gated (on average) off to the south. I am using the term "reversible" in the sense implied by the discussion given by Andrews and McIntyre (1978b, §5.3 and appendix C). Viewed synoptically, this reversible decay, and the associated Eulerian-mean zonal acceleration, are associated with the displacement of the surviving part of the main cyclonic vortex back towards the pole and back into line with latitude circles. By "surviving" I mean the inner part of the main vortex, which was centered over Scandinavia at the stage shown in Fig. 3, and which largely escaped the effects of potential-vorticity mixing (cf. Hsu, 1980, Fig. 7c), so that it was a material entity which carried its isentropic potential-vorticity distribution back with it. (From a Lagrangian viewpoint this entity *is* the narrow polar-night jet, whether or not it is displaced away from the pole.) Reversibility, in the sense under discussion, is the physical reason why Palmer (1981b) was able to explain an episode of high-latitude, Eulerian-mean acceleration towards the end of the second stage using the theory of linear, conservative waves.

In terms of the Eulerian-mean description, then, the high-latitude EP flux divergence would have cancelled much of the earlier convergence due to the initial growth of the wave, leaving a region of maximum time-integrated convergence in middle latitudes resembling that discussed in connection with our Figs. 6 and 7. In Eulerian-mean language one can say that the net change in the mean state over the whole wave-1 episode was of the kind illustrated by Figs. 5, 6 and 7 because the transience due to the arrival of the wave-1 disturbance proved to be more or less reversible in high latitudes, but more or less irreversible in middle latitudes in virtue of the fact that it was in middle latitudes that the waves saturated and ultimately dissipated. Thus one can summarize the essential information about the wave-1 precursor event, despite the complexities of the actual day-to-day evolution of the Eulerian-mean state which showed a mixture of reversible and irreversible changes, by the simple statement that a convergence of the *time-integrated* stratospheric EP flux took place in middle latitudes, where the waves ultimately dissipated.

One can summarize the same sequence of events in a more Lagrangian, synoptically-oriented language by saying that as the wave-1 disturbance grew to large amplitude in late January the main polar vortex was displaced southward off the pole, as illustrated by Fig. 3, carrying a central core of potential-vorticity contours with it. Potential vorticity contours outside some such central core would have been eroded away by the wind field associated with the cutoff Aleutian high, as suggested by Fig. 4. The result would have been weak potential-vorticity gradients outside, and sharp gradients at the edge of, the surviving core region which went back towards the pole when the wave-1 disturbance decayed in mid-February—again, just the configuration needed to focus the subsequent wave-2 pulse, sketched in Fig. 5. There is of course no fundamental reason why the process should have stopped there; had large wave-1 amplitudes persisted a little longer, the core could have been eroded away entirely, in one continuous operation. We would then have had an example of a "type B" major warming.

A question now arises as to whether the buildup to the precursory wave-breaking episode we have been discussing was facilitated in its early stages by the proximity of a subtropical critical line. That is another question to which I shall return, but I can hardly resist the temptation to point out immediately that three papers elsewhere in this issue (Holton and Tan, 1982; Labitzke, 1982; Wallace and Chang, 1982) put forward observational evidence carrying the suggestion that major warmings may be somewhat more likely to occur in years when the tropical quasi-biennial oscillation is in its easterly phase. One fact which may prove significant, and which can be seen from Fig. 3 of Labitzke (*op. cit.*, page 130 in this issue), is that the two winters in the period 1953-1980 in which the quasi-biennial easterlies occupied the *deepest* layer (January 1963 and January 1977) both produced warmings of exceptional strength. In January 1979 the layer of tropical easterlies was not so deep, but it nevertheless extended from 30 mb up to higher than 7 mb (Labitzke, *loc. cit.*; Coy, 1979b).

5. Resonance?

So far I have concentrated on those questions on my list concerning which the greatest progress has been made recently. One question I have ignored altogether is question 1, namely why wave amplitudes should become large in the first place. That question is avoided—or so it would seem at first sight—by mechanistic model simulations of the kind I have referred to, in which wave amplitude is simply prescribed at the bottom of the stratosphere. (I have also, so far, been ignoring question 9, by tacitly following the conventional wisdom that the troposphere does act more or less independently of the stratosphere, which of course is a prerequisite for such an artificial lower boundary condition to make any kind of sense at all.)

Observational evidence has long suggested that a good answer to question 1 for the real atmosphere would entail a good answer to the age-old question of what causes tropospheric "blocking" and related anomalies. A theoretician is tempted at once to suggest resonant growth. According to this idea, planetary waves forced by topography or stationary thermal forcing can grow anomalously if the basic state evolves into a configuration such that a free mode of the whole atmosphere exists and is nearly stationary. This possibility has been discussed by a number of authors, most extensively by Tung and Lindzen (1979a, b); see also Clark (1974) and Simmons (1974).

Recently Plumb (1981a, b) has pointed out that for realistic wave amplitudes the strongest growth is to be expected not when the linear condition for resonance is satisfied, but rather when the mean state is initially to one side of resonance, such that if a stationary, topographically-forced wave starts to grow the change in the mean state induced by the wave growth takes the mean state further *towards* resonance. Such "self-tuning" can then lead to further wave growth. It is precisely this positive feedback process that gives rise to the inviscid topographic instability noted earlier by Charney and DeVore (1979, §2) and further studied by Paegle (1979). For general initial conditions, with a free, travelling planetary wave present, Plumb's scenario implies the slowing down and growth of the travelling wave as the mean state approaches resonance. Interference with the stationary, forced wave contributes to the growth of the total disturbance in its early stages and hence to the evolution of the mean state. The model scenario is strikingly reminiscent of the behaviour of travelling waves which is often observed to precede major and minor warmings in the real stratosphere. For instance, such behaviour was observed to precede the large wave-1 event of January 1979 (Madden and Labitzke, 1981; Quiroz, 1979; for several other examples see Quiroz, 1975). This slowing down of the travelling wave is itself a clear indication of an approach to resonance.

