Abstract

The Eighth Cyclone Workshop was held at the Far Hills Inn and Conference Center in Val Morin, Quebec, Canada, 12–16 October 1992. The workshop was arranged around several scientific themes of current research interest. The most widely debated theme was the applicability of "potential vorticity thinking" to theoretical, observational, and numerical studies of the life cycle of cyclones and the interaction of these cyclones with their environment on all spatial and temporal scales. A combination of invited and contributed talks, with preference given to younger scientists, made up the workshop.

1. Workshop background

Working scientists and students interested in cyclone-related research problems have used the venue of periodic cyclone workshops to exchange ideas and information from observational, theoretical, and numerical studies of cyclogenesis and the life cycle of cyclones. This article reports on the most recent cyclone workshop gathering, held at the Far Hills Inn and Conference Center in Val Morin, Quebec, Canada, during 12–16 October 1992.

The idea to conduct cyclone workshops was first explored during the American Meteorological Society’s (AMS) Weather Analysis and Forecasting Conference in the Washington, D.C., area in October 1978. Donald Johnson chaired a preliminary planning meeting (attending were Lance Bosart, John Cahir, John Hovemare, Carl Kreitzberg, Chester Newton, Norman Phillips, Frederick Sanders, Phillip Smith, Ronald Taylor, Dayton Vincent, and Johnson) in which it was generally agreed that a focused research effort on the extratropical cyclone should be initiated and carried out, and the findings debated at periodic scientific workshops. Johnson agreed to serve as the chair of the informal Extratropical Cyclone Project Steering Committee (other steering committee members included David Baumhefner, Bosart, Hovemare, Smith, Taylor, and Vincent), which would arrange and organize the workshops.

Table 1 lists the eight cyclone workshops that have been held to date. The first meeting was held at the National Meteorological Center (NMC) during 30 May–1 June 1979. Subsequent meetings alternated between a location at or near NMC (to encourage participation by operational meteorologists and modelers) and elsewhere. After the seventh gathering, at the Drexel Lodge in Newton Square, Pennsylvania, in October 1988, a brief hiatus occurred before the most recent workshop, in October 1992. Smith took the lead in rekindling interest in the cyclone workshop by chairing an informal gathering of interested participants at the AMS Annual Meeting in Atlanta, Georgia, in January 1992. A new steering committee was formed (members were Bosart, Daniel Keyser, Johnson, Patricia Pauley, Donald Perkey, Melvyn Shapiro, and Smith) to oversee the effort. Bosart was "elected" (by virtue of being absent from the discussion—he was elsewhere in the hotel attending another meeting) as workshop coordinator and convenor of the Eighth Cyclone Workshop, held during October 1992. Preference was given to younger scientists and graduate students in the scheduled talks, and younger scientists were also invited to give some of the theme talks. A principal driving force behind the workshop was a desire by
participants to debate the value of “potential vorticity (PV) thinking.” Sanders challenged the group to show how much more has been learned about the structure and behavior of cyclones through “PV thinking” than through the established “omega equation thinking” or “Sutcliffe development thinking.” The challenge (bait?) was taken up eagerly by other scientists.

The workshop review appears in session order. Two authors are listed for each session: the session chair first and the session note taker second.

### 2. Workshop overview

**a. “PV thinking”—Daniel Keyser and Gary Lackmann**

This session began with a presentation by Rainer Bleck entitled “Who Needs Potential Vorticity?” This question rapidly crystallized into one of the main themes of the workshop. Bleck’s hypothesis is that PV can be used to remove misconceptions and to replace needlessly complicated dynamical theories with simpler ones. As an example of a (common) misconception, he showed a newspaper weather map that explained weather events as being caused by high or low atmospheric pressure. An example of a needlessly complicated dynamical theory that still does not penetrate to the heart of the matter, in his opinion, is the one dealing with secondary circulations associated with the deformation of the mass field near jet streaks.

Bleck proceeded to point out links between jet streaks, Petterssen type B cyclogenesis, the Eady model, and PV thinking. As a means of depicting the linkage between PV and cyclogenesis graphically, Bleck presented three-dimensional wire-mesh plots illustrating the phase relationship between upper- and lower-tropospheric PV perturbations during extratropical cyclogenesis. Bleck stated that representation in isentropic space allows depiction of surface-trapped PV anomalies in terms of absolute vorticity spikes protruding upward from the surface (stalagmites) and of stratospheric anomalies extending downward from the tropopause (stalactites). He expressed hope that his method of combining the two classical ingredients of extratropical cyclogenesis in a single picture will aid the understanding of baroclinic instability and will allow the operational and theoretical communities to describe this process in mutually understandable terms.

Baroclinic instability in the Eady model is characterized by zero interior PV gradients and is driven entirely by boundary temperature gradients. Consistent with this behavior, plots showing the evolution of an Eady mode depicted isentropic absolute vorticity stalagmites and stalactites growing into the interior of the domain.

Finally, Bleck speculated on the causes of our inability to predict intensity changes in hurricanes. He surmises that diabatic heating in the ascending air leads to an accumulation of anticyclonic PV in the upper part of a hurricane. Unless the surrounding atmosphere has similar PV values, this air will be dynamically constrained from flowing out to the sides, and the hurricane weakens “under its own weight.” Bleck asserts that successful prediction of hurricane intensification will not be possible until PV can be measured reliably both in the upper part of the vortex and in the surrounding environment. Presently, no prospects exist for obtaining the required data.

Next, Christopher Davis discussed various aspects regarding the application of PV thinking to the real atmosphere. A broad topic was that of “direct” versus “indirect” effects. As an example, Davis considered an interior diabatic heat source. A direct effect is the diabatic alteration of PV due to the heating; an indirect effect is the alteration of the background PV field through advection by the flow induced by these diabatically generated anomalies. Another significant topic dealt with inversion of individual PV anomalies. Three possible ways to define PV anomalies are in terms of 1) a departure from a spatial or a temporal mean, 2) the difference between two instantaneous states ascertained, for example, from parallel model integrations in which particular physical processes are included or excluded (i.e., the sensitivity approach), and 3) changes in PV due to advection and non-conservative processes determined through explicit integration of PV conservation equations. In order to isolate net differences in PV due to diabatic processes, an adiabatic mesoscale model simulation was contrasted with a full-physics run. An explicit PV integration was used in conjunction with the full-physics run to isolate the portion of the total PV difference due to direct diabatic PV alteration. The full-physics run exhibit-
ited reduced PV aloft near the cyclone (in the 400–200-mb layer) and increased PV at lower altitudes (in the 900–450-mb layer) relative to the adiabatic run. The difference in PV between the two runs was largely due to indirect effects: deformation of the tropopause by latent heat–enhanced secondary circulations played an important role in the moist run. Davis noted that advective (indirect) effects are likely to be larger near the tropopause because of the large background gradient of PV found there.

