

***The Atmosphere—
A Challenge
The Science of
Jule Gregory Charney***



**edited by Richard S. Lindzen, Edward N. Lorenz,
and George W. Platzman**

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Charney's Work on Vertically Propagating Rossby Waves*

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1. Introduction

This essay began as an appreciation and analysis of the work Jule Charney turned to on leaving the Institute for Advanced Study and arriving at MIT. This work is clearly identified with a number of seminal papers: Charney and Drazin (1961), Charney and Stern (1962), and Charney and Eliassen (1964)—each of which still figures prominently in the Citation Index. Unfortunately, it soon became clear that an adequate study of even these three works would hover hopelessly between encyclopedic volume and superficiality. As a result, this essay will eventually focus on Charney and Drazin (1961). A more general preliminary discussion, however, might be in order.

Charney came to MIT in 1956. Behind him were his doctoral thesis on baroclinic instability as published in Charney (1947) and his crucial related work on the quasi-geostrophic approximation (Charney, 1948). A limited version of the latter already had appeared in the thesis. It should be recognized that without the quasi-geostrophic approximation, the study of baroclinic instability would have been an almost insuperable problem. As noted in Charney's recollections,¹ the above two items were his proudest achievements. The years at Princeton were, of course, extremely important for both Charney and meteorology. Under von Neumann, Charney led the development of numerical meteorology for both forecasting and experimentation. Nonetheless, Charney himself did not feel that his accomplishments in this area were as singular as his earlier achievements.² To be sure, the work at Princeton was collaborative in contrast to his earlier work. Moreover, during the 1950s the bulk of his delivered lectures were essentially somewhat nontechnical discussions of numerical predictions—its achievements and its future.³

* Revision of a contribution to the Charney memorial symposium at MIT, March 1983.

¹ Most of the following footnotes refer to Jule Charney's edited recollections, included in this volume as Chapter 2. On p. 28 Charney specifically identifies his thesis as his most influential work. On p. 30 he describes the revelatory quality of his discovery of the quasi-geostrophic equations. On p. 63 he points out that much of his subsequent work depended on "a consolidation and appreciation of the geostrophic formalism."

² In Chapter 2, p. 30, Charney (referring to the quasi-geostrophic approximation) states, "But I don't think that anything I did in numerical weather prediction was comparable in originality."

³ In terms of record keeping, Charney was remarkably meticulous. In his professional papers (MIT Institute Archives, Manuscript Collection 184) there is what appears to be a complete set of his (sometimes very casual) notes for all lectures he delivered—excluding course notes.

I believe there is reason to think that Charney was beginning to perceive his own role in NWP (numerical weather prediction) as that of a propagandist—and that he was chafing under this constraint. This, however, is not the reason Charney gives for his decision to leave Princeton for MIT. Rather, he refers to the absence of von Neumann, to his sense of isolation at Princeton, and, in particular, to his strong desire to be in an institution with a strong program in meteorological data analysis.⁴ At that time Starr's was perhaps the leading effort in global meteorological data analysis. In addition Lorenz, in developing his treatment of energetics, was adding to the theoretical framework of such analyses. While Starr's approach is under increasing question today, there is little doubt that in the '50s it was reckoned to be the wave of the future.

In view of the above, we cannot be surprised that Charney did not in his early years at MIT turn to numerical models. Despite the reasons Charney gave for coming to MIT, it is not surprising that Charney's first publications at MIT dealt with his earliest interests: quasi-geostrophy, baroclinic instability, and internal Rossby waves. Although it is convenient, as a practical matter, to discuss Charney's activities in terms of his published papers, what Charney did first and foremost was to think—and his thoughts did not always appear in papers. Nevertheless, these first papers are doubtless a good record of some of the thinking Charney must have been doing during his years at Princeton. His paper with Eliassen (Charney and Eliassen, 1964) was also a return to the authors' earlier interest in the role of Ekman pumping—this time applied to hurricane depressions. What is remarkable, perhaps, is that the papers on baroclinic instability (Charney and Stern, 1962) and on internal Rossby waves (Charney and Drazin, 1961) should, in fact, have become the foundation papers for much of modern stratospheric dynamics.

The remainder of this paper will consist of a detailed discussion of a paper which is a particular favorite of mine: "Propagation of planetary-scale disturbances from the lower into the upper atmosphere" (Charney and Drazin, 1961). This paper is noteworthy among Charney's papers in providing quantitative explanations of specific observed geophysical phenomena. Perhaps more important is the fact that it is a lovely example of an array of interesting thoughts seeking an excuse for publication.