Plumb presents a combination of detailed analytical theory and numerical experiments which demonstrate convincingly that nonlinear resonance in this sense plays an important role at least in certain idealized model warming simulations, of the general sort first studied by Geisler (1974). Geisler's model is like Matsuno's but ignores questions of latitudinal propagation and focusing, by restricting the flow to a beta-plane channel bounded by latitudinal, perfectly-reflecting walls, and truncating spectrally in the meridional as well as the zonal direction (see also Holton and Mass, 1976). Clearly there is plenty of scope for a resonant cavity to form in such a model. It can happen for instance if vertically propagating planetary waves are reflected back down from high altitudes in the model, in the manner discussed by Plumb, and by Tung and Lindzen in their second paper (1979b).

In spherical geometry, the defocusing effect illustrated in Fig. 1 suggests that reflection from the tropics or subtropics is likely to be more important than reflection from above, in helping to form a hemispheric or smaller cavity capable of exhibiting resonant behaviour. Wave activity could be sent back along the ray paths suggested by the directions of the EP fluxes in Fig. 1 if there were a suitably oriented reflecting surface in the subtropics. (During resonant growth the *net* wave flux would still be represented in that case by arrows directed as in Fig. 1.) As already mentioned, one of the ways in which such a reflecting surface could arise is through the presence of a nonlinear critical layer, as was proposed in this context by Tung (1979). The conditions under which reflection would occur have been much debated, but it can be shown very generally, following an argument presented by Killworth and McIntyre (1982), that

(a) if a quasi-geostrophic Rossby-wave critical layer is entraining no new potential-vorticity contours (by growing in width, for instance, or by translating sideways), and

(b) if the overall time scale is short enough for the critical-layer region to be considered free of external, zonal-mean forcing (including that due to mean diabatic effects, and to mean viscous stresses, if any, at the edges of the critical-layer region),

then the critical layer cannot sustain wave absorption for much longer than the time for wave breaking to occur. After that it must reflect, at least in a time-averaged sense. (The sustained absorption observed in general circulation statistics applying to seasonal and longer time scales can be traced to violation of condition (b).) The time for wave breaking to occur depends on the wave amplitude, and can be roughly estimated as the time for an air parcel near the center of the anticyclonic system of closed streamlines in the critical layer, when viewed in a frame of reference moving zonally with the wave, to travel about halfway round the center. Killworth and McIntyre's argument is a rigorous version of that already sketched in connection with Figs. 4 and 5, the essential point being simply that conservation of potential vorticity restricts the net amount by which the mean profile of potential vorticity can change, in a given latitude band, quite irrespective of the details of the wave breaking and any associated potential-enstrophy cascade. This imposes a bound on the time integrated potential-vorticity flux, and therefore on the time integrated EP flux convergence.* The bound is evidently zero if the initial potential-vorticity gradient is zero, which verifies the fact, mentioned earlier, that a critical layer will reflect immediately if *previous* wave-breaking events, or other causes, have already annihilated large scale potential-vorticity gradients in the critical-layer region.

If a reflecting surface exists in the tropics or subtropics, then the higher its latitude in the winter hemisphere, the smaller the resulting cavity, and the greater the potential for a rapid response. The whereabouts of a critical layer depends of course on the phase speed of the waves giving rise to it. But, other things being equal, we would expect poleward critical-layer positions to be more liable to occur when tropical winds are easterly. That is why I am inclined to believe the hint from observations, referred to at the end of the last section, that stratospheric warmings may be facilitated by the presence of a deep layer of quasi-biennial easterlies.

I am not trying to suggest, of course, that nonlinear critical layers are always perfect reflectors, even when condition (b) is well satisfied. We should not forget condition (a), especially in a strongly time-dependent situation. As soon as wave amplitudes grow, as happened in late January 1979 for instance, the critical layer will widen, and eat into the ambient potential-vorticity gradient on each isentropic surface. Killworth and McIntyre's argument suggests that the critical layer will act as a partial absorber as long as it continues to entrain new potential-vorticity contours. Such entrainment would occur, as noted in the statement of condition (a), either if the wave were growing or if its phase speed were changing appropriately relative to the mean flow, making the critical layer translate sideways. Observations suggest (*e.g.* Madden and Labitzke, 1981, Fig. 3) that *both* effects were occurring in late January 1979. So if resonant growth was involved it may well have been slowed down to some extent by the resulting wave absorption. Dunkerton *et al.* (1981, appendix B) argued that the translation effect reduced, but did not eliminate, the reflectivity of the nonlinear critical layer in the mechanistic model simulation studied there. The importance of such effects, which like those studied by Plumb represent an intrinsically nonlinear aspect of the resonance problem, has yet to be assessed quantitatively.