Jeffrey Whitaker described an application of PV thinking to simple linear and nonlinear models of moist baroclinic instability. The simulations involved a two-dimensional Eady model in which all rising (sinking) air is assumed to be saturated (unsaturated). It was emphasized that in theoretical models of the interaction between baroclinic development and latent heat release, it is necessary to couple PV thinking with interpretations constituting the Sutcliffe–Petterssen view of cyclogenesis (in which the \( \omega \) equation plays a central role), because of the direct link between latent heat release and rising motion. The results of Whitaker’s linear simulation are consistent with those of Davis: a vertically oriented couplet of low high PV in the vicinity of the region of latent heat release. A piecewise inversion, which partitions between boundary and interior PV anomalies, showed that including latent heating effects modifies the boundary edge waves through flow induced by the interior anomalies. Vertical motions are shown to reinforce internal PV generation. The nonlinear simulation exhibited much larger growth rates, along with stronger fronts and updrafts, for the moist case relative to the dry case. Whitaker found that the lower thermal wave was not amplified by the positive interior PV anomaly, although the warm-air seclusion was aided by interior PV advection. An additional feedback was due to the advection of warmer air with lower moist static stability into the frontal-updraft region by the flow induced by the interior anomalies. These internal feedbacks between the frontal PV anomaly and the ageostrophic frontal circulation are responsible for a more intense cyclone, yet do not alter the larger-scale baroclinic wave.

William Blumen showed time sections of sharp fronts exhibiting eddies in the prefrontal or postfrontal cold air or both. He proposed representing frontal features through the wavelet transform. Some questions regarding small-scale frontal dynamics are: 1) Do there exist elementary coherent structures associated with frontal turbulence? 2) Is there a universal character to frontal/turbulence interactions? 3) Can we compute flow evolution in fronts with a reduced number of degrees of freedom using the wavelet transform?

Mark Stoelinga pointed out that there are two main approaches in using PV diagnostically to describe and interpret cyclogenesis. One is to exploit PV conservation; the other is to exploit PV nonconservation. For the latter, one must somehow deduce the amount of PV change following an air parcel and arrive at an invertibility relation. Stoelinga partitioned PV into advected, diabatically generated, and frictionally generated components. The “Scamp” storm of February 1987 was used as an example. Since PV generation by latent heat release had already been discussed, Stoelinga focused on frictional effects. To isolate these effects, explicit PV integrations in model simulations without surface energy fluxes were compared to model simulations in which there were neither surface energy nor momentum fluxes. Results showed that the average PV in the vicinity of the storm increased due to friction. The run without friction did, however, produce a deeper storm, the reason being stronger latent heat release in that case. Also, the surface potential temperature anomaly was stronger in the frictionless case because of stronger advecting low-level winds. Thus, indirect effects (stronger latent heating) outweighed the direct effect of increased PV locally.

Because of differences in the altitude of atmospheric jet features, Shapiro pointed out that one must exercise caution in selecting isentropic surfaces on which to display PV. To represent all important features on one chart, Shapiro plotted the vertically integrated tropospheric PV. The ERICA (Experiment on Rapidly Intensifying Cyclones over the Atlantic) IOP-4 case was used as an illustrative example. Plots of vertically integrated PV and ozone are well correlated and indicate that a deep precursor fold in the tropopause existed upstream of the incipient cyclogenesis region. Displays of PV in adiabatic versus diabatic (moist) model runs for IOP-4 were contrasted: the diabatic run showed a pronounced “low PV notch” north of the cyclone. This low PV region evidently resulted from both direct and indirect effects of diabatic heating and was shown to be very shallow—confined to the vicinity of the tropopause. Shapiro illustrated how, through the invertibility principle, a mesoscale PV charge can influence a synoptic-scale region of a size given by the Rossby radius of deformation.

b. Cyclogenesis as viewed from the PV framework—

Frederick Sanders and Michael Dickinson

Sanders introduced the session by noting the large amount of attention given recently to application of PV to the problem of cyclone development. He observed that as a result we have learned much about the structure and behavior of PV, but reminded participants that the goal is to find out more about the cyclone itself.

The first speaker was John Nielsen, whose presentation was entitled “The Elements of Cyclogenesis.” He contrasted the view of observationalists, who see cy-
clogenesis as the result of nonlinear interactions between entities such as upper-level troughs and ridges, jet streaks, and low-level fronts and jets, with the traditional theoretical view of cyclogenesis as the growth of normal-mode perturbations on an unstable basic state. The question arises whether observed cyclones arise from baroclinic instability in the latter sense or baroclinic development in the former, interactive, sense. The approach used was to examine perturbation PV and associated geopotential height anomalies. When two PV perturbations and associated geopotential height anomalies are sufficiently close together, the combined perturbation energy is larger than when they are farther apart, owing to the contribution provided by the product of the geopotential height perturbation of each with the PV perturbation of the other. The total perturbation energy may grow simply from the shortening of the distance between two such centers. This is a simple example of the superposition effect present in either barotropic or baroclinic atmospheres, as noted in recent papers by Brian Farrell. In traditional normal-mode instability, superposition effects are absent since the phase separation between upper and lower disturbances is fixed and energy increases because PV anomalies amplify.

In the observed atmosphere, the strongest gradients of PV are found along the tropopause where PV increases poleward and along the bottom boundary where potential temperature (surrogate PV) increases equatorward. Through the interior troposphere there are no comparable gradients of PV except where produced locally by latent heat release during cyclogenesis. These are the elements of the PV field. The elements of cyclogenesis are then 1) the superposition effect, 2) remotely induced amplification wherein the flow associated with each perturbation serves to increase the intensity of the other (sometimes referred to as baroclinic instability even when unstable normal modes are not present), 3) generation of PV by latent heat release, 4) topographic effects, 5) Rossby wave propagation, and 6) the reduction of the Rossby radius of deformation. Of these, 1 and 2 were found to be ubiquitous and perhaps dominant; 3, a result of cyclogenesis rather than a cause; and 4 through 6, of lesser, although not vanishingly small, importance.

