangent without ever needing the declination.

p. 263 "…with accuracy of as 0.01°."

p. 263 The last equation does not agree with (3–54); the constant term is 357.5277233° and not 357.52910918°.

p. 264 The parenthetical statement $\lambda_{\text{ecliptic}} \approx \nu_{\odot}$ is not true, rather,

$$\lambda_{\text{ecliptic}} - \lambda_{M\odot} = \nu_{\odot} - M_{\odot}$$

is the applicable formula that leads to the formula for $\lambda_{\text{ecliptic}}$ at the bottom of the page.

p. 267 A two-argument arctangent should be used to calculate α as it can range over all four quadrants.

p. 313 Example 6–2 is ill-considered. Notice that the "final orbit" altitude is written "37,6310" (it should be 376, 310). For comparison value, it should have the same initial and final altitudes as Examples 6–1 and Examples 6–3, with an intermediate altitude of 47, 386 km, because that's what's summarized in Table 6–1 on p. 322. The actual example 6–2 is evidently computed as part of the "Transfer to the Moon" results in the second half of Table 6–1.

p. 318 "… we see that the true anomaly must obey the following restriction to avoid parabolic solution…" The transfer true anomaly he computes would give $e = \infty$, not $e = 1$ (parabolic orbit). Also, the restrictions are broader than he states: if departure is perigee, then $R > 1$ and to avoid *negative* eccentricities, one must have $\cos \nu_b < 1/R$. That is to say, one cannot aim to too close to perigee.

p. 321 The two equations $\tan \phi_{\text{trans}_b}$ and ϕ_{trans_b} appearing on the same line should be more clearly separated.

p. 322 One or the other of the terms "perigee" or "periapsis" should be used consistently, not mixed.

p. 331–334 See comment for p. 107.

p. 339 Section 6.5.2 is entitled "Fixed Δv Maneuvering" but fixed Δv maneuvering is only an incidental discussion; the section is actually about computation of the payload angle whether or not Δv is fixed, as the example makes clear.

pp. 341–342 There are two computations of γ, one at the departure burn and one at the arrival burn. Therefore "initial" and "final" are not appropriate subscripts because they have been used as the overall initial and final quantities, not per-burn. The example shows the calculation of the two angles $\cos \gamma_1$ and $\cos \gamma_2$ with identical formulas but different results.

p. 342 The inclination change is $\Delta i = -28.5°$, but the value used in determining the solution is $\Delta i = 28.5°$.

p. 347 "…and k is the time…" k is not a time; it is a dimensionless integer counting the number of revolutions. The synodic period is

$$\frac{2\pi}{\omega_{\text{int}} - \omega_{\text{tgt}}},$$

not k.

p. 362 The units DU* and TU* are defined in terms of R, which is a ratio, not a measure of length. It appears that the distance unit should be the initial radius, from Example 5–12 (?).

p. 366 Example 6–12, vehicle's initial acceleration is stated as $4.0 \times 10^{-3}\,\text{m/s}^2$ and then restated as $4.0 \times 10^{-6}\ \text{m/s}^2$.

p. 374 First full paragraph is redundant — the same points are expressed in the footnote on p. 372.

p. 375 After second equation; it is not "another assumption" to drop the $\boldsymbol{r}_{\text{rel}}$ dot product term; it is consistent with the first-order criterion stated previously.

p. 375 It should be stated that (6–50) is true *only* for circular orbits. Using the relation $h = r^2\dot{\nu}$ (see inside front cover)

$$\omega = \dot{\nu} = \frac{h}{r^2} = \sqrt{\frac{\mu p}{r^4}}.$$

p. 376 Third equation, the vectors $\boldsymbol{\omega}$ should have the subscript "R."

p. 385 The $\dot{x}$ labels for negative values do not have the 0 subscript.

p. 406, 409 See comments for p. 173.

p. 416 In equation (7–12), the constant term should be

$$-\frac{4\mu^2 D_2^2}{D^2}$$

i.e., the D_2 should be squared.

