Comments on “An Operational Ingredients-Based Methodology for Forecasting Midlatitude Winter Season Precipitation”

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ABSTRACT

Wetzel and Martin present an ingredients-based methodology for forecasting winter season precipitation. Although they are to be commended for offering a framework for winter-weather forecasting, disagreements arise with some of their specific recommendations. In particular, this paper clarifies the general philosophy of ingredients-based methodologies and shows how the methodology presented by Wetzel and Martin has the potential to be misinterpreted by their choice of diagnostics (including their PVQ and the so-called traditional techniques) and their use of cloud microphysics. Given that winter-weather forecasts are imperfect at present, this paper advocates continued exploration of scientifically based forecasting techniques.

1. Introduction

Wetzel and Martin (2001, hereafter WM) offer an ingredients-based methodology (IM, when referring to WM’s ingredients-based methodology) that aims to be “an operational tool for the analysis and prediction of winter precipitation events” (WM, p. 156). Although their efforts to wean scientists, forecasters, students, and others off the so-called traditional techniques (i.e., the synoptic-climatology method, the Cook method, the Garcia method, the magic chart, and the LEMO technique; described in WM, p. 158) are commendable, we feel there are problems with their proposed methodology. Whereas WM rightly stress important, often underappreciated, issues about the processes involved in synoptic-scale aspects of winter precipitation, we believe that some parts of their method may mislead at times. In addition, we believe that some statements in WM are internally inconsistent and could be interpreted in ways the authors did not intend.

In section 2, we define and clarify the general philosophy of ingredients-based methodologies in comparison to the IM presented in WM. We discuss the problems with applying quasigeostrophic (QG) reasoning, specific measures of instability, and the combination of these two measures into WM’s PVQ diagnostic in section 3. Section 4 surveys the role of cloud microphysics in winter precipitation. Section 5 expresses our dissatisfaction with the continued use of the so-called traditional techniques and WM’s use of them. Section 6 concludes these comments.

2. Use of ingredients-based methodologies

The concept of ingredients-based methodologies was developed by Doswell (1987), Johns and Doswell (1992), Doswell et al. (1996), and Schultz and Schumacher (1999), drawing upon earlier work by McNulty (1978, 1995). In section 2a, we expand on the discussion of ingredients-based methodologies by WM in their section 2 by further clarification of ingredients-based methodologies, which may not have been clear in the past. In section 2b, we show some interpretations of ingredients-based methodologies by WM with which we take issue.

a. Concepts behind ingredients-based methodologies

The notion of ingredients-based methodology is predicted upon taking advantage of what is understood about the processes producing a particular weather
event. A proper ingredient is defined to be a necessary item, associated unambiguously with the event. Ingredients defined in this way are necessarily general. For example, although precipitation is essentially associated with the ascent of moist air, the physical process by which ascent is created has deliberately not been specified, as ascent can arise from many different processes on virtually any scale. What is not specified with an ingredient is its sufficiency—the absence of the ingredient precludes the event, but the presence of the ingredient is not enough by itself to produce the event. With some ingredients, there is no way a priori to specify how much of an ingredient is enough. In the presence of near saturation, only a small amount of ascent would produce at least some precipitation, whereas if the air is only moderately moist, more ascent is needed. Nor is it possible to prespecify the temporal and spatial scale of the ascent.

Moreover, it may not be possible to define a complete set of ingredients for some events. For example, the current knowledge of tornadogenesis does not permit the specification of an ingredient without which deep convective storms cannot produce tornadoes. If and when our understanding of tornadoes permits such specification, that information can be incorporated into ingredients-based tornado forecasting.

Thus, it could be argued our notion of an ingredient is not very useful in forecasting the weather. Doswell et al. (1996) argued explicitly that the ingredients for some events (e.g., flash-flood-producing rainfalls) could be assembled in so many different ways that being precise about the details of the ingredients would be futile. Although an ingredients-based methodology does not yield a checklist or a recipe for forecasting events, its advantage is to provide focus on only those aspects of a situation that are unambiguously relevant. Given one can examine numerous diagnostic and prognostic products, focusing on the ingredients keeps the job manageable.

b. Wetzel and Martin’s ingredients-based methodology (IM)

Within the context of the previous summary of ingredients-based methodologies, our disagreements with WM become apparent. For example, WM (p. 157) note:

The approach presented here also makes a clear distinction between ingredient and diagnostic. Ingredients are the physical elements or processes directly involved in a meteorological event, while diagnostics are the observable or derived quantities that can be used to assess the presence and strength of an ingredient. Previous work has often blurred this distinction, as illustrated by the use of the mixing ratio as an ingredient by Doswell et al. (1996) and Nietfeld and Kennedy (1998). Mixing ratio is actually only one of a number of parameters that can be used to quantify the moisture availability and, thus, is more appropriately considered a diagnostic of the moisture ingredient.

