Errata: Midlatitude Synoptic Meteorology (December, 2012)

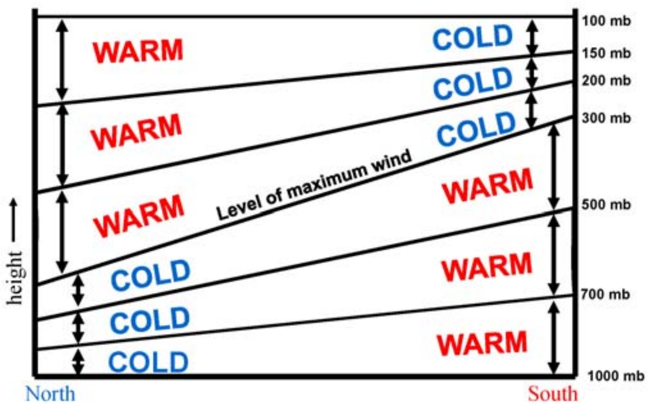
My apologies for the various inevitable errors contained in this first-edition text. We are working diligently to correct them. I gratefully acknowledge the contributions of many students, friends, and colleagues who have noted these "issues" with the text. Most significantly, tremendous appreciation is due to ***Professor John Nielsen-Gammon*** at Texas A&M University, who has provided a thorough list of corrections and suggestions. The vast majority of the items listed below are from his comprehensive review. Profs. Brian Colle, Steven Decker, Russ Schumacher, and Scott Steiger are also credited for their contributions. If you find additional issues, please email them to me at gary_at_ncsu.edu

Sincerely, Gary Lackmann, 12 December 2012

Chapter 1

Page 4, Col 1, top line: Actually, in pressure coordinates, the vertical axis can be taken to point downward, so this statement isn't accurate in that circumstance. Corrected wording: "An exception is found in the case of isobaric coordinates, in which the vertical axis can be taken as directed downward."

Page 13, Col 1, Fig. 1.5: The axis label on the left should be z (height) with an upward arrow. Pressure is already on the right, and it's irregular. Z is the linear, vertical coordinate in this diagram. A revised figure is included below.



Page 14, Col 1, line 7: Although natural coordinates are often defined as mentioned here, to avoid the introduction of a negative sign in eq. 1.39 and subsequent expressions, we can define the $\hat{n}$ direction as pointing towards *higher* values of geopotential. Replace "… is oriented normal to height contours, pointing toward lower values" with "… is oriented normal to height contours, pointing toward *higher* values". Alternatively, one could introduce a negative sign on the right side of equations 1.39, 1.40, 1.41, and 1.42.

Page 14, Col 2: There is some complexity in interpreting equation 1.41, because if the geostrophic wind turns with height, the orientation of $\hat{n}$ also changes. Over a layer in which the geostrophic wind direction changes with height, the $\hat{n}$ direction is taken perpendicular to thickness contours, with larger thickness values in the positive $\hat{n}$ direction.

Page 24, Col 2, top: "The wavelike perturbations…" would better be "Many of the wavelike perturbations…" because some of the small-scale waves in these figures are likely to be gravity waves.

Page 28, Col 1, end of first paragraph: Should read "… strong winds ***or*** a warm lower surface".

Page 30, Col 1, eq. 1.59: Note that this formula gives the turbulent heat flux near the surface.

Page 31, Col 2, Problem 7: The text describing figure colors doesn't match the figure: It should read "The plot below shows a 120-h forecast of 500-mb height (blue contours) with SLP (red contours) superimposed…"

Chapter 2

Page 36, Col 1, end of column: Note that the hydrostatic assumption is made right away, in equations (2.1)-(2.3) because this is used to express the horizontal pressure-gradient force in terms of the geopotential gradient.

Page 39, Col 1, first sentence of last full paragraph: Assumption (i) should be assumption (1).

Page 41, Col 2, top: Note that $\vec{V}_g$ and Φ are also unknowns, but they are observable. Both the height tendency and $\vec{V}_g$ are derivatives of Φ and so the number of true unknowns can be reduced.

Page 48, equation 2.31, figure 2.10, and in several places in the text in Col 2 of p. 49: The more direct expression of the Q-vector is to use temperature, rather than potential temperature. Then, the coefficient out in front of the brackets in (2.31) is just R/p, and T replaces θ in each instance.