In the discussion following Nielsen's presentation, questions were asked about the interactions between jet streaks and cyclogenesis, which seemed to be missing from the proposed elements of cyclogenesis. Nielsen replied that such features may be viewed as part of the upper-level PV anomaly. There were queries about the influence of propagation of energy from upstream and about origin of the upper-level PV anomalies if they were not the product of normal-mode development. Finally, it was not clear that this approach yielded information on cyclone behavior that could not be obtained from traditional quasigeostrophic explanation, although the simplicity and elegance of the PV approach were not questioned.

In the next presentation, Michael Morgan spoke on "The Motivation and Interpretation of Analyses of Tropopause Potential Temperature and Lower-Tropospheric Equivalent Potential Temperature." The motivations for the study include conservation of PV following the motion on isentropic surfaces and the invertibility principle relating the wind field to the mass field through some appropriate balance condition. He presented a sample analysis of PV on an isentropic surface, showing a concentration of PV gradient in the vicinity of the jet stream, with decidedly weak gradients on the equatorward side and relatively modest gradients on the poleward side. A composite vertical cross section of PV and potential temperature confirmed the strong PV gradient in the tropopause region near the jet, serving as a guide for Rossby waves. It was noted that the development of surface inversions due to nocturnal cooling often produces large values of PV of doubtful dynamical significance (increased PV just above the surface is compensated by a local minimum of potential temperature at the surface).

A compact representation consisted of plotting maps of the potential temperature on the surface at which the PV value is 1.5 PV units, indicative of the tropopause. With this type of chart, a region of blocking over the North Atlantic was easily seen. A tropopause fold (multiple values of potential temperature for the tropopause PV value) was apparent as was a hurricane outflow region. Interior (tropospheric) PV anomalies may not be represented on the tropopause map. An exception might be the negative PV anomaly found on the tropopause above a region of concentrated latent heat release such as can occur with a major convective outbreak. In such cases a positive interior PV anomaly would be seen in the middle troposphere below the negative PV anomaly on the tropopause.

In discussion it was suggested that the addition of wind data to the analysis on the 1.5 PV unit surface would enable evaluation of thermal advection at the tropopause. In response to a question about what could be seen on PV charts that would be invisible on conventional constant pressure charts, the tropospheric region of high PV concentration due to latent heat release was mentioned. Finally, it was noted that monthly mean charts or cross sections of PV would not show the strong gradient seen on a given map associated with the jet/tropopause region because of vertical and horizontal migration of this feature from day to day.

The final presentation, entitled "Evolution of Potential Vorticity Anomalies during Explosive Maritime Cyclogenesis," was made by Lackmann (with Keyser and
The maximum deepening rate for the selected storms, 4 mb per 6 h, was considered to be the time when the deepening first reached 4 mb per 6 h. The composite analysis revealed that at 500 mb at and prior to onset time, on the planetary scale, there was a broad region of negative geopotential height anomaly across the central Pacific, with positive anomalies on the west coast of North America. This closely resembled the positive phase of the PNA (Pacific/North American) teleconnection. Further east the height anomaly was generally negative over the western North Atlantic, with some evidence of a precursor disturbance propagating northeastward over the North Atlantic, leaving a baroclinic zone in its wake. At onset time there was already a well-developed large-amplitude ridge, which had developed just prior to the intensification of the surface cyclone, could be seen downstream.

Detailed analyses were presented for the ERICA IOP-2 storm from 1200 UTC 13 December 1988 at 12-h intervals through 0000 UTC 15 December 1988. For each time, individual map panels showed potential temperature and pressure on the tropopause (1.5 PV unit surface), as well as the lower-tropospheric Ertel PV and the 850-mb equivalent potential temperature and geopotential height. The development of the surface storm was associated with the southeastward movement of a cold anomaly (potential temperature below 300 K) on the tropopause (pressures near 500 mb) toward the east coast of North America and offshore. Latent heat release associated with the developing storm also resulted in the creation of an interior PV anomaly along the warm-front boundary.

There was a question concerning the involvement of the subtropical jet, to which the answer was that it may have participated in the development of a precursor disturbance. In reply to a query whether the Pacific anomaly pattern may have reflected the presence of El Niño years, it was asserted that the same PNA positive pattern was still present in the data when the El Niño years were excluded from the sample. The PNA positive pattern, moreover, was seen as early as 60 h prior to onset time. The existence of a precursor disturbance suggests that the cyclogenesis process can respond to multiple "hits" from disturbances propagating along the tropopause. Alternatively, the significance of the precursor, in addition to leaving a baroclinic zone for later development in its wake, may be to bring cold air over relatively warm water, reducing the static stability and promoting the flux of heat and moisture from the sea into the air mass involved in the subsequent cyclogenesis.

The theme for this session was a departure from the PV thinking concepts that dominated sessions 1 and 2. It provided an opportunity for expression of alternative and complementary views of cyclone development. Peter Zwack (with Judy St. James) opened the session with work addressing how diabatic heating changes the PV structure and the balanced flow response for given static stability profiles. The response was diagnosed by solving a Sutcliffe-type development equation, which required computation of a vertical velocity. A heating maximum of 5 K day⁻¹ in the 400-600-mb layer of a barotropic atmosphere produced a vertical circulation with a maximum in upward motion in the vicinity of the applied heating. The effect of the upward motion depended on the static stability profile, since adiabatic cooling due to upward motion can offset the imposed heating. For all static stability profiles, Zwack and St. James found that the vertical velocity maximum was strongest for heating at upper levels, whereas the greatest surface development was found for heating at low levels.

In the spirit of the theme of this session, Paul Hirschberg questioned whether it was wise to abandon viewing physical processes through the advection of state variables such as temperature. He reviewed the works of Sutcliffe and Petterssen, who envisioned development in terms of cyclonic vorticity advection at the level of nondivergence (LND) and warm-air advection overlapped. This result is consistent with the requirement of net warming of the air column over the deepening cyclone. Development ceased when the surface cyclone was situated directly below the lower-stratospheric warm pool.

