Comments on "An Improved Technique for Computing the Horizontal Pressure-Gradient Force at the Earth's Surface"

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14 September 1987 and 30 October 1987

1. Introduction

In a recent paper, Sangster (1987) presents a revised version of his earlier work (Sangster, 1960). In both these papers, Sangster makes an effort to treat the troublesome problem of estimating the horizontal pressure gradient force (HPGF) at the earth's surface. Sangster suggests that we abandon the conventional method for doing so; viz., reducing pressure to sea level and then finding the gradient of the sea level pressure. As described in Harrison (1963), the standard method of reduction attempts to reduce the impact of the diurnal temperature wave and makes the so-called "plateau correction" in mountainous regions. Hence, the usual surface observation of sea level pressure (SLP) makes it rather difficult to recover the actual station pressure. Most surface observing sites, including many which do not report SLP, also include a different sort of pressure reduced to sea level, the altimeter setting (or ALSTG), which accomplishes the reduction in a straightforward way using the Standard Atmosphere (see List, 1966, p. 269 ff.). The ALSTG is the basis for the Bellamy's (1945) altimeter correction system of representing the pressure field, which Sangster employs in both his papers (Sangster, 1960, 1987—hereafter referred to as S60 and S87, respectively).

In these works, Sangster has expressed eloquently some of the vexing aspects of trying to employ the SLP data to infer the HPGF. Although Bellamy's method has been around for more than four decades, and it is more than 25 years since Sangster first published his scheme for representing the HPGF, the SLP continues to be the "industry standard" and S87 reveals Sangster's apparent annoyance at the lack of acceptance of his approach. In these comments, I wish to provide some additional explanation for one aspect of Sangster's scheme, to point out some potential problems in using Sangster's approach, and to comment on the case studies presented in S87 with regard to how they support his contention that we abandon a "habit of 100 years duration" in favor of his method.

2. Sangster's geostrophic divergence

Meteorologists are accustomed to regarding geostrophic divergence in pressure coordinates as essentially negligible, arising only from the variation of the Coriolis parameter with latitude. If one neglects this contribution to the geostrophic divergence, the two-dimensional divergence of the HPGF vanishes identically in p-coordinates. In S60 the potential function associated with the geostrophic wind is described but never elucidated, while in S87 the geostrophic divergence is noted to be an artifact of being in the "sigma" system of coordinates (see Pielke, 1984, Ch. 6, for a discussion of these coordinates), but no explanation beyond this statement is given.

Sangster's technique takes advantage of the hydrostatic equation in combination with measurements on a sloping σ-surface (the surface of the earth) to yield the horizontal pressure gradient. Because the surface temperature and pressure and, hence, the density are known, the Sangster method operates effectively by employing this density in the hydrostatic equation to infer the vertical pressure gradient. Another way of seeing this is to consider the well-known relationship that $\alpha\nabla_z p = g\nabla_p z$, where α is the specific volume (or ρ^{-1}, where ρ is the density) and g is the acceleration owing to gravity. If ξ represents some generalized vertical coordinate,

$$\nabla_\xi \phi = \nabla_z \phi + \nabla_\xi z \frac{\partial \phi}{\partial z}, \qquad (1)$$

where ϕ is some scalar function, then by substitution of pressure, p, for both ξ and ϕ, one obtains this standard relationship. However, if we employ the transformation comparable to (1) when going from a p-coordinate system to the generalized ξ-system,

$$\nabla_\xi \phi = \nabla_p \phi + \nabla_\xi p \frac{\partial \phi}{\partial p}, \qquad (2)$$

then by setting $\phi = z$ and using the hydrostatic equa-

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tion, it is easy to show that the geostrophic wind ($\mathbf{V}_g$) satisfies

$$\mathbf{V}_g = \frac{\mathbf{k}}{f} \times [g\nabla_\xi z + \alpha\nabla_\xi p]. \quad (3)$$