Page 58, Col 2, sentence before equation 2.42: Note that $\bar{u}_g$ is a function of z as well as y.

Page 65, Col 2, first full paragraph: It is the vertical motion of air of a given temperature *anomaly* (defined, for example, as a deviation from a latitudinal average), rather than the absolute temperature that is important to the energy conversion (see equation 2.44). In Fig. 2.26, areas of warm anomaly are more likely experiencing ascent, and anomalously cold regions are characterized by descent.

Chapter 3

Page 69-70, equations 3.8 and 3.9: The letter "M" was used in equation 3.3 to denote the mass in a control volume, so to avoid confusion, Ψ_M would be a better choice for the Montgomery streamfunction.

Page 71, Col 1, equation 3.10: This is the equation for vertical air motion, not vertical air motion on an isentropic surface. A nonzero term C means that the vertical motion is NOT constrained to lie on an isentropic surface. The words "… on an isentropic surface" immediately before the equation should be replaced with "in isentropic coordinates".

Page 71, Col 2, third paragraph: A nonzero Term C leads to air parcel movement from one isentropic surface to another, and this could be added to the discussion here to clarify the interpretation.

Page 72, Col 1, equation 3.12: There should not be a vector cross product in the second expression; a dot product belongs there.

Page 74, Col 1, last paragraph: Note that the definition of “system motion” is not simple. One can estimate this by tracking closed centers or features with time, and computing the speed and direction of motion, using this as the storm motion vector.

Chapter 4

Page 88, Col 2, equations (4.21)-(4.24): The Q should be replaced by P in these equations.

Chapter 5

Page 102, Col 1, discussion following equation 5.3: The wording is confusing here- I do not mean to imply that friction is enhancing the growth rate- it is not. Because equation (5.2) is obtained from the frictionless vorticity equation, we should consider this term as being valid above the boundary layer. This statement would better be “In nature, friction limits this growth rate; however, the value of convergence will also tend to increase with the intensity of a system, which partially offsets the spindown.”

Page 105, Col 1, last paragraph of section 5.3.4: For a perfectly sinusoidal pattern, the shorter wavelength is consistent with stronger QG forcing for ascent, as stated. However, in this situation, solving the omega equation and inverting the Laplacian cancels this effect. “While the QG forcing for ascent in this hypothetical situation increases with wavelength, this effect is largely cancelled when inverting the Laplacian to solve for omega.”

Page 106, Col 2, second paragraph: The wording “… and represents an important nonlinear feedback…” should be “… this feedback is consistent with the linear exponential growth mechanism of baroclinic instability (see Chapter 7).”

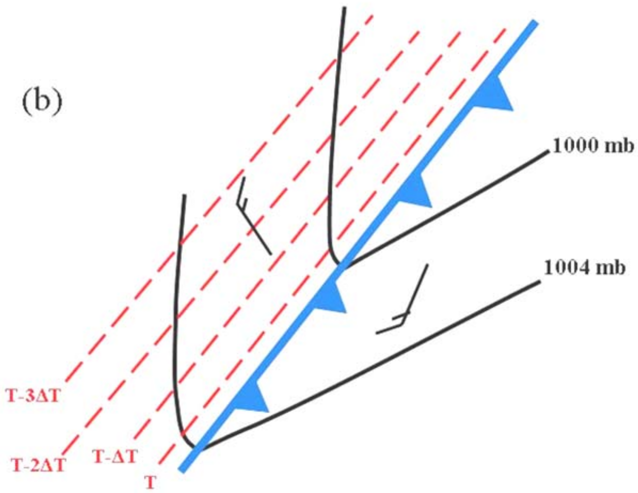
Page 110, Col 1, end of second paragraph: In the sentence reading “This process, referred to as “phase locking”, is also a function of the …”, by “this process”, I am referring to the mutual amplification and constructive cross advections, but I neglected to point out a key effect, which is the propagation effect due to the cross advections. This is separate from the propagation effect of the individual upper and lower waves. For example, suppose that in a situation similar to that depicted in Fig. 5.17 that shear and propagation result in the upper wave to moving too fast. The northerly PV advection induced by the lower wave will act to slow the upper wave, and the southerly warm advection induced by the upper wave will act to speed the lower wave. To correct this, after the sentence ending with “… of their mutual amplification and constructive cross advections.”, change to:

“An additional effect is the change in phase speed in the upper and lower disturbance due to cross advection. Suppose that in Fig. 5.17 that the movement of the upper wave was faster than the lower wave. Northerly cold potential temperature advection induced on the upper boundary by the lower wave will act to slow it, while the southerly warm advection on the lower boundary associated with the upper wave will increase its eastward movement.”