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Juan Carlos Jusem (with Robert Atlas and John Manobianco) applied the pressure tendency equation of Margules (1904) and Bjerknes (1937) to model simulations of ERICA IOP-4 and a cyclone over Argentina. This investigation was motivated by a review of the history of the use of and approximations to this equation. They showed that erroneous use of this equation by investigators in the 1940s and 1950s, as well as insufficient data, contributed to the near absence of further applications. Data provided by contemporary numerical model simulations, however, permit accurate evaluation of all terms in the tendency equation. For the two cyclone cases studied, they found that the pressure tendency minimum was associated with the minimum in vertically integrated density advection. In addition, results of a quasi-adiabatic simulation of ERICA IOP-4 showed that the so-called “joint contribution” of divergence and vertical motion terms offsets the pressure tendency associated with density advection. This is analogous to the cancellation effects between adiabatic cooling due to upward motion and horizontal warm-air advection discussed in the previous two talks. In the full-physics run, the joint contribution term was in the same sense as the pressure tendency. It was emphasized that this view of development does not require the specification of a balance condition, as is the case with most other approaches.

d. Frontal analysis/conceptual model and theoretical issues—Bosart and Eric Hoffman

The presentations in this session dealt with three principal themes: 1) reconciliation of observed cyclone structures with existing conceptual cyclone models, 2) surface analysis techniques for the representation of synoptic and mesoscale frontal structures, and 3) the application of new theoretical ideas to the study of cyclogenesis. Animated discussions were a staple of this session.

Clifford Mass opened the session by challenging current operational surface analysis practices. He argued that traditional surface analysis techniques are flawed because they are based on an overly simplified and outdated version of the Norwegian Cyclone Model (NCM). According to Mass, deficiencies in the underlying conceptual model of cyclone development have been compounded by a lack of consistent and well-defined procedures for defining fronts and for analyzing surface synoptic charts. He presented several examples of potentially confusing surface analyses and concluded with suggestions for different ways that surface analyses could be improved, especially if the operational and research communities would work together to experiment with different possibilities. A very spirited discussion ensued after the completion of this talk. The next talk, by David Olson, reviewed past, present, and future directions of surface analysis techniques at NMC. He emphasized the importance of analysis philosophy and analyst training skills to the successful representation of synoptic-scale and frontal-scale weather features on daily weather maps. Olson concluded his talk with a report on initial efforts to move production of the North American surface analysis to interactive computer workstations, a step already taken successfully for the Daily Weather Map series.

Sanders reported on a detailed case study of the evolution of the surface potential temperature and mean sea level pressure fields that departed significantly from the NCM in association with a March 1991 cyclone passage across the southwestern United States. Initially, a modest surge of rising pressure was seen in California, unaccompanied by any significant temperature variation. This pressure surge raced eastward during the daytime, intensified, and became associated with a significant temperature drop and pronounced wind shift. As this cold front advanced into western Texas, first the pressure surge began to move ahead of the temperature break, and then severe convection developed. The pressure surge itself accelerated eastward to the southeastern states as a ducted gravity wave, outrunning the convection. According to Sanders, "Analysts are urged to confront rather than deny this affront to traditional frontology." Considerable debate followed as to how to represent synoptic and mesoscale weather features on surface weather maps and whether mesoscale analyses should be performed separately. No consensus was reached.

The application and validity of conceptual models to a variety of cyclogenesis and frontogenesis case studies was explored in a trilogy of papers by David Schultz (with Mass), Jonathan Martin, and James Steenburgh (with Mass). Schultz described the evolution of the occluded front in the 15 December 1987 midwestern United States cyclone using a simulation provided by the Penn State/NCAR MM4 model. The occluded front formed as the cold front rotated around the cyclone and merged with the warm front. The resulting warm-type occlusion formed through the merger of the upper-level and lower-level frontal zones, not by the "catch-up" of the cold front with the warm front. Schultz demonstrated that this warm-type occlusion structure formed despite a low-level thermal structure that favored a cold-type occlusion. Martin examined cases of cold fronts aloft drawn from the Genesis of Atlantic Lows Experiment (GALE) and Storm-Scale Operational and Research Meteorology–Fronts Experiment Systems Test (STORM-FEST) field programs. The cases featured eastward-moving squall lines in the lower Mississippi River valley that coincided with baroclinic zones in the middle troposphere. A crucial finding was that the deep vertical circulation associated with the squall line ap-
peared to be initiated by frontogenesis processes aloft, approximately in accord with the conceptual model of cold fronts aloft presented by Martin et al. (1990).

Steenburgh followed with a Penn State/NCAR MM4 model study of cyclone evolution over and to the lee of the Rocky Mountains taken from the 6–7 March 1986 GALE case. In this case, a warm-type occlusion resulted in the lee of the Rockies in response to the arrival of an eastward-sloping cold-air surge aloft (old Pacific air mass) over a low-level lee trough. The resulting cold front aloft marked the back edge of the primary cloud and precipitation shield and illustrated the importance of frontogenetical processes in the middle troposphere in creating a nonclassical frontal evolution. The resulting spirited discussion centered on three issues: 1) applicability of the NCM to modern weather analysis and diagnosis, 2) whether such features as cold fronts aloft should be represented on surface maps, and, if so, in what fashion, and 3) whether conceptual models are useful in diagnosing and predicting cyclones and the weather associated with them. Shapiro pointed out that the European originators of the NCM included provisions for a rich horizontal and vertical structure that was omitted in the overly simplified version of the NCM that took hold in North America. The consensus of the group was that upper-level features should not be represented on operational surface weather maps. As to the validity and utility of conceptual cyclone and frontal models, the assembled gathering agreed to disagree.

The last two papers in the session, by Isidoro Orlanski and M. Mackay (with Kent Moore), dealt with theoretical issues related to cyclogenesis and upper-level development. Orlanski presented a study of progressive downstream development in middle latitudes of the Southern Hemisphere based on European Centre for Medium-Range Weather Forecasts (ECMWF) initialized analyses. His results pointed to the importance of a possible relationship between eddy kinetic energy fluxes in jet stream regions and successive downstream surface development. The timing of the downstream development was in approximate accord with expectations for eddy kinetic energy propagation using simple group velocity arguments. A possible modifying influence of the Andes Mountains on downstream development was also seen. Mackay used a two-layer linear model of baroclinic instability to test the hypothesis that baroclinic waves interact with the zonally averaged flow in such a way as to eliminate the baroclinicity and the source of the instability. Heat fluxes of the fastest growing normal mode were used to calculate the Eulerian mean response to the growing wave, and tendencies in zonally averaged variables were integrated in time to estimate the net adjustment due to wave–mean flow interaction. It was found that when vertical heat fluxes were included, the lower troposphere stabilized after a few days but the upper troposphere destabilized. A stability analysis on the adjusted atmosphere revealed a new interior mode of baroclinic instability with a shorter wavelength than the primary wave. Discussion of these two papers focused on the specifics of the various modeling assumptions and the general applicability of these assumptions to the behavior of the real atmosphere.

e. "Beyond quasigeostrophic (QG) thinking"—Bleck and Jeffrey Chapman

The session, "Beyond QG Thinking," dealt with issues that cannot be addressed properly by the quasigeostrophic approximation. Christopher Snyder discussed the utility of balanced equations (equations retaining certain terms neglected in conventional first-order Rossby number expansions) from both diagnostic and prognostic perspectives. He led the audience through the hierarchy of standard QG, semigeostrophic (SG), and balance equation (BE) systems and emphasized that solving the latter involves iterations between the vorticity and divergence equations.