We agree it is important to make the distinction clear between an ingredient and a diagnostic. Given WM’s explanation of an ingredient and a diagnostic, it is not clear why they consider “forcing for ascent” as an ingredient (pp. 158–159). It seems to us, by both our and WM’s definitions, that “forcing” is a diagnostic for the ingredient of ascent. It is quite possible to have forcing for ascent (especially using their choice for a diagnostic: \(\nabla \cdot \mathbf{Q}\)) without actually having any ascent and, consequently, no precipitation (e.g., Stensrud and Maddox 1988). Further discussion of QG diagnostics is found in section 3a of these comments.

We concur with WM’s argument that mixing ratio is most properly viewed as a diagnostic for the generic term moisture, but we believe this usage was reasonably clear in Doswell et al. (1996). The word moisture is ambiguous, unlike “ascent,” for which \(w = dz/dt\) is a direct measure. The advantage to using mixing ratio (or specific humidity) to represent moisture, as done in Doswell et al. (1996), is that it is a direct measure of water vapor content. For example, relative humidity requires temperature and dewpoint temperature requires pressure.
to have the same knowledge mixing ratio (or specific humidity) alone provides.

Finally, WM have taken ingredients-based methodologies a step farther, and created a methodology that produces quantitative precipitation forecasts. This is both encouraging and discouraging. It is encouraging because it makes an attempt to use the basic ingredients-based methodology to develop a scheme to produce forecasts. It is discouraging because WM’s Table 3 represents the sort of problem that led us to be advocates of ingredients-based forecasting methodologies. Table 3 provides what will be interpreted and applied by many as a checklist of parameters, complete with predefined thresholds or “critical values.” This, in our view, is the antithesis of ingredients-based methodologies. The advantage to thinking in terms of ingredients, rather than specific diagnostic variables with accompanying critical values, is that it explicitly accounts for the diverse ways in which the necessary ingredients are concatenated. However much ingredients-based thinking might have been used to develop Table 3, we feel that it is the presentation of such a result in WM that obviates the advantages of the ingredients-based methodology.

3. The PVQ diagnostic deconstructed

We disagree with WM’s use of PVQ, a diagnostic derived from the product of the saturated equivalent (or moist) geostrophic potential vorticity [PVes in WM; MPV in Schultz and Schumacher (1999)] and the Q-vector divergence. In this section, we describe our concerns with employing the Q vector, the saturated equivalent geostrophic potential vorticity, and their product.

a. Quasigeostrophic thinking

The stated purpose of WM is to provide a systematic approach to forecasting winter weather. As noted in section 2, ingredients-based methodologies do not presuppose particular scales of motion. Indeed, observations of winter weather show snowfall can occur on a variety of scales (e.g., Kocin and Uccellini 1990, chapter 6) from orographically modified flows (e.g., Reinking and Boatman 1986), lake-effect precipitation (e.g., Niziol et al. 1995; Steenburgh et al. 2000), subsynoptic-scale features associated with convective elements (e.g., Holle and Watson 1996), and narrow mesoscale bands associated with frontogenesis (e.g., Sanders and Bosart 1985; Martin 1998; Trapp et al. 2001) to broad regions associated with synoptic-scale (i.e., QG) ascent (e.g., Uccellini and Kocin 1987). Clearly, many situations exist where non-QG processes can be important to winter-precipitation distribution, a point WM note twice in their paper (pp. 159 and 166). Nevertheless, by explicitly neglecting non-QG processes in their advocating the PVQ diagnostic, specifically, and their IM, in general, WM implicitly endorse the omission of those processes from their ingredients maps.