It is of interest to look for resonant behaviour of the kind under discussion in mechanistic model simulations permitting latitudinal wave propagation, such as the one studied by Dunkerton *et al.*, an early stage of which provided Fig. 1a. For models with an artificial lower boundary, as in this case, the precise conditions for resonant behaviour would be unlikely to agree with those for the real atmosphere, but the phenomenon would be fundamentally the same. C.-P. Hsu and I believe we have found an example of such behaviour in this same model simulation. A weak resonance effect seems to be the correct explanation of a spontaneous growth in the EP wave flux from the bottom boundary which Dunkerton *et al.* had noted but had been unable to explain. This growth, by a factor 2 or so (compare Fig. 1a with Dunkerton *et al.*'s Fig. 1c), is quite important in helping to induce the simulated major warming. It appears to be attributable to reflection from the nonlinear critical layer in the model, as it advances from the subtropics towards a position in middle latitudes from which some of the reflected waves constructively interfere with those forced by the boundary. Evidently this is one of the possible mechanisms whereby real atmospheric cavities,

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* Moreover the bound still holds even if the potential vorticity is subject to eddy diffusion within the critical layer, provided only that condition (b) is not violated. Note on the other hand that no such bound applies to absorption by *gravity-wave* critical layers, since there is no constraint analogous to that imposed here by conservation of potential vorticity.

as well as model ones, might tune themselves toward resonance.

One check on this interpretation comes from the time evolution of the phase tilt in the lower stratosphere shown in Dunkerton *et al.*'s Fig. 3b. Qualitatively speaking it compares well with the phase behaviour of simple analytical solutions describing wave growth in a resonant cavity with the same artificial lower boundary condition, for instance the solutions presented by Simmons (1974, Figs. 10 ff.). The amplitude behaviour is qualitatively similar also (Dunkerton *et al.*, Fig. 3a; Simmons, *loc. cit.*). From a comparison with the patterns of amplitude behaviour shown in Fig. 18 of Matsuno (1971), in Fig. 4a of Schoeberl and Strobel (1980a), and in Figs. 8 and 16 of Koermer *et al.* (1982), one gains the impression that a similar resonant enhancement may have occurred in those simulations as well. Moreover, there is at least one published example of what looks like an occurrence of essentially the same signature in the real stratosphere. This is apparent in the wave-2 amplitude shown in Fig. 2a of Hirota and Sato (1969), which refers to the lower stratosphere in January 1963. It may be significant that the mean zonal wind shown in the same figure is suggestive of a northward-moving critical layer just when the amplitude has the appearance of growing resonantly.

We now come to a subtle and intriguing question. What, if any, is the connection between the foregoing ideas and tropospheric phenomena such as "blocking"? There seem to be two quite separate versions of the resonance theory in the literature, of which the first is the one we have been discussing:

Version 1: The troposphere and stratosphere act as one big cavity, which on occasion becomes tuned—or rather suitably *detuned*, as we should say in the light of Plumb's work—in such a way as to exhibit resonant behaviour and lead to large planetary-wave amplification.

Version 2: The troposphere acts as a cavity by itself, and decides what it will do largely independently of the stratosphere, to a first approximation. The stratosphere merely responds as to a given forcing from below.

Tung and Lindzen's second paper (1979b), for instance, is concerned with version 1, and their first paper (1979a) relates more directly to version 2. In support of the notion of an independent troposphere involved in version 2 there is the long-familiar fact that many aspects of the large-scale flow in the middle and upper troposphere seem to be captured by simple barotropic models —hence the time-honoured notion of "equivalent-barotropic" flow in the troposphere. The ray-tracing results of Karoly and Hoskins (1982, page 113ff. in this issue) provide further support by suggesting that the leakage of stationary planetary waves from the troposphere into the high stratosphere is quite modest: many ray paths originating in the troposphere stay within the troposphere or lower stratosphere, because the troposphere tends to have a higher refractive index squared than the lower stratosphere in middle latitudes (see also Fig. 3 of Matsuno, 1970, and Fig. 8b of O'Neill and Youngblut, 1982). Comparatively few rays go high into the stratosphere, even for zonal wavenumbers 1 and 2. No rays for zonal wavenumbers 3, 4, etc. can cross the lower stratosphere at all in middle and high latitudes, which for those wavenumbers and the conditions assumed is a "tunnelling" region where the corresponding refractive index squared is negative (*i.e.* where the quantity plotted in Fig. 5c of Karoly and Hoskins, 1982, page 117 in this issue, is less than 3, 4, etc.). Further theoretical evidence for the approximate independence of the troposphere can be found in recent papers by Held (1982), Hoskins and Karoly (1981), and Simmons (1982a).

I should say at once that I am not trying to suggest that there is any real conflict between these two versions of the resonance theory. As will be explained more fully in the next section, I think they simply represent idealizations of two different aspects of the problem. Version 1 may well help explain at least some of the large, ultra-long wave events seen most prominently in the upper stratosphere, but should probably not be advanced as an explanation of "blocking". Whether version 2 can explain "blocking" is a question which we have not yet touched on. A closer look at that question seems worthwhile, since it will lead to a better appreciation of the nature of the connection between blocking and stratospheric warmings, and in the process to what I believe is a new suggestion for overcoming the problems introduced by artificial lower boundaries (and hence incorrect resonances) in mechanistic models of stratospheric warmings.

Version 2 of the resonance theory involves two separate questions. One is whether the troposphere acts independently of the stratosphere, and the other is whether an independent, equivalent-barotropic troposphere behaves like a resonant cavity. Now the idea that the whole troposphere, or one hemisphere of it, might behave as a resonant cavity seems inconsistent with tropospheric observations. For instance the idea does not seem to fit in very well with the observed fact that blocking highs in, say, the Pacific are not accompanied very often by those in the Atlantic or elsewhere round the globe. To be sure, the wave-2 peak in late February 1979 shown in Fig. 2 was associated with simultaneous Atlantic and Pacific blocking highs, but this seems to be unusual. Synoptically one has the impression that the simultaneity in such examples is fortuitous, and that certain locations are dynamically special in some way, independent of what is going on in most other parts of what one might have envisaged as the tropospheric cavity.