p. 419 After (7–16), "After recalculating the coefficients…" should be "After calculating the coefficients…"

p. 421, 426, 427 It is confusing to write a matrix as a column of vectors, because vectors are customarily interpreted as column themselves. It is preferable to write them horizontally with the broken vertical lines, as is done on pp. 415–416. Then the matrices should be transposed.

p. 425 In the equations for τ_1 and τ_3, the values for JD_2 should be identical but are not. The values given for τ_1 and τ_3 are correct, however.

p. 426 In the matrix equation after the sentence "This value allows you to find. . . ", delete

$$\text{and} \quad \begin{bmatrix} c_1 \\ c_2 \\ c_3 \end{bmatrix}$$

p. 426 In the matrix equation after the sentence "Determine the inital guess of the slant ranges," delete

$$\rightarrow \begin{bmatrix} \rho_1 \\ \rho_2 \\ \rho_3 \end{bmatrix}$$

(It should be obvious something is wrong, when a negative range is given!)

p. 427 The values of f_3 and g_1 given in the example are switched.

p. 427 In several attempts that I and students have made, no one has been able to reproduce his numbers for refinement. Notwithstanding the switch mentioned above, I get numbers that are only within about 1% of his. One thing to note is that the $c\rho = -Mc$ equation that you iterate on is *very* sensitive to values of c; changing them by a few percent changes the result by 40% in this example. This resubstitution process apparently comes (uncredited) from Escobal *Methods of Orbit Determination*, who says "The convergence of the previous iterative loop has not really undergone much investigation; it is suggested by Moulton." This probably could be solved by some kind of Newton-Raphson iteration but it's not clear why one would need or want an exact answer to approximate data — you might as well use the unrefined result to start the differential correction on all your observed data, which you'll have to do anyway.

p. 428 The unnumbered equations between (7–17) and (7–18) are missing a minus sign on either the left or right sides.

p. 429 (7–20) has "ar_2" which should be "a_2".

p. 429 The sentences right after (7–20) are confusing because a) in order to calculate $\boldsymbol{\rho}$ and thus the angle, one must already have solved for $\boldsymbol{r}$, and b) the connection is not clearly made between "an angle greater than 90°" and the $\cos\theta$ equation following. They should say something like: "The quadratic results in two roots. You can reject the root that yields an angle θ greater than 90° because it implies the satellite is not in the field-of-view, where

$$\cos\theta = \frac{\boldsymbol{r}_{\text{site}i} \cdot \boldsymbol{\rho}_i}{r_{\text{site}i}\rho_i}$$

Consult Escobal . . . "

p. 430 In the third paragraph, f is a function that maps from the observation vector $\boldsymbol{b}$ to the state X. In the last paragraph, it maps the opposite direction.

p. 436 Last equation on page "L" should be "L_g".

p. 443 Last expression for $\boldsymbol{r}_2$, the terms multiplying $\ddot{\boldsymbol{r}}_1$ and $\ddot{\boldsymbol{r}}_3$ can be simplified, e.g. $2\Delta t_{32} - \Delta t_{32} = \Delta t_{32}$, and then simplified further by using definitions of Δt. The complete simplification is given in Algorithm 52 on the next page.

p. 445 "I won't detail Lambert's original method because it requires eight separate cases to provide a complete answer." It doesn't; because of restrictions on the angles, there are only four separate cases. And these four cases are really just all possible combinations of answers to *two* yes/no questions (does the chord vector cross the empty focus? the attracting focus?). See Danby.

p. 447 Below (7-33) he says "I've provided the sine expression to resolve quadrants." It would resolve quadrants if it were really a $\sin \Delta\nu$, but it's not. The one on the left in (7-33) will always be positive because of the absolute value, the one on the right has t_m which he defines as +1 for short way and -1 for long way. But that's all you really need, the numerical sine is not important, all you want is t_m: long way or short way.

p. 447 Sentence and last (unnumbered) equation "For instance…" is a non sequitur; the equation is true regardless of the meaning of a and r.

p. 457 Equation for a should be

$$a = \frac{r_1 + r_2 - 2\sqrt{r_1 r_2}\cos\frac{\Delta\nu}{2}\cos\psi}{2\sin^2\psi},$$

i.e., there is a superfluous $\sin \Delta E$ in the text.