Page 115, Col 2, end of first full paragraph: It should be mentioned that the retarding influence of frictional process can also be stronger over land. The corrected text should read “… is likely due to reduced diabatic contributions, generally greater static stability, and stronger friction.”

Chapter 6

Page 135, Fig 6.2: Prof. Brian Colle (Stony Brook University) reminded me that the isobar curvature on the warm side of the front in panel (b) implies a temperature gradient there, but this is missing in the isotherms. To be consistent, the isobars in the warm sector should be drawn straighter. Prof. Colle and I have designed an improved version of this figure shown below:

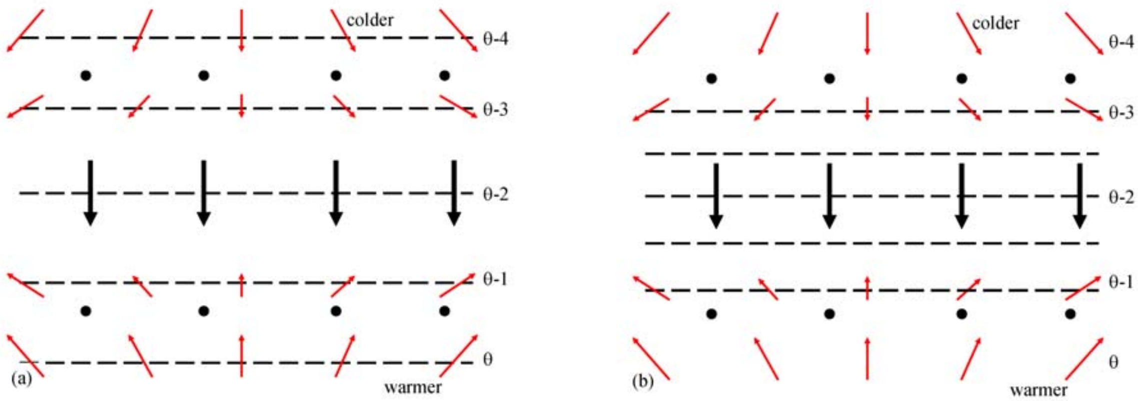


Revised Fig. 6.2b

Page 136, Col 1, bottom: Note that the change in potential temperature gradient ***experienced by the parcel with the strongest potential temperature gradient*** DOES indicate whether the overall front is strengthening or weakening.

Page 136, Col 2, top: Similar to the previous comment, often meteorological software only accounts for the shearing and confluence term on the right side of 6.2. In this case, positive frontogenesis values at the location of the parcel with the strongest potential temperature gradient can either indicate a strengthening front, or that the total frontogenesis including diabatic effects has not been calculated. Revised wording: "Often meteorological software only accounts for the shearing and confluence term on the right side of 6.2. In this case, positive frontogenesis values at the location of the parcel with the strongest potential temperature gradient can either indicate a strengthening front, or that the total frontogenesis including diabatic effects has not been calculated."

Page 144, Col 2, Fig. 6.11 and discussion: It would be very difficult (impossible?) to sketch the pressure or height field with this convergent flow in the original figure because the geostrophic wind is nondivergent (for constant f). The revised Fig. 6.11 is included below.



Page 151, Fig. 6.18a: The cross-section endpoint labels A and B should be reversed.

Page 157, Col 1, lines 5-6: Technically, barotropic vortical flows do not have isentropes, but in the study in question (Schultz et al. 1998) potential temperature is treated as a passive tracer.