That impression is reinforced when one looks at the "teleconnection" patterns whose existence was foreseen by pioneers like G. T. Walker and J. Bjerknes, and which have been emerging more and more clearly in recent years through the systematic use of objective statistical techniques on long time series of tropospheric data. Some recent references are the papers by Wallace and Gutzler (1981) and the thesis by Dole (1981). It now seems clear that these patterns are an important key to understanding the variability of the tropospheric stationary-wave patterns on time scales from several days to a few weeks. As is illustrated by Fig. 2, these are the time scales of interest for the dynamics of stratospheric warmings. One of the most remarkable things about the teleconnection patterns is that, provided the time-mean state and the anomalies about it are defined appropriately, using suitable low-pass filtering of the time series, the spatial anomaly patterns in a given geographical location, particularly the large-amplitude anomalies found in the north Atlantic and Pacific, look much the same for anomalies of either sign.

The independence of sign carries a suggestion that some kind of linear theory ought to be relevant. Indications that this is indeed the case have been emerging from recent work on Rossby-wave propagation on a sphere (*e.g.* Hoskins and Karoly, 1981, & refs.). A number of such calculations have suggested that direct, one-way propagation of trains of stationary, equivalent-barotropic planetary waves from sources in the tropics may account for a good many aspects of the observed teleconnection patterns. The calculations use equations linearized about a zonally symmetric state. When plausible estimates of dissipation are used, resonance plays no significant part in these calculations. There is, however, a still more recent twist to the story. It comes from a remarkable series of model experiments by Simmons (1982a), which extends Hoskins and Karoly's work and appears to take us significantly closer to explaining some aspects of the teleconnection patterns, particularly the large amplitudes of anomalies in the north Atlantic and Pacific. I am told that G. Branstator of NCAR (Boulder, Colorado) has done similar experiments independently, with similar results. The experiments of particular interest to us used a simple barotropic spectral model of the troposphere, and took as basic state not a zonally-symmetric state but, rather, an observational estimate of the climatological January 300 mb height pattern. In the model this pattern was held steady by the application of a suitable distribution of vorticity forcing, conceived as representing whatever topographical, thermal, and time-averaged transient eddy effects are needed to maintain the climatological pattern in the real atmosphere. A localized anomaly in the forcing was then introduced by switching on an additional vorticity source. This was tried at various places in the tropics. Strikingly large responses were obtained in the north Atlantic and Pacific regions, for certain (respectively different) forcing locations. There was no special tendency for the Atlantic and Pacific responses to occur together. The responses were larger by up to an order of magnitude, in streamfunction or geopotential height, than those obtained in experiments with the same forcing anomaly using zonally-symmetric basic states.

These results again reinforce the view that resonance is not likely to be involved on a hemispheric or global scale. The large response builds up well before there is time for significant information to get into other parts of the hemisphere and reflect back. The general behaviour and in particular the time dependence found in the experiments do seem to suggest, on the other hand, that the north Atlantic and Pacific diffluent-jet regions may be acting as comparatively *localized* resonant cavities, involving wave reflection in the zonal as well as the latitudinal direction. The reflection might be only "partial" reflection, not describable by ray-tracing calculations. The cavities would no doubt be quite leaky in any case. Even a very leaky cavity could produce resonant enhancement by a factor of 2 or 3 without much trouble, which is already significant for present purposes. This, then, suggests yet another version of the resonance theory, which as far as I know has not been proposed before:

Version 3: In the northern hemisphere winter the parts of the troposphere over the north Atlantic and Pacific act as separate resonant cavities, which can be excited independently of each other and of the stratosphere, to a first approximation. The stratosphere still responds as to a given forcing from below, and the stratospheric response (for a given state of the stratosphere) will tend to be strongest in wave 1 when the Atlantic and Pacific anomalies happen to have opposite signs, and strongest in wave 2 when they happen to have the same sign.

If this version is a good approximation to the truth then there should be some tendency for the "strong wave 1" and "strong wave 2" conditions in the stratosphere to be mutually exclusive, especially at times when the magnitudes of the Atlantic and Pacific anomalies are at their largest. Time series of stratospheric wave amplitudes should tend to show minima in stratospheric wave 2 at about the same time as large maxima in stratospheric wave 1, and vice versa. Such behaviour is indeed observed, and has often been remarked upon. It is a noticeable feature of Schoeberl's "type A" pattern, of which Fig. 2b is an example. Other examples, at 30 mb as in Fig. 2b, can be seen in Fig. 1 of Labitzke (1982, p. 127 in this issue) and at 100 mb in Fig. 1 of Koermer *et al.* (1982). Of course the wave amplitudes, especially in the high stratosphere, must also be affected by the variability in the responsiveness of the stratosphere itself, which is to be expected for reasons already discussed.*

Version 3 of the resonance theory is not the only idealization which might be capable of explaining the facts under discussion, although my present feeling is that it is the most likely one. An alternative possibility is that large tropospheric responses in the Atlantic and Pacific regions might arise simply from horizontal focusing, and as such might be explicable by ray-tracing calculations in which rays bunch together, or cross each other, forming a "caustic", as they go through the region in question, without reflecting back and forth locally. It would be interesting to carry out the appropriate ray-tracing calculations for the barotropic time-mean state in Simmons' model to see whether or not they can account for the model behaviour in some such way.