A final point about QG diagnostics is their magnitude depends on the horizontal grid spacing of the data from which they are computed. Smaller grid spacings imply larger magnitudes of computed QG-diagnostic quantities and more noisy fields, unrelated to the scales at which such diagnostics are relevant (e.g., Barnes et al. 1996). Wetzel and Martin do not state the resolution of the ETA grids in their section 4 (e.g., some grids are sent from the National Centers for Environmental Prediction at reduced resolutions owing to bandwidth limitations) or whether filtering was performed (e.g., Barnes et al. 1996). Classifying weak, moderate, or strong forcing (WM, Table 2) by predetermined magnitudes of derived quantities such as \( \nabla \cdot Q \) are misleading unless these issues of resolution and filtering are specified clearly.

b. Issues with instabilities

The second component of the PVQ diagnostic, the instability measure, is also of concern. First, selecting only the negative values of PVes implies only instability (inertial, \(^1\) buoyant, or symmetric) leads to heavy precipitation. Even though WM state on p. 158 that heavy precipitation can occur in the absence of instability, other text in WM seems to neglect that caveat. For example, “If an important ingredient is not expected to be present at the next forecast hour, precipitation is unlikely to occur at that hour” (p. 157). Second, WM state, “only further analysis of cross sections can determine whether a vertical or slantwise response [to regions of instability] can be expected” (p. 159). Wetzel and Martin neglect the possibility that horizontal maps of layer-averaged quantities measuring stability, like \( d\theta/dp \) or \( d\theta_{\text{ens}}/dp \), are possible, without reference to cross sections. Also, horizontal maps of lapse rates have long been used by Storm Prediction Center forecasters and others (e.g., Doswell et al. 1985) to identify regions of midlevel conditional instability.

c. The PVQ diagnostic

Even in the absence of the above issues about QG theory and instability measures, we are concerned about the construction of the PVQ diagnostic or a similar replacement. First, constructing the product of the Q-vector divergence and PVes, when both are negative, assumes the two to be collocated. Because Q-vector convergence indicates forcing for ascent and negative PVes indicates instability, often these two are not coincident. For example, solutions of the Sawyer–Eliassen equation (e.g., Emanuel 1985) show that the maximum ascent is offset toward the warmer, more unstable air relative to the forcing. In other situations, the instability may be present above the forcing. For example, Trapp et al. (2001, Figs. 5 and 6) show elevated conditional insta-

\(^1\) At this time, the exact relationship between inertial instability and heavy precipitation, if any, is not known explicitly.
4. Precipitation efficiency and cloud microphysics

Wetzel and Martin assess the precipitation-efficiency ingredient of their IM using results from microphysical studies of hydrometeors. In doing so, they omit important caveats that users of such results should be aware of. While we believe an efficiency ingredient should be an important part of their IM, we feel WM have provided temperatures that may be incorrectly interpreted as required for heavy snowfall, despite some research to the contrary. Specifically, in this section, we will discuss the historical background for such temperature thresholds and the shortcomings of these measures for precipitation efficiency. In addition, we emphasize additional complications in determining the occurrence of sleet and freezing rain.

a. The ice-nucleation threshold

Wetzel and Martin consider $-10^\circ\text{C}$ the operational cutoff for no ice; that is, if the cloud is warmer than $-10^\circ\text{C}$, then cloud ice, and hence snow, is not likely to form. Cloud-physics studies in the 1950s and 1960s (e.g., Mason and Maybank 1958; Mason 1960; Roberts and Hallett 1968) showed the threshold temperatures for ice to form on silicate particles are between $-10^\circ\text{C}$ and $-20^\circ\text{C}$. However, results from 64 soundings taken during snowfall events at Albany, New York; Minneapolis, Minnesota; and Denver, Colorado, show the cloud-top temperature was between $-10^\circ\text{C}$ and $-5^\circ\text{C}$ in six events (Fig. 1); no snowfall reports were associated with cloud-top temperatures greater than $-5^\circ\text{C}$. Therefore, although the $-10^\circ\text{C}$ cloud-top temperature threshold appears in some studies, questions exist about its potential usefulness for every situation.

b. The temperature of maximum depositional growth rate

Wetzel and Martin assert temperatures around $-15^\circ\text{C}$ in a region of strong forcing for ascent are favored for heavy snow. Indeed, the dendritic crystal habit grows at this temperature (see the review in Mason 1971, section 5.5.1; Rottner and Vali 1974) and the highest den-

