Reply

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1. Introduction

In a recent paper (Wetzel and Martin 2001, hereafter WM), we introduced an ingredients-based methodology (IM) that provides operational forecasters with a systematic winter season precipitation forecasting technique grounded in physical principles. We feel, as do a number of forecasters in National Weather Service Weather Forecast Offices (NWS WFOs) who have employed the IM, that it provides an effective framework for assessing the importance of five elemental physical ingredients involved in the production of winter precipitation events: forcing for ascent, moisture, instability, precipitation efficiency, and temperature. Schultz et al. (2002, hereafter SCD), while commending our efforts as a step in the right direction, nonetheless suggest that there are a number of fundamental flaws in our work. Most notably, they argue that the IM is deficient in its definition and application of ingredients, and overzealous in its introduction of the PVQ parameter. We feel that these criticisms are unjustified and often based upon an incomplete reading of our manuscript.

In section 2 of this reply we offer detailed responses to these criticisms in which we will

- clarify the distinction between ingredients and diagnostics as applied in the IM,
- emphasize the flexibility and adaptability of the IM by reiterating the primacy of ingredients over diagnostics in the methodology,
- expose the various misreadings of WM by SCD that led them to view the PVQ parameter as likely to ''endorse inattention'' to the ingredients and their diagnostics, and
- reissue the invitation to interested parties to extend and improve the diagnostics employed in the methodology we have outlined.

We offer some final comments in section 3.

2. Responses to criticisms

a. Definition, choice, and application of ingredients

Central to an understanding of the IM is the distinction between an ingredient and a diagnostic. This distinction is clearly stated in WM where we define an ingredient as ''a fundamental physical element or process that directly contributes to the development and intensity of a precipitation event'' (WM, p. 157). We then define diagnostics as ''the observable or derived quantities that can be used to assess the presence and strength of an ingredient'' (WM, p. 157).[1] Furthermore, we note that ''parameters will be introduced to diagnose each ingredient; however, the IM is not dependent on these specific diagnostics.''

SCD take issue with our inclusion of ''forcing for ascent'' as an ingredient to winter precipitation events, arguing that the ascent itself (ω) be considered the ingredient. There can be no disagreement that ascent is required for precipitation production. However, since a given value of ω is influenced both by the degree of forcing and the stratification, ω itself is not a fundamental element as required by our definition of an ingredient. It is true that any number of diagnostic mea-

[1] Although we refer to the ''forcing for ascent'' ingredient throughout the body of the paper, we inadvertently referred to it as the ''quasigeostrophic forcing for ascent'' ingredient in the introduction and conclusions. This may have caused some confusion regarding one of the ingredients (forcing for ascent) and one possible means of diagnosing it (i.e., using quasigeostrophic theory). We appreciate the opportunity to clarify this matter.

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sures of the forcing for ascent can be used, some with more significant limitations than others, but there can be no ascent without forcing for ascent. On this basis we defend our decision to isolate the forcing for ascent as a fundamental process that contributes to the development of a precipitation event.

Our choice of Q-vector convergence[2] as the diagnostic of the forcing for ascent ingredient comes with restrictions, namely a scale dependence, as discussed in WM (pp. 159 and 166). We chose to employ the Q vector because it offers the distinct advantage of simultaneously capturing a considerable amount of the synoptic-scale forcing as well as some frontal forcing in a simple and familiar diagnostic. The limitations of this choice are clearly acknowledged in WM:

> ''reliance on the Q vector as the sole diagnostic used to evaluate the forcing for ascent ingredient here is limiting. Use of this diagnostic excludes explicit consideration of the effects of ageostrophy on the redistribution of temperature and momentum and, thus, on the vertical motion forcing itself as suggested by Eliassen (1962).'' (WM, p. 166)

The IM we have outlined does not require employment of the QG diagnostic as the only diagnostic for assessing the forcing for ascent ingredient. At the heart of the IM is its flexibility to easily incorporate new means of assessing the ingredients. Therefore, we refute the suggestion made by SCD (section 3b) that WM ''endorse the omission'' of non-QG processes by virtue of the IM's use of the Q vector. The effectiveness of the IM as a *methodology* is not compromised through employment of a scale-dependent *diagnostic* for one of the ingredients because any alternative diagnostic can easily be incorporated into the IM. In fact, WM suggest that future extensions of the IM may use a different diagnostic for the forcing for ascent ingredient.[3]

Regarding our choice of ingredients, SCD imply that our exclusion of precipitation rate as an ingredient is an error. We agree that precipitation rate is closely tied to precipitation intensity; in fact, the two are nearly synonymous. However, we contend that precipitation rate is an intermediate, not fundamental, parameter because it is a derivative of more fundamental processes (strength of forcing for ascent, the stability of the stratification, and the availability of moisture), which are ingredients in the IM. We therefore stand by our more restrictive definition of ingredient and reiterate that precipitation rate does not qualify as an ingredient under that definition.