Chapter 7

Page 176, Col 2, paragraph below equation 7.90: The sentence reading “The forcing for ageostrophic motions is proportional to the Q vector itself…” should read “The ageostrophic streamfunction is related to the Q vector itself…” Equation 7.90 could be solved for the ageostrophic streamfunction, but then computing the vertical motion would require a horizontal derivative, consistent with the vertical motion being related to the divergence of Q.

Page 178, Col 1, equation 7.113: There is a "2" missing from the denominator of the first expression for s. $s = \frac{N_0 k H}{2 f_0}; \quad L_R = \frac{N_0 H}{f_0}; \quad s = k \frac{L_R}{2}$

Page 179, Col 2, below equation 7.115: The words “time-dependent” should be replaced by “growing or decaying” because the real part of the solution is time-dependent too, as it's propagating.

Page 179, Col 2, bottom: In the form of the Eady problem presented here there is no long-wave cutoff in the Eady model. A long-wave cutoff would have a wavelength beyond which there is no growth; no such wavelength exists here. Lindzen (1994) presents a modified version of the problem that does include a long wave cutoff. Should read “… and in fact there is also an effective reduction in growth at large wavelength akin to a longwave cutoff.”

Page 180, equation 7.120: The RHS of the left expression should be "1".

Page 180, Col 2, below equation 7.122: The equation needed is (7.124), not Euler’s formula (7.94). The text should state “… using (7.124), it is evident…”

Page 180, Col 2, discussion of Fig. 7.6a,c: Section 7.3 presents Eady edge waves, which explains a method for isolation of boundary waves. Add a footnote following the sentence ending with “… from the top and bottom boundaries individually (Figs. 7.6a,c).”

Page 180, Col 2, bottom: If we had included friction in the development, then low-level jets could exist in this model. Also, if one allows a wind maximum at the lower boundary to count as a low-level jet, then they do occur. However, we are here referring to isolated internal wind maxima not due to friction.

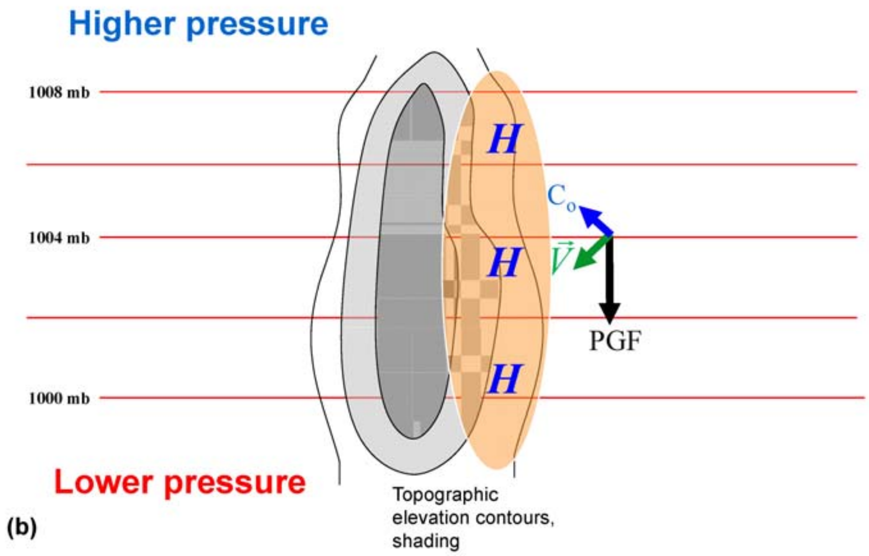
Page 181, Col 2, first full paragraph: Note that the 21 degree phase shift only applies at the upper and lower boundaries. In the interior, the phase shift is as large as 90 degrees.

Page 184, Col 2, discussion below equation 7.129: Note also that c increases with wavelength. Modify to read "… reveals that the phase speed increases with wavelength, and with the basic-state shear…"

Page 185, Col 1, after equation 7.131: Proof of (7.131) was omitted as an end-of-chapter problem. Delete the sentence reading "Proof of (7.131)…

Chapter 8

Page 196, Fig. 8.3b: In order for the wind to slow and deflect as shown in panel (b), a pressure perturbation should already have developed, but this is not indicated by the straight isobars in the graphic. A proper panel (b) would depict the ascent/descent implied by (a), and thus the cooling/warming and the rising/falling pressure. See revised graphic below:



Page 199, Col 1, end: Should be Section 8.2.2, not 8.1.2 as in text.