In either case, the theory as developed so far is still consistent with the original idea that the anomalies in the north Atlantic and Pacific regions have the nature of a one-way response, with or without resonant enhancement, to something that goes on in specific (and respectively different) locations in the tropics. When one asks what that something might be, tropical sea-surface temperature anomalies come naturally to mind. However, while tropical sea-surface temperature anomalies may well account directly for some of the mid-latitude anomalies, especially the longer-lived ones (*e.g.* Horel and Wallace, 1981), recent studies based on general circulation models have demonstrated that this is unlikely to be the whole story. It appears that realistic teleconnection patterns can be produced by general circulation models even when sea-surface temperature is held *constant* (Lau, 1981; M. L. Blackmon, personal communication). Simmons' work seems to suggest a likely explanation for this, too. In his barotropic model experiments the large extratropical response to a given tropical anomaly is sensitive to small variations in the basic state adopted for linearization. The fact that a large response was found at all is itself an indication of such sensitivity. Simmons notes other evidence for this from his experiments, and confirms the sensitivity directly by varying the basic state. If version 3 of the resonance theory proves to be correct, the effects of this sensitivity can probably be thought of in terms of variability in the leakiness of the Atlantic and Pacific tropospheric cavities.

6. Mechanistic models with realistic lower boundary conditions: a suggestion concerning question 9

Let us now summarize the picture that seems to be emerging. The old idea that the troposphere acts almost independently of the stratosphere, especially the upper stratosphere, to a first approximation, continues to be borne out by theoretical developments. But if resonance in the troposphere is involved it seems more likely

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* I suspect that this latter consideration accounts for much of the high-altitude variability of waves 1 and 2 found in the two simulations by Koermer *et al.* (1982), using a topographically forced model in which waves 1 and 2 were forced simultaneously and steadily. Indeed the amplitude behaviour found there is reminiscent of the resonant model behaviour discussed earlier in this section.

to be in the sense of version 3 of the resonance theory than in the sense of version 2. The question remains, in any case, as to whether and how such ideas fit in with version 1, the version envisaged in Tung and Lindzen's second paper (1979b) and developed to a further stage of sophistication by Plumb (1981b). As we have seen, that version also has strong claims on our attention. It seems to entail that the troposphere and stratosphere are *not* independent, with the implication that mechanistic models of the stratosphere with artificial lower boundary conditions may seriously misrepresent reality in this respect.

The view which now seems likely to prove correct is that versions 1 and 3 refer to distinct, co-existing "modes" of wave motion in the real atmosphere, although since nonlinear fluid dynamics is involved the term "mode" is not to be taken in too precise a sense. Version 1 applies to the ultra-long zonal harmonics, waves 1 and 2. These do feel the ground, and extend up through the troposphere and far into the upper stratosphere in winter. Observational and theoretical studies of ultra-long travelling waves suggest that they can organize themselves at least part of the time into free normal modes, implying the existence of deep cavities on a hemispheric scale spanning the troposphere and the stratosphere (*e.g.* Madden, 1979; Schoeberl and Clark, 1980; Salby, 1981). The strongest nonlinearity in the dynamics of these waves occurs not in the troposphere but in the high stratosphere, manifesting itself in the saturation or wave-breaking phenomenon already discussed. As we have seen, wave breaking may itself contribute to the formation, or re-shaping and re-tuning, of an effective cavity. For this and other reasons one would expect the damping and structure of ultra-long-wave free modes to be quite variable (and not necessarily classifiable according to the separable vertical and horizontal structures of tidal theory); but the bottom ends of the gravest modes, so far as they have been detected observationally, tend to have an "external", node-free vertical structure in the lowest few scale heights (*e.g.* Madden and Labitzke, 1981, p. 1252). (This particular free-mode structure, incidentally, is not allowed in mechanistic models in which geopotential height is specified at an artificial lower boundary.)

Version 3, by contrast, refers to tropospheric "long waves" having somewhat smaller horizontal scales, on the whole, and an equivalent-barotropic vertical structure which is comparatively well confined to the troposphere (*e.g.* Fig. 21 of Blackmon *et al.*, 1979). For these tropospheric "long waves" the dynamics is linear or nonlinear according to one's viewpoint. Viewed as anomalies about a "climatological" time mean they seem to behave to some extent linearly, if we take at face value the observational and theoretical evidence already discussed. But what might appear as a linear response in the time-mean formulation would certainly require consideration of a complicated set of nonlinear wave-wave interactions to describe it in terms of *zonal* means and deviations. The translation from the one type of description to the other should contain important clues to understanding the relation between versions 1 and 3, and hence to finding good answers to the last part of my question 9, on how the stratosphere should be coupled to the troposphere in mechanistic models.

The idea that the phenomena envisaged in version 3 are nearly independent of those envisaged in version 1 suggests an analogy with Lighthill's theory of aerodynamic sound generation (*e.g.* Crighton, 1981). In its simplest form, that theory considers the generation of low-frequency sound waves (which we are now going to regard as analogous to the generation of the ultra-long stratospheric planetary waves involved in version 1) by an isolated patch of nonlinear, usually turbulent, fluid motion at low Mach number (which we shall regard as analogous to the large-amplitude tropospheric motion involved in version 3). Lighthill's theory exploits the fact that the turbulence is only a weak radiator of sound, and the reaction of the sound back onto the turbulence correspondingly weak. Thus to good approximation the problem can be solved in two separate stages. The turbulent motion can be computed, or observed, by methods which ignore the presence of the sound field and do not attempt to separate "sound" from other motions—for instance the equations of incompressible flow can be used. This motion is then regarded as known to good approximation, and substituted into the nonlinear terms appearing in the equations for *compressible* flow, giving known source terms from which the acoustic response is subsequently computed. The method works because under the assumed conditions, and with a judicious choice of the mathematical form of the source terms, the acoustic far field is insensitive to errors in representing the turbulent motion. (There is no reason, on the other hand, to suppose that the same would be true of the near field within the turbulent region itself.)