Balanced systems closely approximate the primitive equations for near-geostrophic and nontdivergent flow, exclude gravity waves, and possess reduced dimensionality (fewer equations required to close the system) as well as analogs to PV conservation. Snyder related that balanced systems have advantages as dynamical models through their conceptual simplicity, almost trivial initialization, and potential computational economy; as diagnostic tools they are attractive for determining indirectly measured fields and isolating gravity wave signals or noise.

Accuracy in various parameter regimes was addressed through rigorous scale analysis. Three regimes were identified: the familiar synoptic-scale regime with Ro << 1, a strongly stratified regime with Ro ≳ O(1) and F ≳ 1, and a frontal regime in which scales are anisotropic and Ro ≳ O(1). (Here Ro and F are Rossby and Froude numbers, respectively.) Error analysis revealed that the QG and SG equations are applicable on the synoptic scale, the SG equations near fronts, and the BE in all three regimes. Neglected in this idealized analysis were the impact of diabatic processes on balance and mathematical questions such as existence and uniqueness of solutions. Answers are still sought regarding the role of gravity waves, inclusion of a planetary boundary layer (PBL) in SG and BE models, and an explanation of the extended applicability (i.e., at relatively large Rossby numbers) of balanced systems.

A second talk presented work in progress by Bluestein (with Doug Speheger) on the analysis opportunities provided by the use of a hierarchy of diagnostic models. As an example, these authors propose to use an SG system in their diagnostic work combining data from
vertical profilers, the Oklahoma mesonet, Doppler radars, and numerical models. The relatively high temporal resolution of wind observations within the profiler network allows solution of the SG equations for the ageostrophic winds. The system may break down in regions of highly curved flow, but one can determine from profiler observations if and where this is likely to occur. One important issue is the filtering of the data to ageostrophic winds. The system may break down in vertical profilers, the Oklahoma mesonet, Doppler ra-
friction, recovered as a residual. Surface winds are as the difference of the total wind and the ageostrophic component. The accuracy of this method will be tested by applying it to numerical model output where all components of the flow are known. Analysis at the surface will require determining the contribution of friction, recovered as a residual. Surface winds are being determined from the NCAR Portable Automated Mesonetwork (PAM) system. Future activity will be directed toward incorporating mass continuity and thermodynamic constraints, as well as dual-Doppler data, into the analysis scheme.

f. Cyclone predictability issues—Zwack and Kevin Tyle

During this session, three presenters discussed aspects of cyclone predictability, with applications to both short- and extended-range forecasting. Richard Grumm commenced the session with an examination of the aviation run (AVN) of the NMC Global Spectral Model. The model’s skill in detecting cyclones, in reproducing the deepening rate and location of the storms, and in predicting rapidly deepening cyclones was evaluated from 1989 to 1992. The NMC Nested Grid Model (NGM) and the Enhanced Terrain (ETA) Model were also studied. During the winter season, the AVN was best at detecting cyclones. However, the AVN’s false alarm rate increased with the forecast period. The AVN’s 12-h predicted central pressure was higher than reality; however, the error lessened from 1991 to 1992. Nevertheless, the forecast thicknesses were too low, illustrating the model’s cold bias. The distance error of the AVN was less than the NGM’s at 24 h; by 48 h the difference between the two models was negligible. The 24-h forecast pressure errors of the AVN and NGM have been comparable since the AVN went to T126 resolution in March 1991. For summer 1992, the AVN was best in mean distance error, with the NGM next, and the ETA last. Given the relatively poor performance of the ETA model to date, Bosart questioned whether NMC should gradually shift more of its resources to global models instead of regional models. Michael Baldwin suggested that the problem may be due to the early data dump of the ETA as compared with the NGM and AVN. Louis Uccellini pointed out that although the AVN’s performance may be the best of the three models, it gets out too late for the afternoon forecast. Once the AVN goes to four cycles per day, more forecasters will use it.

The next paper, by Paul Roebber (with John Gyakum and Diep Trat), described model performance with regard to mesoscale precipitation structure. Roebber presented the case of 13–14 December 1998, which involved a coastal front over New England in association with an anticyclone to the north and a developing low to the south. An easterly low-level flow developed. The resulting geostrophically driven frontogenesis contributed to the outbreak of precipitation over New England. Roebber presented the forecasts generated by the Canadian Regional Finite Element (RFE) Model. The model correctly predicted the frontal signature, but its forecast cross-frontal temperature gradient was much stronger than reality, and its positioning of the front was too far offshore. Additionally, the model lagged the onshore movement of the front and inaccurately depicted the front’s orientation. The model failed to take into account boundary-layer effects, which, in reality, resulted in warming on the cold side of the front. As a result, although the RFE did a capable job in forecasting the synoptic-scale precipitation pattern, the mesoscale aspects of the precipitation structure were not well handled. It was suggested that improved vertical resolution might have resulted in a better forecast. Zwack stated that although current models make excellent forecasts of the mass field, they frequently fail to resolve important mesoscale features related to observed weather patterns.