2. Charney and Drazin: propagation of planetary-scale disturbances from the lower into the upper atmosphere

There is, probably, no more frequently cited paper in stratospheric dynamics than this one, the authors of which I shall refer to as CD. Consideration of the vertical propagation of Rossby waves led almost immediately to an explanation of at least two striking features of the high-latitude stratosphere: (i) the presence of strong deviations from axial symmetry in winter, and the absence of such eddies in summer; and (ii) the dominance of large scales of eddies (wavenumbers 1 and 2) in the stratosphere, and the virtual absence of high wavenumbers. In addition,

⁴ Charney's reasons for leaving the Institute for Advanced Study and for coming to MIT are discussed in Chapter 2, pp. 52, 63, 64, and 75.

the observed tilt of long waves as a function of wavenumbers is roughly accounted for. I shall describe these results in somewhat greater detail later in this section. The original paper is reprinted in this volume.

Such a detailed, successful, and convincing exploration of observed phenomena is rare in geophysics, and with respect to such achievements this paper is special among Charney's papers. It is interesting, therefore, to note that the stated motivation for this paper was not the explanation of the above observations. The following paragraph, taken from the paper, offers the authors' stated motivation:

Little is known of the long-period, planetary-scale motions in the upper atmosphere, although motions of this type in the troposphere contain the bulk of the atmosphere's energy. To what extent are such disturbances coupled to the motions in the troposphere? To what extent is the so-called breakdown of the polar-night jet in the stratosphere associated with motions in the troposphere? . . . How much of the energy in the troposphere propagates into the upper atmosphere? The answers to these questions would seem to be central to an understanding of the planetary-scale motions of the upper atmosphere. Thus it has long been a source of wonder to one of us that upper-air motions are not coupled in a more obvious manner to those in the lower atmosphere, as, for example, the motions in the solar chromosphere and corona are thought to be coupled to those in the convective layer of the sun. . . . The tidal oscillations, as well as the gravity waves studied by Hines, travel upward with a slowly decreasing kinetic energy density. If the large-scale tropospheric motions were to propagate in this manner, then, because of their vastly greater energy, an atmospheric corona would in all likelihood be produced. The kinetic energy density in the lower troposphere is of the order of 10^3 ergs cm^{-3} . If this energy were to travel upward with little attenuation and be converted into heat by friction or some other means at, say, 100 km, where the density is diminished by a factor of 10^{-6} it would raise the air temperature to about $100,000^\circ\text{K}$. At such temperatures most of the atmosphere would escape the earth's gravitational field. It is important for the understanding of the upper atmospheric motions to know why this does not occur, i.e., why the tropospheric energy is so effectively trapped.

The first sentences provide an admirably stated program for understanding the stratosphere. It is both Drazin's recollection and Charney's,⁵ however, that the possibility of a terrestrial corona was uppermost in Charney's mind. It is also my recollection from a seminar on the subject that Charney gave in 1960 at the Woods Hole-MIT-Harvard Colloquium.

The interesting thing about this motivation is that it assumes vertically propagating Rossby waves could produce a corona. As recently noted (Lindzen and Schoeberl, 1982) this is not a correct presupposition! The vorticity associated with vertically propagating Rossby waves is limited by the vorticity available in the mean flow at each level, and this limit is sufficient to prevent Rossby waves from growing to an amplitude which approaches that needed for a corona. Such a result is intrinsically nonlinear and thus did not immediately emerge from the linearized

⁵ In Chapter 2, p. 35, Charney describes his initial preoccupation with the fact that one "can have vertical propagation of long waves." This was in the late 1940s, and was already influenced by his contacts with Evry Schatzman. In Chapter 2, pp. 77-79, Charney provides a fairly complete discussion of CD.

calculations in CD—though they might readily have checked the point a posteriori.

The quoted paragraph is of interest in that it very concisely points to some of the findings and ideas of the late '50s that influenced Charney:

1. The recent discovery of stratospheric sudden warnings (Scherhag, 1952).
2. The exploration of the upper atmosphere—mainly by rocket—and the discovery of vertically structured winds, which Hines (1959) attributed to internal gravity waves. Associated with this was the verification of amplitude growth as approximately $\exp(z/2H)$ (where H = scale height).
3. The idea in astrophysics that the solar corona is produced by the dissipation of waves excited in the convective region. This idea was intensively pursued by a close friend of Charney's, the astrophysicist E. Schatzman (see Schatzman and Souffrin, 1967).

These new results and ideas were regarded as exciting and fundamental and clearly were the motivating ideas for the authors of CD.