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**Fig. 1.** Scatterplot of the cloud-top temperature ($^\circ\text{C}$) vs pressure at cloud top (hPa) from 64 soundings during snowfall events at Albany, NY; Minneapolis, MN; and Denver, CO. Courtesy of P. Roebber, University of Wisconsin—Milwaukee.
dritic growth rate by deposition occurs near this temperature (e.g., Mason 1953; Jayaweera 1971; Ryan et al. 1976; Takahashi et al. 1991). Also, the growth of dendrites through deposition and aggregation has been observed to produce the greatest snowfall rates and accumulations, if the high snowfall rate continues for a relatively long period (e.g., Auer 1971; Auer and White 1982; Lawson et al. 1998). Nevertheless, there are other processes that act to create large snowfall rates. For example, after nucleation, ice crystals can grow through continued deposition, aggregation, and riming (e.g., Pruppacher and Klett 1978, p. 27). Therefore, choosing a single temperature regime in which ice crystals grow most rapidly is an incomplete way to view the complex precipitation processes occurring in a cloud. For example, Stewart (1992) found enhanced aggregation can occur within deep isothermal layers near 0°C. Thus, simply knowing that the location of the −15°C isotherm coincides with strong ascent may not be sufficient to determine the potential for heavy precipitation.

c. Snow at warm temperatures

Wetzel and Martin’s IM also does not account for snow when the surface temperature is greater than 0°C. Using laboratory experiments, theory, and observations, Matsuo and Sasyo (1981a,b) demonstrate falling snowflakes do not melt completely when the air temperature exceeds 0°C and the subcloud air is unsaturated. As an ice crystal enters the warm subcloud air, cooling from evaporation is greater than warming from conduction and the temperature at the surface of the ice crystal remains subfreezing. Matsuo and Sasyo (1981a,b) show snow can occur when the relative humidity is 40% and the temperature is as high as 7°C. In addition, sensible cooling of the air by melting snow can also cause a changeover from rain to snow, a factor noted in several forecast busts (e.g., Bosart and Sanders 1991; Kain et al. 2000) and omitted from the IM proposed by WM.

d. Freezing precipitation

In determining precipitation type, WM advocate “a rough characterization” from the 850-hPa −4°C isotherm as the delimiting value for snow. Although their case study shows the −4°C boundary is too rigid (e.g., the rain at Milwaukee described on p. 164), WM continue to advocate this critical value (p. 163). In this section, we highlight some issues worth considering in evaluating precipitation type.

Wetzel and Martin state, “If the cross section indicates that the wet-bulb temperature remains everywhere below zero, the precipitation will remain frozen and fall as snow” (p. 160). This assertion may not always be true. Huffman and Norman (1988) and Rauber et al. (2000, 2001a,b) have shown many cases of freezing precipitation at the surface associated with soundings completely below freezing.

Further, WM do not provide any scientific evidence, aside from a personal communication, that the depth of the warm layer is generally proportional to the maximum wet-bulb temperature (WM, p. 160). To examine the validity of this statement, the wet-bulb temperature warm-layer depth is compared to the maximum wet-bulb temperature for 68 rawinsonde soundings during freezing-rain events at Albany and Buffalo, NY; Spokane, WA; Greensboro, NC; and Peoria, IL, from the Robbins and Cortinas (2002) dataset. The line represents the linear fit to the data: $y = -0.16 + 0.0026x$, with a correlation coefficient of 0.76.

Thus, the advice WM offer for determining the precipitation type on p. 160 is overly simplistic. That a complicated scheme such as Rauber et al.’s (2001a, Table 1) six-category classification does not provide an unambiguous partition for discriminating freezing rain from ice pellets is just one piece of evidence that should invite a reconsideration of such simplified approaches as WM’s to the difficult problem of forecasting precipitation type.
e. Relative humidity

Wetzel and Martin assert a threshold of relative humidity of 80% be employed for the moisture ingredient. As detailed by Schultz and Schumacher (1999, p. 2725), a single threshold is too simple. Certainly, 80% relative humidity in an observed sounding does not imply saturation, even given instrument error. For numerical models, whether saturation is considered to have been attained depends on the particular moisture schemes employed. For example, in the version of the Eta Model operating in September 2001, stable clouds in the explicit moisture scheme begin to develop at a relative humidity threshold of 95% at the earth’s surface, with that threshold decreasing linearly between the 1st and 10th model levels to 80% at a water grid point and 75% at a land grid point, with that threshold remaining constant at that 10th-level value at all levels above (G. Manikin 2001, personal communication). Therefore, it is incumbent to be aware of details in the model formulation when applying such simple rules.