M. Steven Tracton concluded the session with a discussion of ensemble forecasting, the “wave of the future” for extended-range (>5 day) weather prediction. This technique involves the generation of an array of forecasts resulting from uncertainties in the model’s initial conditions. Ensemble forecasting is based on probabilistic as opposed to deterministic principles, and each ensemble forecast consists of a multiple number of dynamical model runs starting from slightly different initial conditions. The goal is to develop a more objective means of ensemble forecasting. Beginning in December 1992, the NMC Medium Range Forecast (MRF) Model has been used to produce ensemble forecasts. Up to day six, the model uses T126 resolution; beyond that, T62 resolution is employed. Tracton presented the case of 25 December 1998 as an example to illustrate the nature of the divergence of the various forecasts. Examination of the standard deviations about the ensemble mean indicates where some of the uncertainties lie. After similar forecasts are clustered together, one can assess the probability of a given event occurring. There is a strong correlation between the number of individual forecasts that predict a given event and the likelihood of the event’s actual occurrence. Addition-
ally, the skill of the T62 ensemble forecast is consistently better than that of the single T126 forecast. Steve Mullen briefly discussed the variability (in terms of speed of cyclone movement and position) of the 48-h NGM. Although the systematic error was small, the variability of the forecasts about the model systematic error at 48 h was large. Ensembling reduced the variability: with eight ensembles, displacement error was similar to the baseline systematic error.

g. Synoptic and mesoscale vortices—Toby Carlson and Haig Iskenderian

Several specific cases of cyclogenesis were discussed in the first half of the session. Gregory Hakim (with Bosart and Keyser) documented an example of a spectacular cyclogenesis event over the upper Ohio Valley on 25–26 January 1978. The noteworthy feature of the event was the amalgamation of two distinct vorticity maxima in the northern and southern branches of the westerlies coincident with the explosive cyclogenesis. Hakim showed that the trough merger occurred in conjunction with confluent flow involving a northern polar jet and a southern subtropical jet, which brought into close proximity two potent upper-tropospheric PV maxima. As the two upper-level maxima merged in the horizontal and coupled with a preexistent low-level PV maximum, rapid surface cyclogenesis ensued, with pressure falls of 41 mb in 24 h, a rate rarely achieved over land. Another case of remarkable continental cyclogenesis and a severe weather outbreak, that of 28–29 March 1984, was described in two numerical model–aided diagnostic studies by Michael Kaplan (with Steven Businger) and Gyakum (with Ying-Hwa Kuo and Zitian Guo). In the early stages of the storm, a focused area of rain was present over Alabama. The resulting concentrated region of latent heat release helped to generate a lower-tropospheric PV maximum. This low-level feature, combined with an upper-level PV maximum, led to cyclogenesis. Later in the storm’s evolution, convection over North Carolina, combined with a strong jet in the upper troposphere, produced a favored area of horizontal divergence aloft, resulting in rapid pressure falls at the surface.

Two examples of smaller-scale cyclonic vortices were then discussed. F. Martin Ralph (with Paul Neiman) described an interesting case of double-vortex structure over the central United States on 5 March 1992. A small-scale secondary cyclone formed along the cold front of a preexistent cyclone. This secondary vortex moved northward and strengthened, while the primary tracked eastward and decayed. With the aid of solar heating during the next day, convection formed in association with the decaying primary cyclone. A simulation of meso-β cyclonic vortices along a coastal front off the Carolinas was discussed by James Doyle (with Thomas Warner). The Penn State/NCAR nonhydrostatic model (MM5) with 5-km horizontal resolution successfully simulated the precipitation pattern and wind structure of the vortices when compared with the available surface wind and dual-Doppler data. The vortices, on average, possessed a 1-mb pressure perturbation at the surface, with convergence concentrated below 850 mb. Undulations formed along the coastal front, leading to convergence and eventually to convection, with the generation of environmental vorticity by the mechanism of vortex-tube stretching. Surface sensible heat fluxes appeared to be crucial to the maintenance of the coastal front, while latent heating and nonhydrostatic effects were of secondary importance.

Finally, Chun-Chieh Wu (with Kerry Emanuel) presented a method to determine the steering flow of a hurricane using the invertibility principle of PV. Potential vorticity fields were calculated using gridded NMC 2.5° × 2.5° latitude–longitude upper-air analysis fields. A storm perturbation PV was defined relative to a seasonal mean PV field. A piecewise inversion of the PV fields was performed to yield the flow fields. The steering flow was defined as MEAN + U5 + L5E, where MEAN is the mean flow, U5 is the flow at 300 mb and above, and L5E is the flow diagnosed below 400 mb, excluding the storm perturbation PV. Encouraging results were obtained for two hurricane cases.

h. Oceanic observations, analysis, and cyclogenesis—Wendell Nuss and W. Edward Bracken

The first talk, by Russ Schneider, presented a comparison of the life cycles of 11 extratropical cyclones through the use of quasi-Lagrangian budget equations. The quasi-Lagrangian budgets were calculated over a 7.5° latitude radius cyclindrical volume around each storm, and results for the water vapor budget only were highlighted in this talk. The results for the 11-cyclone sample showed some significant and interesting differences between maritime storms and continental storms. In general, the eastern Pacific cyclones in the sample had three to four times as much initial water substance present as the continental cyclones. The moisture convergence into the cyclone budget volume was also greater for the eastern Pacific cyclones. Schneider made an important point about the role of storm motion and Eulerian transport of water into the storm volume. In some storms, it is the storm motion that contributes water to the storm through the boundary influx ahead of the storm. For other storms, the apparently strong Eulerian advecion of water vapor from the southwest is effectively shut off because of movement of the storm away from this moisture source (the relative winds on the south-
the accuracy of the water vapor analysis over the followed, several participants raised questions about the accuracy of the water vapor analysis over the ocean and the ability to do budget computations from these analyses. Schneider responded that the analyses are relatively accurate and did not feel that his results are compromised significantly by analysis errors.

Timothy Hewson presented a classification scheme for frontal waves and the results of a frontal wave climatology from the FRONTS 1992 project. The classification scheme categorizes a frontal wave by three factors: 1) whether warm- or cold-air advection dominates, 2) the type of system (high or low) in the positive cross-front direction, and 3) the type of system in the negative cross-front direction. This leads to up to nine different types of frontal waves, of which only five were observed with any frequency. These were cold-front troughs, cold-front waves, warm-front waves, col waves, and warm-sector waves. Cold-front troughs and cold-front waves were the most frequent types, and they tended to develop into cyclones with frequencies of 67% and 48%, respectively. Warm-front waves and warm-sector waves were less frequent and tended not to develop. The most interesting type of frontal wave seemed to be the col wave, which occurred rather frequently and developed 63% of the time and possibly greater as the sample included a few uncharacteristic events. The col waves were observed to develop almost always under a confluent trough. In the discussion that followed, Grumm reported that he had seen the behavior of col waves very frequently when he was doing his North Pacific climatological study. There was also discussion about whether diffuent or confluent troughs would be more favorable for development. Hewson explained that for Great Britain confluent troughs were favorable and agreed that for other regions diffuent troughs might be favored.