Page 200, Col 1, Fig. 8.7b: The final fluid shape shown here is not consistent with PV conservation, which would show a convex shape to allow strongest anticyclonic relative vorticity where the fluid is shallowest.

Page 216, Col 1, problem 4: There's no solid black vertical line in these figures, but there is supposed to be one aligned with the zero value. Delete sentence in problem 4 reading "The solid black vertical line indicates zero advection."

Chapter 9

Page 223, Col 1, top: The accompanying adiabatic cooling only partially compensates the warm advection; Figs. 9.3 and 9.7 demonstrate that the warm-advection layer was warming rapidly. To correct, add the word "partially" after "but the accompanying ascent"

Page 231 and 233, Figure 9.13 and accompanying discussion in Col 1 page 231: Panels (a) and (b) in this figure are incorrect. The corrected panels (a) and (b) are shown below:

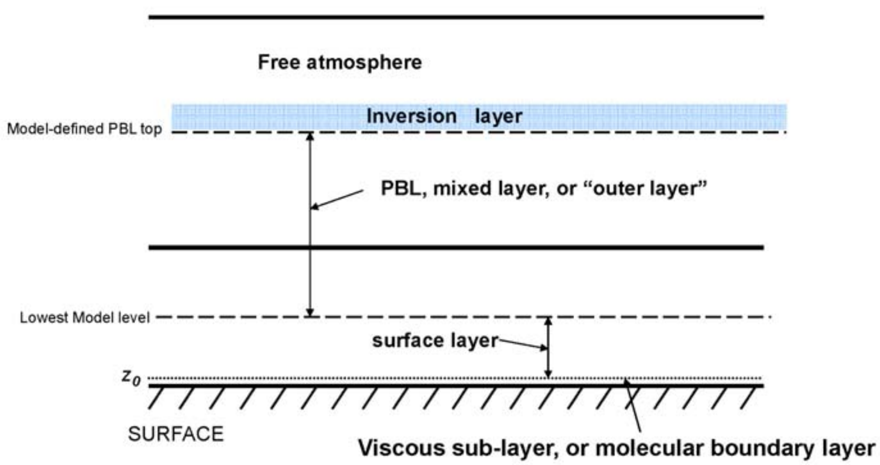
Page 237, Col 1, top: The height of the lakes (75-175 m) should also be taken into consideration when comparing to the dry adiabatic lapse rate. Add “The height of the Great Lakes above sea level should also be taken in to account in this calculation.”

Chapter 10

Page 248, Col 2, 4 lines from bottom: “shown” should be “shone”

Page 255, Col 1, end of second paragraph: The COMET Operational Models Matrix is discussed further in Section 10.7.

Page 256, Col 1, middle: Figure 10.6 does not depict the inversion layer, but this would typically coincide with the layer denoted “model-defined PBL top”. See modified figure below:



Page 277, Col 1, last paragraph: All orbits are characterized by a balance between the centrifugal and gravitational forces. This should be worded “… so as to allow force balance while maintaining a fixed position over the rotating earth…”.

Page 281, Col 2, end of top paragraph: Note that minimizing the cost function provides the most likely estimate for that particular choice of the cost function.

Page 283, Col 2, immediately before equation 10.21: "analog" should be "analogous" and the reference to (10.14) should be to (10.16).

Page 288, Col 2, bottom paragraph: The wording should be “Many model physical parameterizations are often designed using measurements over a relatively small range of conditions, but then are applied over a broad range of conditions when models are run in diverse circumstances.”

Page 294, Col 2, beginning of section 10.7.1.1: The situation is perhaps clearer if we consider the naming at NCEP as corresponding to "model runs", rather than models.

Page 300, Col 1, point 2: NCEP's original model postprocessing was based on a “perfect prog” approach, but this is not the case for MOS approaches used there now. Delete this point.

Chapter 12

Page 329, Col 2, second paragraph of section 12.1: Saucier (1955,1989) is not in the list of references. Reference should be:
Saucier, W. J., 1989: *Principles of Meteorological Analysis*. 2nd ed. Dover, 438 pp.