It should be noted that the vertical propagation of Rossby waves was not a new subject. The equations were already developed in Charney (1947) where vertical propagation of Rossby waves was clearly evident. The matter was made explicit in Charney (1949) and in Smagorinsky (1953). (The latter work was done by Smagorinsky at Princeton, where he was working with Charney.) As noted in CD, however, this early work was not really concerned with the stratosphere except insofar as it provided an upper boundary condition for tropospheric solutions. Certainly one of the first to consider the vertical propagation of Rossby waves through the middle atmosphere (as the stratosphere and mesosphere are commonly referred to today) was Ooyama (1958). Ooyama used the previously mentioned $\exp(z/2H)$ behavior of internal waves to show that disturbances initiated in the ionosphere would have negligible amplitudes in the troposphere. For this limited, but important, purpose, it was adequate for Ooyama to ignore the role of mean winds. Mean winds, however, are crucial to the problem of upward transmission of Rossby waves from the troposphere to the stratosphere.

In reading Charney's papers one is frequently struck by the immense value of casting a "broad net." Lindzen and Schoeberl (1982) provide a short and simple answer to the "corona" question, but, had Charney taken this focused approach, CD would have been infinitely less important for the development of stratospheric dynamics. It would also have omitted several other items of potential import for dynamic meteorology.

Perhaps the best way to expose what is in this remarkable paper is simply to go through it—albeit rapidly. The paper is twenty-six pages long. Recall that the paper deals almost exclusively with the vertical propagation of Rossby waves on a beta-plane where zonal flow and static stability are formally taken to be functions of height alone. At least eight of these pages are devoted to methodologies of solving the vertical structure equation in layered atmospheres and in the numerical results of such calculations. Not surprisingly both the methodology and the numerical results were both quickly superseded and are of little current interest. What is exciting about this paper is that it is not simply a paper about why the atmosphere doesn't have a corona, but (as I have already indicated) it was also a vehicle for Charney to remark on many things that he was thinking about since his thesis.

2.1. Derivation of the wave equation (pp. 84-87)

Here CD derive the quasi-geostrophic equations and linearize them to obtain the same equation for vertical structure used by Charney in his thesis. The derivation fairly systematically exploits small Rossby number and large Richardson number approximations. There is, on the whole, little new here. One may, however, wonder why Charney felt compelled to rederive these equations so frequently throughout much of his career. Certainly the treatment in Charney (1948) is particularly lucid and readable. But Charney was obviously concerned with the fact that although hydrostatics and geostrophy were the basic approximations, considerations of Richardson number and geometric scale also seemed crucial. These remain matters of interest and concern to the present.

Among the novelties in this section is the derivation of the wave equation for a spherical atmosphere wherein angular velocity

$$\omega_0 = \frac{u_0}{a \cos \phi}$$

(u_0 = basic velocity, ϕ = latitude, a = earth radius) is dependent only on height, z , and where f (the Coriolis parameter) is taken to be constant. The need to take f as being constant suggested that the beta-plane approximation may have been central to quasi-geostrophic theory—an intuitively disagreeable thought. In fact, Matsuno (1970) has succeeded in generalizing quasi-geostrophic theory so as to allow fairly general variation in u_0 in the meridional plane, and, more important, allowing full variation in f . Pedlosky has also worked on this question using multiple scale analysis (Pedlosky, 1984).

There is also in this section a derivation of an energy equation wherein it is shown that time changes in total Rossby-wave energy (horizontal kinetic and available potential) arise from pressure work terms at boundaries, conversions of zonal kinetic energy, and/or conversions of zonal available potential energy.

2.2. Solution of the wave equation (pp. 87-91)

The wave equation referred to is the equation for the vertical variation of the perturbation northward velocity, v , on a basic zonal flow u_0 where time, longitude and latitude dependence of the form $e^{i(kx+ly-kt)}$ has been assumed. The equation is

$$(u_0 - c) \frac{d}{dz} \left(\frac{\bar{\rho}}{N^2} \frac{dv}{dz} \right) - \left[\frac{d}{dz} \left(\frac{\bar{\rho}}{N^2} \frac{du_0}{dz} \right) + \frac{\beta \bar{\rho}}{f_0^2 u_c} (u_0 - c - u_c) \right] v = 0 \quad (1)$$

where $u_c = \beta / (k^2 + l^2)$. It is noted that the transformation $\chi \equiv (\bar{\rho} / N^2)^{1/2} v$ leads to an equation of canonical form

$$\frac{d^2 \chi}{dz^2} + n^2 \chi = 0 \quad (2)$$

for waves, where

$$n^2 = - \left\{ \frac{(k^2 + l^2) N^2}{f_0^2} + \sqrt{\frac{N^2}{\bar{\rho}}} \frac{d^2}{dz^2} \sqrt{\frac{\bar{\rho}}{N^2}} \right\} + \frac{N^2}{u_0 - c} \left\{ \frac{\beta}{f_0^2} - \frac{1}{\bar{\rho}} \frac{d}{dz} \left(\frac{\bar{\rho}}{N^2} \frac{du_0}{dz} \right) \right\}$$