Joseph Sienkiewicz presented a description of the products produced by the NMC High Seas Warning and Forecast Program. The high seas program is responsible for weather forecasts, wave forecasts, and gale warnings for both the North Atlantic (to 40°W) and North Pacific (to 180°W) basins. Sienkiewicz presented examples of the types of products produced by NMC and highlighted the difficulty in forecasting parameters such as wave height when these parameters vary over short temporal and spatial scales. The wave forecasting problem was brought up in the discussion when several participants asked whether NMC used the navy or NMC model for wave forecasting. The consensus opinion was that the navy ocean wave model seemed to have more diverse operational uses than the NMC model, but that considerable work remained to improve all operational wave prediction models.

Five short talks were also presented. The first, by Stanley Benjamin, examined the correspondence of PV and dry air on the 6.7-μm water vapor imagery. The PV analyses, produced by the Mesoscale Analysis and Prediction System (MAPS) of NOAA's Forecast Systems Laboratory, showed generally good correspondence between PV maxima and water vapor dark bands during the formation stages of cutoff low development. During the cutoff stage, high PV air tended to coincide with moist regions on the imagery. In the discussion that followed, it was suggested that isentropic trajectories could be used to sharpen the relationship between the PV and water vapor signatures. The second talk, by James Berdegues and Pauley (presented by Pauley), examined the ageostrophic vertical circulations as portrayed by the divergent ageostrophic wind and the vertical motion following the methodology proposed by Keyser. Results from numerical model simulations of ERICA IOP-4 suggested that frontogenetical forcing was important to the development of the IOP-4 cyclone. The contribution of diabatic heating to the development of the downstream jet was also highlighted. The next talk, by William Lapenta (with Franklin Robertson, Perkey, and Kreitzberg), examined the role of the sea surface temperature (SST) distribution on various ERICA cyclones. The Gulf Stream position was changed in a series of simulations. The primary result was that when the storm was south of the Gulf Stream it tended to develop more than if it was placed north over colder water.

The next talk, by Mark Sinclair, examined the use of relative vorticity for determining the onset of oceanic cyclogenesis in the Southern Hemisphere. Using ECMWF analyses, Sinclair demonstrated that low-level relative vorticity maxima were evident in regions where satellite imagery showed cyclogenesis. He hypothesized that the relative vorticity maxima would track poleward during cyclogenesis and that development was initiated by favorable juxtaposition with upper-level forcing. The final talk of the session, by Robert Kelly, Da-Lin Zhang, Roebber, and Gyakum (presented by Gyakum), examined the large-scale structure of the early stages of North Pacific cyclones. Their results showed that the stronger the initial low-level circulation, the stronger the subsequent cyclogenesis tended to be. Stronger systems were associated with phasing of northern and southern systems and storms that moved south of Japan but not over Japan. Stronger storms were also associated with a favorable thickness advection pattern 24 h prior to development. Gyakum indicated that an antecedent low-level cyclone may be important for realizing strong development. During the
discussed the role of the downstream ridge in the development of strong systems was noted.

i. General discussion—Bosart and Roebber

Time was set aside for the unscheduled presentation of new ideas and research, and for the general discussion of issues raised during other sessions. These are summarized below.

In an effort to organize ideas concerning upper-level fronts, Carlson presented a conceptual model of cold frontogenesis aloft. This model describes a sloping interface from the cold front aloft to the surface, which may be reflected in a surface front. The flow entails two main streams of air: a descending branch behind the surface front and an ascending branch out ahead of the upper cold front, leading to a frontogenetic confluent zone with a sharp gradient in humidity.

Baldwin summarized the ETA model currently under development at NMC. The model’s most distinguishing feature is a modified sigma coordinate ($\eta$) in the vertical with silhouette step topography, allowing the use of more realistic terrain. Additional features include higher horizontal resolution (40 km) and more coordinate surfaces in the boundary layer and near the tropopause than in current operational models. Implementation is currently planned for January 1994, when the ETA model will replace the Limited-Area Fine-Mesh Model (LFM). The LFM, however, will still be used to generate model output statistics (MOS) for Alaska. Operationally, the ETA model will be run to produce twice-daily 48-h forecasts, along with 36-h mesoscale forecasts.

A diagnosis of vertical motion in the vicinity of a numerically simulated jet streak embedded within a short-wave trough was presented by Keyser (with Chapman). They showed that it was possible to diagnose the “classic” four-quadrant vertical motion signature associated with jet streaks, using two modified forms of the quasigeostrophic omega equation, and showed that this signature results from the component of the $Q$ vector normal to the isentropes and from the vertical gradient of advection of the geostrophic shear vorticity. The jet streak signature accounted for one-third to one-fourth of the total vertical motion pattern in this particular case and had the effect of shifting the location of the upward-motion maximum ahead of the trough axis toward the cyclonic shear side of the jet.

An overview of the early Norwegian conceptual frontal models was presented by Shapiro. Based on unpublished sources (private letters from J. Bjerknes), Shapiro deduced that Tor Bergeron was responsible for proposing the occlusion concept in the frontal model that appeared in Bjerknes and Solberg (1922). This frontal model included both primary and secondary warm/cold fronts at the surface and aloft and recognized zero-order discontinuity cold fronts. Shapiro noted that the frontal structures discussed in Shapiro and Keyser (1990) were also described by this frontal model. The audience was inspired by Shapiro’s presentation, and it was suggested that this historical perspective be presented in an article in the AMS Bulletin.

Bleck helped to get the general discussion started by posing the question, “Would PV arguments predict that most cyclones are type B?,” in reference to the stratification of cyclone development suggested by Petterssen and Smebye (1971). Bleck suggested that consideration of the meridional variation of surface potential temperature versus similar variations of tropopause-level potential temperature indicates the overall dominance of the upper-level PV charge. The general discussion that followed focused on the applicability of PV concepts to problems of both theoretical and operational interest, and whether the trend toward the use of these concepts exclusively in research was in itself a problem. Although no clear consensus emerged, a key point was that PV represents a new tool for both the theoretician and the forecaster, and, as such, should be put to use where and when it is demonstrated to be useful.