In (3), f is the Coriolis parameter and $\mathbf{k}$ is the unit vector in the vertical. From (3) it may be seen that a calculation of the geostrophic wind requires a local value for α, presumably derived from pressure and temperature measurement on the ξ-surface. Clearly, the special case of $\xi = p$ does not require this. It also may be seen readily that the two-dimensional divergence of $\mathbf{V}_g$ on a surface of constant ξ (neglecting the β-effect and any map projection terms) is

$$\nabla_\xi \cdot \mathbf{V}_g = \left.\frac{\partial p}{\partial x}\frac{\partial \alpha}{\partial y}\right]_\xi - \left.\frac{\partial p}{\partial y}\frac{\partial \alpha}{\partial x}\right]_\xi = J_\xi(p, \alpha), \quad (4)$$

where $J_\xi(p, \alpha)$ is the Jacobian operating on p and α in the ξ-surface. Thus, there will be geostrophic divergence on any surface of constant ξ if there are p-α solenoids anywhere within that surface. While in the p-system it is not possible to have any such solenoids, in both the z-system and the various types of σ-systems, p-α solenoids are possible. Sangster's expression for the geostrophic divergence is equivalent to that contributed by p-α solenoids in his σ-surface, which can be seen from the definitions of z_p and S^* in Bellamy (1945).

Another way of looking at the geostrophic divergence is to note that the horizontal divergence in meteorological coordinate systems is not a tensor invariant, although the three-dimensional divergence is. This has been noted, although not in such terms, by Schaefer (1973). Thus, when two-dimensional divergence is measured on a sloping surface, one must include the effects of vertical shear of the horizontal winds to arrive at the true horizontal divergence. Since vertical shear of the geostrophic wind is directly related to baroclinity, the connection to p-α solenoids is obvious.

Finally, it appears that Sangster's equations (11) and (13) in S87 are unnecessarily complex. Equation (10) in S87 implies that

$$\nabla[S_r(z_p - \hat{z}_p)] = S^*\nabla z_p + \mathbf{C}, \quad (4')$$

where $\mathbf{C}$ is an analog to an integration constant that arises from the cancellation of the divergence operators from both sides of equation (10) in S87, and it must satisfy the condition that $\nabla \cdot \mathbf{C} = 0$. Since $\mathbf{C}$ is nondivergent, we can express it as the curl of some vector streamfunction ϕ_c (i.e., $\mathbf{C} \equiv \nabla \times \phi_c$). From (4') it follows that

$$(z_p - \hat{z}_p)\nabla S_r - (S^* - S_r)\nabla z_p = \nabla \times \phi_c, \quad (5)$$

so that equation (11) in S87 becomes the much simpler expression $\nabla_p z = \nabla D' + \nabla \times \phi_c$. It seems clear that one would choose boundary conditions in solving equation (10) in S87 for S_r so as to minimize the contribution from ϕ_c, yielding the result that $\nabla_p z \approx \nabla D'$. Hence, as noted in S87, D' indeed comprises "most of the HPGF" as a result of the near-cancellation of the terms involving gradients of S_r and z_p. Moreover, since $\nabla \cdot \nabla \times \phi$ vanishes identically for any vector ϕ, Sangster's formula for the geostrophic streamfunction (Eq. (13) in S87) reduces to $\nabla^2 H = \nabla^2 D'$. This latter result is not surprising since both H and D' are geostrophic streamfunctions, the Laplacians of which both equal the geostrophic vorticity. Solving Sangster's equation (13) subject to condition (15) is equivalent, therefore, to solving the Laplace equation $\nabla^2(H - D') = 0$ with the boundary condition that $H - D' = -S_r\hat{z}_p$ everywhere on the boundary.

3. Diurnal variations in the surface geostrophic wind

It is of interest to note that Sangster's approach is quite sensitive to the diurnal surface temperature oscillation. In fact, Sangster (1967) has called attention to this characteristic in noting the diurnal variation in the surface geostrophic wind he calculates. Although he considers this to be a desirable characteristic of his approach, it does mean that this calculation of the horizontal pressure gradient force may not be representative of that force above the layer within which the diurnal temperature oscillation is confined.