[2] We agree with SCD that we should have alerted readers of WM to the horizontal resolution of the National Centers for Environmental Prediction Eta Model output we employed in our analysis (80 km); the grid spacing can affect the magnitude of the QG diagnostic quantities (Barnes et al. 1996).

[3] For example, the operational ingredients maps available online at http://speedy.meteor.wisc.edu/~swetzel/winter include fields of frontogenesis calculated using the full horizontal wind.

Careful, appropriate, and constructive application of any forecast tool is critical to assessing its utility. SCD are particularly troubled by WM's Table 3, which summarizes the ingredients we consider and the diagnostics we use to measure them. We are confident that the operational meteorologists who constitute a large percentage of the readership of this journal, being trained scientists, will not employ the IM as a ''checklist of parameters'' as feared by SCD. To do so would require an unlikely inattention to the accompanying textual explanation. As we make clear in sections 3f and 4 of WM, the physical basis of the IM, its most important and powerful attribute, can only be effectively incorporated into the forecast process by performing a detailed analysis of the ingredients. Table 3 is simply meant to serve as a general, experience-based starting point for such an analysis of the distribution of ingredients and application of the IM.

b. The PVQ parameter

It is well known that enhanced vertical motions might be expected where forcing for ascent and instability are collocated. In WM we introduced the diagnostic parameter PVQ (the product of PV_{es} and $\nabla \cdot \mathbf{Q}$ when both are negative) to assist in identifying such collocations. SCD's criticism of PVQ rests on the incorrect suggestion that WM expect and intend PVQ to identify areas where *any* snowfall is expected, and to do so independently of the other elements of the IM. We do not expect PVQ to perform as such an all-purpose winter precipitation prediction tool. Instead, PVQ is intended only as a graphical aid for identifying areas where PV_{es} and $\nabla \cdot \mathbf{Q}$ are collocated and, thus, where precipitation rates may be enhanced and convection may occur.

SCD are troubled by the fact that we depict only the negative values of PV_{es} in our Figs. 1c, 2c, and 4a. They state that doing so ''implies that only instability (inertial, buoyant, or symmetric) leads to heavy precipitation'' (SCD, p. 8). This inference is clearly at odds with what is actually stated in WM regarding this issue:

> It is important to remember that PV_{es} need not be negative for significant precipitation to fall. In fact, heavy snow often occurs when sufficient moisture and strong forcing are present in a stably stratified atmosphere. *Therefore, contours of PVQ should only be used to identify areas of potentially convective snowfall.* (WM, p. 159)

Furthermore, SCD contend that the case study presented in WM illustrates that ''focusing on the PVQ diagnostic underrepresents the area affected by precipitation, even when moderate and heavy'' (SCD, section 3c). This comment reflects SCD's misconception regarding PVQ and ignores the fact that heavy snow can occur in the absence of convective activity. The latter concept is reinforced by the analysis presented in our case study. In Fig. 2d of WM, PVQ occurs in only the southeast corner of the state, coincident with the station where thundersnow was reported on the surface maps shown in Fig. 3 of WM. The overall extent of precipitation throughout the region is in good agreement with the areas where there is forcing for ascent in the presence of ample moisture. Furthermore, the areas of moderate to heavy snow from northeastern Iowa through central Wisconsin correspond quite well to the region where *moderate to strong forcing* occurred in the presence of ample moisture and the *absence of instability*. Our analysis of the details of the precipitation distribution in this case did not, and could not, rely on a solitary focus on PVQ. We do not advocate or suggest such a use of this diagnostic parameter.

SCD also accuse us of making unproven generalizations concerning the importance of symmetric instabilities with regard to precipitation organization. We simply state: ''Operationally, it is important to distinguish between gravitational and symmetric instabilities only insofar as the atmosphere may respond differently to each type of instability with respect to the organization of precipitation bands'' (WM, p. 159). Our choice of the word ''may'' demonstrates that we acknowledge the unresolved role of symmetric instabilities in the atmosphere as discussed in Schultz and Schumacher (1999). Furthermore, we indicate that the distinction between gravitational and symmetric instabilities ''is not emphasized in the IM because both instability mechanisms have similar implications, namely increased snowfall amounts and the potential for lightning and thunder'' (WM, p. 159).