p. 471 The sentence repeating that universal variables will be used is redundant as that fact was stated in the previous paragraph.

p. 471 "This value then adds to 1.0 and the algorithm proceeds. Notice if the vectors are exactly 180° apart, the addition yields 0.0…" These sentences make no sense. For starters how does a value "add" to something?

p. 471 "… represents a Hohmann-like transfer, which we can solve using simple two-body dynamics" is a non sequiter; we're solving *everything* with two body dynamics as is explained at length on the previous page. Instead it should say "… which we can solve using the techniques of the previous chapter."

p. 486 Problem 2, It appears that there are 25 hours in this day — the times go 23:50, 24:10, 24:30, 24:50, 0:10, 0:30.

p. 486 Problem 4, "Be careful of the units because this problem is solved with AU's" is a puzzling statement because no distance units appear in the problem statement at all, and one can pick any units to solve it.

p. 491 The two paragraphs beginning "We're very familiar..." and ending "...predict the orbit of the Moon about the Earth." should be indented and attributed to Bate, Mueller, and White, as they are virtually verbatim from their Section 9.1.

p. 491 Third paragraph: "We're very familiar with perturbations in almost every area of life. Seldom does anything go exactly as planned; rather, it is perturbed by unpredictable circumstances." This is a seriously misleading statement, implying that "perturbation" is synonymous with "random" and "unpredictable." While there certainly are random perturbations (e.g., drag), many are not (e.g., geopotential perturbations). Keep in mind that perturbations are small variations from a known solution, not (necessarily) random variations.

p. 491 Fourth paragraph: "Don't get the idea that perturbations are always small..." Yes, do get the idea — that is how most mathematicians define perturbations. If they do become large enough that they no longer can be considered small, then they are no longer perturbations. Rather, a new analysis must be performed, solving the problem from scratch without assuming a Kepler orbit. For example, the three body problem where the third mass is significant is solved differently than a third body perturbation on the Kepler problem. On p. 573, he gets it right: "[The concept of perturbing forces] describes small forces, relative to the attraction of the central body's point mass, which cause small deviations from the unperturbed motion caused by this central force."

p. 493 "accurate than" is partially underlined.

p. 495–496 Though the title is "Encke's formulation" the text refers to it as "Encke's method" — to be consistent with the Cowell terminology (where the distinction is important), it should be called "Encke's formulation" throughout.

p. 503 Missing right parenthesis $\dot{y}(t_{n+1}) = f(t, y(t))$.

p. 503 Depending on how you count order, these might be called third order methods(?)

p. 504 "The summed differences are a way to add the back differences without having to maintain the individual back differences." No — rather, they are a way of integrating without letting error from the integrated variable accumulate. In either case, individual back differences are only maintained for the $N+1$ most recent points (where N is the order as I count it), and then only if the difference form is used; if the ordinate form is used, back differences are not computed.

p. 505 Despite the statement "Remember that the forumulation is for second order systems. . . ," Gauss-Jackson (second order) integrators are used when drag is present by also computing a first integral.

p. 508 The relation defined by (7–8) is more properly a *generalized* Sundman transformation (see (4–39), where it is correctly stated that the power of r is 1, not 3/2, for a Sundman transformation). Also, "Sundman" is misspelled.

p. 540 Second line: "Even with canonical units,. . . " This is irrelevant — numerical problems stemming from the subtraction of close large numbers are independent of scale factor (such as a unit conversion), as computers storing floating point numbers normalize the mantissa anyway.

p. 544 Below (8–42) it is stated "where $D_{aphelion}$ is the days from when the Earth is at aphelion." This is incorrect, it is 2π times the time since aphelion as a fraction of the whole year.

p. 575 Second paragraph of Section 9.3 implies that perturbations can be something other than small, as in use of the term "weakly," which is redundant. See comment on p. 491.

p. 594 Bottom equation for $\mathcal{H}$ is the negative of the correct expression.

p. 682 "Tracking systems such as GPS . . . rely on a transmitting and receiving clock." GPS with four satellites in view does not rely on a receiving clock.

p. 687 In equation (10–7) and above, b should be a vector $\boldsymbol{b}$.