Sangster takes his diurnal variation of the surface geostrophic wind to imply a corresponding change in the horizontal pressure gradient. Thus, he is suggesting that there is a 24 h change in the low-level pressure gradient over the Plains with an amplitude of roughly 50–100% of that gradient during the diurnal cycle. Since the surface pressure is the weight per unit area of the atmospheric mass above the surface (irrespective of the variable used to represent pressure or the coordinate system employed), this large inferred diurnal change in the pressure gradient must involve some process causing large mass changes every 24 hours. It is of some importance to note that Holton (1967) has shown that, to a first approximation, the pressure field within the boundary layer remains unchanged through the diurnal oscillation in temperature. Thus, it is not clear that diurnal changes in the geostrophic wind are the result of large diurnal variations in the low-level pressure field. This has important implications for Sangster's case studies, the discussion of which follows.

The point of this is to indicate that I believe there is reason to doubt the practical application of this technique to infer "true" horizontal pressure gradients. It is hard for me to accept the reality of such an enormous change in horizontal pressure gradients, although it is possible that the diurnal change in density could cause significant changes (i.e., on the order of 15%) in the geostrophic wind. A more likely explanation for the very large variations in the SGW produced by Sangster's technique is that the employment of the local surface density results in an inaccurate specification of the vertical pressure gradient. An incorrect estimate of the vertical pressure gradient implies that the derived surface "horizontal" pressure gradient is not truly horizontal.[1] If one were to use these estimates for specifying the surface pressure gradient in a primitive equation-based numerical weather prediction model, it is hard to imagine how the model could reconcile this enormous variation in the lower boundary values with the pressure field within the free atmosphere, especially if that model did not have sophisticated boundary layer physics.

4. Sangster's case studies

For the case studies presented in S87, in both §6a and §6b the comparison between Sangster's presentation and his version of the standard approach is confined to showing his surface geostrophic wind (SGW) and the SLP isobars. He notes, quite correctly, that these are quite different portrayals of the HPGF. However, as noted above, there is reason to question the representativeness of Sangster's estimates of the surface pressure gradient. The geostrophic wind involves the product of the density and the pressure gradient, and it appears that a major impact of the diurnal temperature oscillation on his geostrophic wind may be through the density contribution. That is, the variations in Sangster's surface geostrophic wind can occur without significant changes in the pressure gradient, per se. Thus, the comparison made in his case studies may not be entirely to the point.

In the set of cases in §6b, the point is made that the SGW incorporates the diurnal variations associated with the changing temperature. Although mention is made of the observed boundary layer wind maximum (BLWM) in the Plains, the SGW typically reaches its maximum during the afternoon, while it is well-known that the BLWM is a nocturnal phenomenon. If this argument is to be used in favor of the SGW method, it seems to me that some explanation is needed for the significant difference in timing between the SGW maximum and the BLWM. The processes described by Blackadar (1957) and Holton (1967) are essentially ageostrophic and assume the streadiness of the pressure gradient. On the other hand, Bonner and Paegle (1970) have considered the effect of diurnal variations in the geostrophic wind on the inertial oscillation caused by varying boundary layer eddy viscosity. Their simple boundary layer model suggests that an afternoon maximum in pressure gradient can, indeed, amplify the nocturnal wind maximum, and they conclude that "the amplitude of the oscillation is fairly sensitive to . . . the phase difference between eddy viscosity and pressure gradient oscillations." Their model also indicates sensitivity to the *amplitude* of the diurnal variation in the geostrophic wind and does not use an amplitude comparable to the geostrophic wind itself. Thus, their study supports my concerns about the possible deleterious effects of incorporating such a large diurnal variation of the SGW in a primitive equation numerical model.