The analogy, then, suggests a similar exploitation of the idea that large-amplitude tropospheric motions can nonlinearly excite the ultra-long planetary waves envisaged in version 1 while themselves remaining unaffected to some approximation. The simplest realization of the idea could start with the observed height field at say 250 mb. Instead of imposing that field at an artificial lower boundary, it could be multiplied by an empirical, equivalent-barotropic vertical structure $f(p)$ suggested by observations (*e.g.* the vertical structure for the Atlantic and Pacific areas shown in Fig. 21 of Blackmon *et al.*, 1979 —note that this falls off sharply in the lower stratosphere), and the result substituted into the nonlinear terms in the dynamical equations of a suitable model. The model would represent the whole atmosphere including the troposphere and the high stratosphere, as in Schoeberl and Strobel (1981b) and Koermer *et al.* (1982). By "nonlinear terms" I mean those contributions to the advection terms which would be neglected if one were linearizing the equations about some reasonable zonally-symmetric state. The contributions to these nonlinear terms from the self-interaction of the prescribed tropospheric motion would be written, so to speak, on the right of the equations, and from then on, as far as the model is concerned, treated as a *known forcing.*

The main interest would then lie in the projection of this forcing onto zonal harmonics 1 and 2. The key point about forcing the model in this way is that it does not change the tuning of any ultra-long-wave resonant cavities which might exist in the model, as envisaged in version 1, but does allow those cavities to be excited by nonlinear coupling to the tropospheric blocking or other anomalies envisaged in version 3. Following Lighthill, one neglects the cross-terms involving products of the given tropospheric motion with the model's response to it. By analogy with Lighthill's theory, one would not expect the response of the model to this forcing to give an improved approximation to the detailed flow in the troposphere itself, which is analogous to the turbulent near field in the acoustic problems. But it might be reasonable to hope that the ultra-long-wave response, including the nonlinear response in the high stratosphere, would be well represented, like the acoustic far field.

The simplest realization described above would use geostrophic winds for the prescribed tropospheric motion, and because of the assumed equivalent-barotropic structure there would be no nonlinear forcing from the temperature-advection terms. More elaborate realizations are clearly possible, using observed "tropospheric" winds and temperatures at a number of levels, up to 50 mb or so, and it will be important to find out how much difference this makes. It will also be of great interest to see how much it matters whether or not the tropospheric fields are *low-pass-filtered* to suppress fast-evolving, synoptic-scale motions such as mid-latitude depressions. Since the latter tend to be organized into "storm tracks" on a global scale the associated nonlinear effects might turn out to contribute directly and significantly to the forcing of the ultra-long waves.**

7. Shear instability?

The idea that shear instabilities, either barotropic or baroclinic, might play a major role in sudden warmings has been out of favour for many years now, and for quite good reasons on the whole, although suggestions that they might be important have been made from time to time since Charney and Stern's well-known paper of 1962 (*e.g.* Kuo, 1979; J. Frederiksen, personal communication). As far as I know, no-one has ever produced a remotely realistic-looking stratospheric warming simulation of which a major cause was shear instability. In his study of a warming occurring in a general circulation model, O'Neill (1980) tested whether necessary conditions for shear instability had been satisfied, and concluded that such instability was unlikely to have been important. Of course potential-vorticity patterns of the sort suggested by Fig. 4 do undoubtedly tend to be barotropically unstable on small scales. A specific example has been analyzed in detail by Killworth and myself (1982). However, that kind of instability is probably more important for expediting the small-scale mixing of potential vorticity than for large-scale developments. As such it could be said to play a supporting role in the drama but not a leading one.*

Nevertheless, the transient details of what I have been calling the "mixing" of potential

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* In particular, the instability makes no difference to the conditions (a) and (b) mentioned in section 5, under which sustained wave absorption by critical layers is impossible—contrary to an earlier speculation of mine reported at the IAMAP meeting in Canberra in 1979.

** Yes! Opsteegh & Vernekar JAS 39, 737.

vorticity depend on the precise history of planetary-wave behaviour as well as on the pre-existing distribution of potential vorticity. While we remain comparatively ignorant over what the potential-vorticity distributions on isentropic surfaces look like during real warmings it is difficult to rule out the possibility that breaking planetary waves might sometimes produce sufficiently large regions of negative potential-vorticity gradient at the right moment for significant large-scale instabilities to ensue. In addition, I have not mentioned diabatic effects, which could in principle directly generate such gradients, albeit rather slowly. In the analysis by Kanzawa (1980) for the period prior to the major warming of January 1973, there are some indications of negative gradients even in the Eulerian zonal mean.

Very recently, O'Neill and Youngblut (1982) made the interesting suggestion, supported by computations of large-scale potential-vorticity gradients from observations, that barotropic instability could have expedited the spectacular warming of January 1977. During that exceptional event, easterly winds appeared in the tropospheric as well as the stratospheric polar cap. The potentially unstable region appeared to be centered on the upper troposphere. It remains to be shown, however, that instability theory would predict growth rates, phase speeds, EP flux patterns, etc., consistent with the observed behaviour. For a pure barotropic instability one would expect horizontal EP fluxes from regions of negative into regions of positive potential vorticity gradient (like Fig. 2 of Edmon *et al.*, 1980, turned on its side). However, the observed directions of the EP fluxes do not seem to bear an obvious resemblance to any such pattern.