In summary, a careful reading of our paper makes clear that PVQ is meant only to be a graphical aid that alerts a forecaster to the fact that two important ingredients (which can, synergistically, produce enhanced vertical motions) are coincident in time and space. When combined with an analysis of the five fundamental ingredients, PVQ can be a convenient, practical tool to help highlight where precipitation rates may be enhanced. Used in isolation, as a magic parameter, PVQ would indeed be dangerous. As a result, PVQ is always presented and discussed in conjunction with the other ingredients on the WM maps, providing clear evidence that we do not ''endorse inattention to potentially significant information in the individual diagnostic fields'' as asserted by SCD (section 3c).

c. Temperature and efficiency

In WM, our assessment of precipitation efficiency is based upon ''guidelines determined by statistical studies'' (WM, p. 160). SCD agree in general with the validity of these guidelines but point out that they are not able to explain every detail of every case.[4] SCD present a review of additional precipitation efficiency and microphysics concepts. Their review makes clear that research in cloud microphysics is still grappling with some fundamental questions pertaining to temperatures characterizing the ice-nucleation threshold and maximum depositional growth rate. Therefore any diagnostic of precipitation efficiency cannot, at present, comprehensively describe nature. We encourage them (and others) to translate this review into operationally useful tools that can be incorporated into the IM as diagnostics of the temperature and efficiency ingredients. Given the many physical factors involved in determining the phase of winter precipitation, we wish to reiterate the caution expressed in WM that ''an accurate forecast of precipitation type ultimately requires the careful monitoring of observed temperature and moisture profiles as the storm develops'' (WM, p. 160).

d. Use of traditional snowfall prediction techniques

The IM introduced in WM is offered as a step toward the replacement of the so-called traditional techniques. Our Table 1, far from suggesting that these techniques are ''incomplete demonstrations of ingredients-based methodologies'' (SCD, section 5), simply demonstrates that they make very little reference to what we consider the ingredients necessary to produce precipitation.

In an operational environment, forecasts for winter season precipitation require that the snowfall accumulation be quantified, a function not provided by the IM. Currently, operational forecasters at NWS WFOs base their estimates primarily on traditional empirical techniques. Our discussion of the traditional techniques in the context of the IM is motivated by the need to develop an approach that simultaneously focuses attention on physical processes while providing forecasters with some quantitative guidelines for snowfall prediction. When a physically based, operational approach for forecasting snow amounts becomes available, the physically deficient traditional techniques will become obsolete. Development of the IM in WM is one step in such a process.

3. Final comments

The goal of WM was to present an operational, numerical model–based method that is useful in the forecast process and serves as a first step toward the development of an approach that would replace traditional empirical methods. We employed an ingredients-based methodology in this pursuit. An important distinction exists between the *operational* IM developed in WM and the notion of an ingredients-based methodology described by SCD. SCD maintain that the value of an ingredients-based methodology arises largely from the fact that it provides ''focus on only those aspects of a situation that are unambiguously relevant'' (SCD, section 2a), not from the aid it provides in forecasting the

[4] For instance, SCD find fault with the suggestion of $-10°C$ as an operational cutoff for ice in clouds stating that, ''questions exist about its potential usefulness for every situation'' (SCD, section 4a, p. 10). They then present evidence that it is valid 93.8% of the time.

weather. Our intention in WM was to vest this important intellectual exercise with direct operational utility.

The IM was introduced as a systematic, physically based operational methodology designed to help forecasters focus on the evolution and interaction of the fundamental elements and processes underlying the production of winter season precipitation. We believe that our methodology and choice of ingredients are robust and that some extension of the IM may eventually offer a welcome replacement for traditional snowfall prediction techniques. Additionally, by emphasizing the primacy of ingredients over diagnostics, the design of the IM allows for considerable flexibility in the choice of diagnostic parameters. Many different sets of diagnostics can be used, without compromising the utility of the IM, so long as those diagnostics serve to assess the presence and strength of the ingredients. We invite extensions and improvements to the diagnostics employed for each ingredient, recognizing that any choice comes with limitations and that any one set of diagnostics will not be suitable for all forecasters in all regions. To date, several NWS WFOs have taken our ingredients scripts[5] and tailored them to their own needs. We encourage other users to do the same. Only such a wide operational test can ultimately determine the utility of the IM for forecasting winter season precipitation.

[5] These GEMPAK/UNIX/AWIPS scripts are available freely at the following World Wide Web address: http://speedy.meteor.wisc.edu/~swetzel/winter.

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