The session closed with a discussion of future problems. The problems considered were diverse; however, the discussion emphasized the need to change the focus of cyclone research from the deepening phase of only the most extreme events toward a broader understanding of the entire life cycle of storms. This understanding requires research across the full range of time and space scales and the full range of cyclone behavior, including weak storms. Additionally, more specific consideration of the role of anticyclones, both in terms of their contribution to the dynamics of cyclone evolution and as a research problem in their own right, was urged.

j. Oceanic cyclogenesis—Gyakum and Schultz

Gyakum presented “Memories of Sanders and Gyakum (1980) and Recent Results from CASP.” Gyakum was asked to present his and Sanders’ recollections of putting together their seminal paper on the climatology of explosive oceanic cyclogenesis. While Gyakum was a graduate student at MIT, Sanders had offered several problems for him to investigate. The one in which Gyakum was most interested was the long history of difficulty in forecasting oceanic cyclogenesis. Gyakum then showed Fig. 3 from Sanders and Gyakum (1980), which exhibited the climatology of rapidly developing storms. Rapid cyclogenesis tends to occur in western oceanic basins. Whether this region is favored because of the cold-air outbreaks over the warmer waters and the associated sensible
heat transfer or because the strong SST gradient acts to enhance baroclinity is a topic of active research today. One region evidently not associated with either effect is the central Pacific southwest of Alaska and may have been associated with the presence of ocean

weather ship Papa, since removed (suggested by Bosart), or downstream Rossby wave propagation (suggested by Shapiro). Cyclone climatologies also suggest the existence of a secondary western Atlantic cyclogenesis maximum off the coast of Labrador over sea ice or relatively cold SSTs. Intense storms in the Newfoundland–Labrador region frequently are small-scale phenomena. Understanding such cyclone structures was one of the motivations of the Canadian Atlantic Storms Program 2 (CASP 2), the field phase of which was conducted during January through March 1992.

During the 1970s into the early 1980s, existing operational prediction models tended to underestimate/miss explosive cyclogenesis at all forecast projections. Forecasting of explosive ocean cyclogenesis was much more successful during ERICA. Gyakum suggested several reasons: increased model resolution and better boundary-layer and convective parameterizations. Problems still exist in forecasting rapid cyclogenesis near the western boundaries of operational nested models, possibly because the lateral boundary conditions are not handled well. Sienkiewicz suggested that NMC’s AVN model does a credible job in forecasting explosive cyclogenesis over the North Pacific. Shapiro referred to research at ECMWF suggesting that increased horizontal resolution led to better extended-range forecasts. Gyakum countered with the possibility that the use of parameterizations originally designed for coarse-resolution models and subsequently applied to higher-resolution models may be a problem. Nuss and Tracton debated whether the quantity of observations over the North Pacific Ocean was sufficient for good forecasts. Recent results presented by Gyakum showed that out of 20 cases of oceanic cyclogenesis off eastern Canada, 14 were well forecast by both the RFE and the NGM at 36 and 48 h. The six cases of poor forecasts of cyclone intensification were common to both models. This prompted the oft-cited comment that “the models look more like each other than they do the real atmosphere.” Gyakum concluded by saying that regime changes affected the number of cyclones and that frontogenesis in polar airstreams is not well forecast, and stressed the need to model better the interactions between the mesoscale and larger scales. In the discussion that followed, Mass queried whether cyclogenetic maxima over the North Pacific had been duplicated by GCMs. Steenburgh cited the need to verify position errors as well as central pressure errors. Bosart wondered whether the six misforecast outliers were a function of the large-scale regime. Shapiro noted that in December 1992 and January 1993 dropwindsondes were to be released over the North Pacific and the data included in the NMC and ECMWF models in order to test the effect of enhanced data in this region. He also noted the success of the Norwegians in forecasting polar lows over the Norwegian Sea with a 25-km horizontal resolution model.

Gyakum cited one example in which researchers at the Recherche en Prevision Numerique (RPN) of the Atmospheric Environment Service in Dorval, Quebec, had found it necessary to include a large domain to adequately forecast a small-scale polar low.

Neiman’s (with Shapiro) topic was “Mesoscale Characteristics of an Intense Extratropical Marine Cyclone: ERICA IOP-4.” Neiman presented a review of the observational aspects of ERICA IOP-4. Among the noted features were the presence of a strong upper-level front over land before cyclogenesis and the evolution of the low-level temperature pattern in accord with the Shapiro–Keyser (1990) conceptual model. He also presented distributions of PV within the warm front and its bent-back extension along the polar airstream. Large upward fluxes of heat and moisture from the sea surface exceeded 4600 W m⁻², several times larger than those observed in mature tropical cyclones. He also indicated the need for additional model verification other than the central sea level pressure, including scale-dependent verification. In the future, Neiman suggested the need for further modeling work to examine the interplay between the synoptic and mesoscales, nonconservative processes, and along-front variability. He also suggested the need for validation of structural details from modeling results, and the use of remote sensing technology for examining the structure of cyclones. In the discussion period, Kreitzberg noted that the aircraft flights at the end of the ERICA IOP-4 cyclone life cycle were the smoothest. He also noted that in this case the onset of occlusion did not signify the death of the cyclone since the occlusion formed early in the cyclone’s lifetime. Martin noted the similarity of mesoscale vortices along the warm front to the structure of cellular radar echoes frequently seen along cold fronts. Keyser offered the suggestion that the mesoscale warm-front disturbances were manifestations of shearing instability. Nelson Seaman added that modeling results showed that shearing instability is what breaks up the front and that convection takes over to further destroy its two-dimensionality.

Chapman (with Keyser and Evelyn Donali-Grell) presented “An Examination of the Shapiro–Keyser Conceptual Model of Maritime Cyclones: Representativeness, Vertical Structure, and Dynamical Interpre-
tion north of the warm front. Pauley added that in her wave, frontal fracture, T-bone, and warm-core seclusion for oceanic cyclogenesis and to extend their results to the earlier stages of cyclogenesis and not during the rapid intensification stage as measured by the standard decrease in cyclone central pressure. Growth was attributed both to surface sensible heat fluxes and to the release of latent heat. Low-Nam added the differences in growth rates of two simulations—one without surface fluxes and the other without latent heat release—from the control experiment with both surface fluxes and latent heat included. He then compared this value with the difference in growth rates from a simulation with neither fluxes and latent heat and the control simulation. He found that the latter was larger than the former, implying a synergistic relationship between the model parameterized fields. He concluded that an increase in surface fluxes caused by the positive feedback between the increased wind speed and the release of latent heat was as important as the increase in fluxes during the preconditioning period for cyclogenesis. In the discussion that followed, Davis cautioned that Lorenz’s chaos theory may make it difficult to interpret the results of modeling sensitivity studies when selected parameters are switched off or on.

3. Workshop conclusion

At the conclusion of the workshop there was broad agreement that the emphasis on selected discussion topics had been useful in helping the participants to focus on important scientific issues and problems. Toward this end, Bosart suggested that subsequent cyclone workshops should be scheduled on the basis of compelling research needs. Pauley and Nuss kindly offered to host the next workshop in Monterey, California, at the appropriate time.

References