The case in §6c compares the observed surface winds at selected sites to both the SLP-derived geostrophic wind and his SGW. Using observed surface winds to choose between these disparate approaches implies that there is some more or less fixed relationship between surface winds and the HPGF. While the validity of this assumption at levels above the surface boundary layer is well recognized, it is not clear to me that it holds near or at the surface. Moreover, such a comparison is still hampered by the fact that the SGW incorporates the effects of density variations as well as the horizontal pressure gradient. There is little doubt of the tendency for strong SLP gradients over sloping terrain to be associated with observed surface winds that are substantially subgeostrophic when SLP is used to derive the geostrophic wind, but it is not obvious to me that this tendency is solely (or even primarily) an artifact of the SLP reduction process. It may well be that this is the case, of course, but the evidence presented in this paper is not sufficiently convincing to abandon SLP in favor of Sangster's approach, in my opinion.

5. Summary and conclusions

Sangster has shown that his method is a more direct approach than the standard one for estimating the horizontal pressure gradient force. His theory suggests that his approach is indeed capable of establishing the HPGF at the surface. Although I have tried to offer some additional explanation for the geostrophic divergence (a concept meteorologists may find somewhat unusual), I do not dispute that his theoretical approach is valid. However, I have suggested that the Sangster scheme still may be unsatisfactory for dealing with the problem of estimating the horizontal pressure gradient at the surface, owing to its sensitivity to the diurnal temperature wave. Depending on the application one has in mind, the incorporation of a diurnal oscillation in the HPGF may or may not be desirable. Further, I believe that there is still room for doubt that the pressure gradient near the surface over the Plains actually varies with an amplitude of 50%–100% of the gradient only as a result of the diurnal cycle. My doubt is a practical one, rather than a problem with Sangster's theory—some of the observed variation in Sangster's SGW may be the result of density variations rather than changes in the pressure gradient and I believe that those density variations may also result in a systematically unrepresentative specification of the vertical pressure gradient, as inferred from the hydrostatic

[1] *Note added in proof:* If $\xi = \sigma$ and $\phi = p$ in (1) and one employs the hydrostatic equation, it can be seen that the HPGF is the difference between two large terms. In addition to being sensitive to the density, this difference is very sensitive to the value assigned to the terrain gradient on a σ-surface.

equation. There is no obvious way to address these doubts, short of detailed observations of pressure and density within the lower troposphere over a domain large enough to include the significant terrain features. I know of no such observations, at present.[2]

Sangster has tried to show the superiority of his technique via case studies. Unfortunately, his cases do not really give compelling evidence in favor of adopting his approach. If his cases documented the superiority of his surface geostrophic wind in representing the *true* horizontal pressure gradient at the surface, this would constitute one type of compelling evidence, in my view. Unfortunately, the actual horizontal pressure gradient (and, hence, the true HPGF) remains unknown in such studies. As it is, his cases are not truly enlightening with regard to this issue, but merely show that inferring the surface geostrophic wind from the SLP gradient is not equivalent to his approach for determining the pressure gradient *force* at the surface. This is a consequence of the fact that the surface horizontal pressure gradient force involves both the surface horizontal pressure gradient and the local density.

An approach to validating Sangster's scheme that he has not yet pursued in the formal literature is a convincing demonstration that employment of his SGW would lead to improved weather forecasts, irrespective of its merits or demerits in representing the HPGF. That is, if forecasts based on his SGW are superior in some demonstrable way to those based on SLP, then there is a compelling reason to adopt his scheme. At this point, neither S60 nor S87 have accomplished this particular end.

[2] *Note added in proof:* Parish et al. (1988) recently have measured an amplitude for the variation in the geostrophic wind of about 3 m s^{-1} in synoptically quiescent conditions.

Acknowledgments. I would like to express my thanks to my colleague Dr. Robert P. Davies-Jones for his contributions to my understanding of the nature of geostrophic divergence and for his willingness to discuss the issues involved in these comments. Also, discussions on this topic with Dr. Daniel Keyser have been helpful to me.

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