It should be cautioned that the zonal mean states predicted by numerical simulations of warmings using *mechanistic models* which are spectrally truncated in the zonal direction could be quite misleading as a basis for estimating the potential for large-scale instability. Such truncated models have a strong tendency to predict negative Eulerian-mean potential-vorticity gradients across the entire region of wave breaking, as was first shown by Geisler and Dickinson (1974). The models exaggerate these gradients because the truncation prevents them from correctly representing the smaller-scale aspects of the mixing process, with the result that the model's imitation of the mixing process takes place on larger scales than is realistic. So the negativeness of large-scale potential-vorticity gradients, and with it the potential for large-scale shear instabilities, would almost certainly be overestimated by any such truncated model.

8. Concluding remarks

Perhaps the most remarkable thread running through the recent developments I have touched upon concerns question 3 on my list. It seems that linear planetary-wave theory is more powerful than one might have thought—provided we treat it fairly by linearizing about a basic state which fits the physical situation as closely as possible. The promising results from simple, barotropic models simulating aspects of tropospheric "blocking" and related anomalies, by linearizing about a climatological time-mean state rather than a zonally symmetric one, are a case in point (section 5). The sensitivity of stratospheric planetary-wave focusing or defocusing to the precise configuration of potential-vorticity gradients in the polar-night jet is another (section 3). This has an obvious bearing on questions 2 and 8 concerning the forecasting of warmings, and in particular it helps answer the old question why major warmings are relatively uncommon.

These discoveries raise the difficult question of what should be meant by a basic state "which fits the physical situation as closely as possible", when as often happens the basic state is observed only in the presence of large-amplitude disturbances. There is no reason to suppose that an Eulerian mean (time mean or zonal mean as the case may be) is a particularly good answer, even though the work of Butchart *et al.* shows that in some circumstances it may be considerably better than no answer. Eulerian averaging will generally smear out the potential-vorticity gradients to which wavelike disturbances are sensitive. The problem is especially acute when wave amplitudes are large, and it suggests that cross-sections of the Eulerian-mean state would not be a very useful aid to forecasting. It should be noted that Butchart *et al.* took advantage of the unusual gap between the wave-1 and wave-2 events in January-February 1979 in order to minimize this particular problem. Their initial conditions were based on the observed conditions for 16 February, near the crossover point in Fig. 2b at which conditions were least far from zonally symmetric.

Of course the question of how the basic state should be estimated may seem beside the point for the purposes of detailed, high-resolution numerical forecasting, to which a division into "basic state" plus "disturbance" is irrelevant. But a good practical definition might lead to a way of producing two-dimensional observational pictures of potential-vorticity gradients and refractive indices which would be of real help to a forecaster with no access to the full machinery of a high-resolution numerical forecasting facility. And together with the suggestion in section 6 it might lead to the development of a more powerful generation of inexpensive, mechanistic, "wave-mean" models with which to study the scientifically important questions of sensitivity and so on, the answers to which must eventually have an impact on any forecasting procedure.

One simple idea worth trying would be as follows. In the situation suggested by the heavy curves in Fig. 5, the contours of Ertel's potential vorticity in each isentropic surface are tightly spaced in the main part of the polar-night vortex, and will tend (after judicious smoothing to remove small-scale "debris") to have the same overall shape as the vortex itself. The same will be true of the contours of quasi-geostrophic potential vorticity in isobaric surfaces (Charney and Stern, 1962). The idea is to invoke the approximations of quasi-geostrophic theory and to associate with the distorted polar vortex a zonally symmetric basic state which is constructed simply by deforming these quasi-geostrophic potential-vorticity contours back into zonal circles, in each isobaric surface, preserving the area enclosed by each contour (a procedure consistent with the standard approximations of quasi-geostrophic theory). One could then construct a corresponding zonal velocity profile $\bar{u}(y, z)$ by solving a Poisson-like equation in two dimensions, which is a well-conditioned, and by modern standards computationally inexpensive, process. The resulting zonally symmetric polar-night-jet structure would have potential-vorticity gradients closely comparable to the actual large-scale gradients in the distorted polar vortex, and would almost certainly be more relevant to questions of focusing and so on than the Eulerian zonal mean. The precise result would depend on the boundary conditions adopted when solving for $\bar{u}(y, z)$, but the result should be much the same for any reasonable boundary conditions imposed not too close to the polar-night jet, say at the ground, in the mesosphere, and in the tropics. It would be most interesting to see whether a procedure like this could help take simulations like those of Butchart *et al.* (1982) another step closer to reality.

It hardly needs saying that there are many important questions which I have failed even to touch on. Some are discussed in the survey by Murgatroyd and O'Neill (1980). One is the effect of stratospheric warming, and the complementary cooling in the tropics and the summer hemisphere (the dynamical reasons for which were spelt out by Dunkerton *et al.*, 1981, §6) upon the infrared radiation budget of the troposphere (Ramanathan, 1977). Another is the comparison between the northern and the southern hemisphere—a further experiment set up by nature which we should try to understand. It is to be hoped that future modelling studies of planetary-wave events on different basic states will include examples representative of the southern hemisphere, so far as conditions there can be estimated.

There is one aspect of question 6 on the possible role of critical lines which I have not mentioned so far, but which should perhaps be put on record for completeness. It is now being recognized more and more clearly that a descending mesospheric critical line is *not* an important part of the dynamics of sudden warmings (*e.g.* Davies, 1981; Dunkerton *et al.*, 1981; Geisler, 1974; Holton, 1976; Houghton, 1978; Plumb, 1981b).* The essential reason is that the fall-off of mean density with height is more than sufficient to cause rapid saturation and wave breaking on a large scale, once waves with realistic amplitudes can be persuaded to propagate vertically or, even more effectively, to focus strongly into the high-altitude polar cap. Under such circumstances the waves will inevitably break quite irrespective of whether a critical line is present.