p. 692 The w_i equation near bottom of the page makes no sense; elements should not be squared and it is 2×2 whereas the rest of the example is eight dimensional. Besides, w_i was a scalar earlier in the page. It's best to forget w_i and define W directly in terms of its elements.

p. 692 In the J equation, sum is to N while the prepatory definitions on the previous page N is specifically 8.

p. 693 The interpetation of the elements of the covariance matrix as the variances of the best-fit parameters is incorrect. They must first be multiplied by the variances of the observations, which is estimated by the sample variances of the observations,

$$\sigma^2 \approx s^2 \equiv \frac{1}{N-2} \sum_{i=1}^{N} \bar{r}_i^2$$

So the standard deviations he quotes should be multiplied by 0.471, which make them not as bad as he thinks. Notice that the covariance matrix *does not measure observation error* in any way; it does not depend on observations at all! It only depends on the times (independent variable values) of observations. A good tip-off that something is wrong is a dimension check: if the dimensions of the independent variable (x) is D, then the dimensions of the covariance matrix are

$$\begin{bmatrix} 1 & 1/D \\ 1/D & 1/D^2 \end{bmatrix},$$

whereas α has the dimension of observed quantity, and β that of observed quantity divided by D. Thus we need to introduce something that gives the dimension of the observable. See Bevington and Robinson for a complete description. (Noticed by Kevin Stefanik.)

p. 694 Unnumbered equation with partial derivatives of the residual $\bar{r}_i$ has x_o subscripted with “1” instead of “i”.

p. 695 A^T is not the same as the linear case so the reference to page 679 should be removed; it should defined as it's not the same as the linear case; it is the $2 \times N$ matrix in the previous equation.

pp. 701–703 The observation quantities inexplicably have a bar over the symbol y.

p. 711 Re “subtract the biases from Table 4-4” see comment above; are all biases positive?

p. 712 In the weight matrix W, the order of the numerical and values and symbols do not correspond. If you follow the left-to-right order in Table 10–2 (range, azimuth, elevation) then the symbols σ_β^2 and σ_ρ should be switched, and the numerical values are in the right order.

p. 712 As a cautionary note, it should be pointed out that the units for the 1,1 element of W are m^{-2}, and the measurement of range in Table 10–2 is in km, so a conversion will be necessary.

p. 713 Apparently, the standard deviation of the state vector is again being incorrectly computed as the square root of the diagonal elements of the covariance matrix. See correction for p. 693.

p. 715 Remove the comma in “the nonlinear, update equation…”

p. 717 Erroneous reference to Equation (10–20) should be to (10–19).

p. 727 It should be stated that $\boldsymbol{z}_k$ refers to the observations.

p. 727 Not clear why R has the subscript k as it doesn't get updated.

p. 727 Last equation of Algorithm 63 should be

$$P_{k+1} = [I - K_{k+1}H_{k+1}]\,\bar{P}_{k+1}.$$

p. 729–730 In the K_1 and K_2 equations, R should not have a bar over it.

p. 742–744 The "ITRF" subscript on the matrices needs explanation/reminder, as it does not occur directly in the index and is apparently only discussed in a footnote on p. 151.

p. 743 "The equations of motion due to the nonspherical Earth depend ..." This statement is true, but it's not clear why he makes it in implicit contrast — it's also true for the two body force.

p. 882 Second sentence of page should say "...the operation is not associative."

p. 883 A 2×2 matrix is not inverted by taking the transpose, changing the sign of the off diagonal terms, and dividing by the determinant. Rather it is inverted by *swapping the diagonal elements*, changing the sign of the off diagonal terms, and dividing by the determinant.

p. 911 Units are not supplied for semimajor axes.

p. 948 The index entry to International Terrestrial Reference Frame (ITRF) should have a reference to p. 158.

back cover "...now available for free in both FORTRAN, PASCAL, and C."

Notation: sometimes vectors are denoted with an arrow $\vec{x}$ (most of the book, e.g., Chapter 7), and sometimes in boldface $\boldsymbol{x}$ (Chapter 10). In Chapter 10, sometimes matrices are in boldface and sometimes not (still true?).