5. Beyond the so-called traditional techniques

The so-called traditional techniques (i.e., the synoptic-climatology method, the Cook method, the Garcia method, the magic chart, and the LEMO technique; described in WM, p. 158) are ad hoc ways of dealing with the considerable complexity of quantitative precipitation forecasting. Not only do these “traditional techniques” lack many of the ingredients identified by WM to be important for winter precipitation forecasting (their Table 1), some (e.g., the magic chart, the Cook method) are not based on any processes related directly to snowfall production. That these techniques are perceived to be useful, often within a few inches of the observed values (e.g., Nietfeld and Kennedy 1998), is perhaps more a testament to luck than the technique capturing the physical processes involved. Moreover, statistically rigorous studies of a large number of scenarios over a wide range of events in an operational forecasting environment have not been performed with these techniques. While these techniques may have been useful in their time, we assert that they should be abandoned in favor of more scientifically based means of determining winter-precipitation amounts, which likely rely on numerical weather prediction.

Therefore, that WM would suggest these techniques in their Table 1 are incomplete demonstrations of ingredients-based methodologies, then state “the physical basis and flexibility of the IM can be coupled with the quantitative predictive guidelines of a traditional technique” and employ “the Garcia method in collaboration with the IM” (p. 165) puzzles us, especially after claiming the use of traditional techniques “is now unnecessary given the analysis and diagnostic tools available” (p. 156). We believe forecasters, researchers, students, and others would do best to focus on the actual processes producing winter precipitation as ingredient-based methodologies suggest and couple that information with quantitative precipitation forecasts from operational forecast models, suitably modified depending on how the forecast and/or atmosphere is evolving.

Finally, even if forecasts of liquid precipitation amount were perfect from numerical weather prediction models, conversion of such forecasts to snowfall amount is not unambiguous. Since most current numerical modeling systems do not predict snowfall amount explicitly, some method must be assumed to estimate the snowfall depth from the snow-water equivalent. A common approach is to assume a 10:1 ratio of freshly fallen snow to liquid precipitation, equivalent to a snow density of 100 kg m\(^{-3}\). Observations of this ratio from freshly fallen snow at six locations across the western United States and Alaska range from less than 5:1 to greater than 25:1 (LaChapelle 1962; reproduced in Doesken and Judson 1997, p. 15; Judson and Doesken 2000). In addition, there are numerous problems with measuring snowfall including sublimation, compaction, drifting, and the frequency of snow-depth measurement (Doesken and Judson 1997; Doesken and Leffler 2000). Clearly, our ability to improve snowfall forecasts depends on understanding these processes beyond applying a simple 10:1 ratio.

6. Conclusions

In these comments, we raise a number of issues with the ingredients-based methodology (IM) proposed by WM. Wetzel and Martin (2001) state “the efficacy of the IM is not restricted to specific synoptic or thermodynamic conditions” (p. 166). Unfortunately, their choice of diagnostics (e.g., QG) restricts the inherent flexibility of such an ingredients-based methodology. Had WM stuck to the traditional application of ingredients-based methodologies (section 2), then their methodology would have been more relevant to a larger variety of winter-weather situations.

These criticisms of WM’s ingredients-based methodology should not be interpreted as a criticism of scientific-based studies of winter-weather processes for the purpose of improving forecasting. Wetzel and Martin make a move in the right direction, toward a process-oriented approach to forecasting, instead of a rules-of-thumb-based approach. We believe that they do not go far enough, however. We hope the comments raised here encourage others to investigate physically based processes of winter weather and develop methodologies, both physically based methods and numerical model

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3 The Rapid Update Cycle (RUC) is an exception. Snow accumulations are calculated using a 10:1 ratio between snow depth and snow-water equivalent. The snow-water equivalent is not diagnosed based on temperature, but is forecast explicitly through the mixed-phase cloud microphysics in the model (http://maps.fsl.noaa.gov/vartxt.html).
techniques, that can be employed to improve winter-weather forecasts.

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