It should perhaps be recalled, in this connection, that the only reason why critical lines appear to play a special role in linear theories which assume steady, conservative waves is that such an assumption forces all the EP wave flux to converge onto the critical line. That is, the assumption forces all the wave transience and dissipation to go on inside the critical-line singularity. It is not the critical line *per se*, but rather the transience and dissipation, that is the fundamental feature to be abstracted from such theories when looking for a correspondence with more realistic models and with the real atmosphere. The point was appreciated by Matsuno and Nakamura (1979, §6), and our Fig. 7 provides a good illustration of it. The horizontal cross-flow is not taking place along

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* *Note in proof:* Also Grose and Haggard (1981).

a horizontal critical layer, as might be thought at first sight from a superficial recollection of Matsuno and Nakamura's model together with approximate theories relating residual and Lagrangian-mean circulations (*e.g.* Dunkerton, 1978). A horizontal critical line is present, but it is many scale heights further up, at about 80 km, and is quite irrelevant to what is going on. Even if the waves had propagated straight upwards (in fact, they were still somewhat defocused), they would have saturated, or dissipated in other ways, well below 80 km.

Planetary waves which are strongly defocused and propagating equatorwards as in Fig. 1, on the other hand, are by no means so certain to saturate in the absence of critical lines. They are not getting into less dense altitudes so quickly, especially in the case of wave 2, and they are spreading into far larger geographical areas. That is why it is in the subtropical or mid-latitude stratosphere, rather than anywhere in the mesosphere, that critical lines—or, more to the point, any regions of sufficiently weak zonal wind relative to the waves—are likely to be significant for determining the favoured sites for wave breaking. And it is the tendency of such regions to become reflectors after wave breaking, as opposed to absorbers during it, under the circumstances discussed in section 5, which appears particularly significant for understanding how warmings work. Reflection from such regions can play two distinct roles. First, it provides a robust mechanism for counteracting the defocusing effect, as discussed in section 3. The mechanism does not cease to operate when basic velocity profiles change slightly. Second, if the wave-breaking region has a suitable shape and position the reflection from it may cause resonant enhancement of wave amplitudes in the stratosphere as a whole (as anticipated by Tung and Lindzen, and discussed in section 5 under the heading "version 1 of the resonance theory"). This may help bring about the large wave amplitudes which could lead to a precursory minor warming or to a major warming. Observational information about wave phase speeds, as compared to zonal-wind strengths in the mid-latitude and tropical stratosphere during the buildup towards a large-amplitude stratospheric wave event, would therefore be of great interest. Better still, synoptic maps of the wind field viewed in a frame of reference rotating with the angular phase speed of each prominent wave component, especially if accompanied by estimates of the motion of air parcels in anticyclonic regions of closed streamlines (recall Fig. 4), would give an immediate idea of where, and how quickly, the waves are breaking.

So where have we got to with the resonance theory, question 5? (Apart from saying that the answer to question 5 is almost certainly yes.) The theory seems to be evolving with bewildering rapidity, as new observational and theoretical evidence becomes available. There is now no real doubt that nonlinear resonance is significant in at least some mechanistic model simulations of stratospheric warmings (Plumb, 1981b), and in a definitely unrealistic way if artificial lower boundaries are used. It seems very likely that resonant effects will be found in most such models, whether or not they allow for latitudinal propagation, and in section 5 I mentioned some evidence for this already in hand. As far as the real stratosphere is concerned, viewed on the time scales of sudden warmings, the weight of evidence, theoretical as well as observational, now points clearly to the existence of deep resonant cavities on a hemispheric scale, in the sense of what I called version 1 of the resonance theory. The damping and structure of those cavities is likely to be highly variable, but other things being equal they should tend to respond fastest and thus be at their most effective (less damped by diabatic effects, for instance) whenever the tropical quasi-biennial easterlies span an exceptionally deep layer, as they did in January 1963 and January 1977. Since the response tends to be largest at high stratospheric altitudes but comparatively modest in the troposphere, such hemispheric cavities probably make little direct contribution to the most prominent equivalent-barotropic planetary-wave anomalies in the troposphere, the anomalies associated with phenomena such as "blocking". As discussed in sections 5 and 6, there are some indications that the latter phenomena may involve an entirely different set of resonant cavities largely confined to the troposphere, which are more local than hemispheric, and which couple *non-linearly* to the "stratospheric" cavity. This idea certainly merits further investigation. We can be sure that more will soon be known about the tropospheric problem, at least, if only because of the special opportunities afforded by the FGGE data for that remarkable year 1979, along with the recognised need to study the interaction between the tropics and middle latitudes in order to make progress in medium-range weather forecasting.

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成層圏突然昇温の力学をどう理解すればよいか？

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成層圏突然昇温に関する Matsuno の先駆的な数値実験が成功して以来，この壮大な自然現象が力学的な原因に由来するものであることは疑いをはさむ余地のないところである。しかし，その理論的なモデル化や衛星観測に基づく諸研究は，昇温現象の詳細にわたる理解や適切な予測に関してある程度の見通しが得られた段階に到ったばかりである。本論文では，この現象に関する最近の研究の進展ぶりを自由に論じ，あわせて，数値モデル化に際して対流圏の運動を先駆的に与えることによって生ずる偽の共鳴を避ける方法など，将来の研究のあり方についても示唆を与える。