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is a local index of refraction. Equations in the form (2) are, by now, old hat in dynamical meteorology. Any discussion of such an equation these days would likely focus, for example, on the role of meridional gradients of potential vorticity in vertical propagation of Rossby waves. No such discussion is found in CD. Only 25 years ago, the very idea of vertical propagation for Rossby waves was sufficiently novel that much discussion was devoted to noting that (2) was like Schrödinger's equation or the equation for electromagnetic wave propagation.

Most of this section is devoted to describing exact solutions to (2) for various special cases (constant u_0 , constant \bar{T} , constant u_{0z} , constant \bar{T}_z , etc.). Most of these solutions are well known—and of primarily technical interest. In fact, the results for constant u_0 and constant \bar{T} succinctly summarize the main results of the whole paper. Under these conditions CD obtain

$$n^2 = -\frac{1}{4H^2} - \frac{N^2}{f_0^2} \left\{ (k^2 + l^2) - \frac{\beta}{u_0 - c} \right\}. \quad (3)$$

Propagation occurs when $n^2 > 0$, and this occurs when

$$0 < u_0 - c < \frac{\beta}{(k^2 + l^2) + (f_0^2/4H^2N^2)} \equiv u_c.$$

For stationary waves ($c \equiv 0$), this equation immediately yields the famous result that vertical propagation is prevented both by easterly u_0 and by westerly u_0 in excess of u_c . This immediately explains the absence of wave disturbances in the easterlies of the summer stratosphere.

Reference to (3) shows, moreover, that for fixed u_0 , increasing horizontal wavenumber leads to trapping with the trapping increasing ($\text{Im}(n^2)$ increasing) as horizontal wavenumber increases. Thus, the dominant eddy scales increase with altitude.

Finally, CD show later in the paper that upward propagation is associated with westward tilt, and (3) gives the variation of tilt with horizontal wavenumber.

These main features of vertically propagating Rossby waves are remarkably consonant with observations. To be sure, more accurate consideration of such matters as variations in shear, meridional curvature, etc. lead to modifications, but much of the essence is here. Indeed, the success of the constant u_0 and \bar{T} calculations suggests that WKB methods would be useful for more general situations. Unfortunately, CD erroneously conclude that because n changes significantly in a scale height, WKB methods are inappropriate.

CD finally conclude that layer methods (wherein the atmosphere is divided into layers where u_0 and \bar{T} are approximated with forms for which analytic solutions are available and solutions are matched at layer interfaces) are the methods of choice in view of their success for Schrödinger's equation. They fail to notice that singularities of the sort where $u_0 = c$ do not occur in Schrödinger's equation, an oversight that leads to the only serious gaffe in this paper.

2.3. *Methods of excitation of planetary waves (pp. 91–92)*

This truly remarkable section has not received the attention it deserves. It contains two points of great importance: (i) it argues that orography is likely to

be more important than heating in forcing stationary waves, and (ii) it notes that it is meaningful to consider baroclinic instabilities above their steering levels in terms of vertically propagating (or trapped) Rossby waves. This feature has been extensively exploited by my colleagues and me in recent years (Lindzen, et al. 1980; Lindzen and Rosenthal, 1981).

With respect to the first point the argument runs as follows: For topography with height h , zonal wavenumber k , and mean flow u_0 , stationary waves are forced by w' at the "ground" where

$$w'(x, y, 0) = u_0(0) \frac{\partial h}{\partial x} = ik u_0(0) h. \quad (4)$$

Using reasonable values CD obtain $w' \sim 0.2 \text{ cm s}^{-1}$. For heating, they consider the zero average zonally asymmetric component of the surface heat (latent plus sensible) flux and ask what w' would be needed to carry this flux into the atmosphere, i.e.,

$$\bar{\rho} w' c_p \bar{T} = F,$$

and conclude, for reasonable (and even excessive) choices, that this gives $w' \sim 0.1 \text{ cm s}^{-1}$, which is smaller than what they obtained from (4).

2.4. Boundary conditions (pp. 92–94)

Even in this largely technical section there are some points of broader interest. First, there is the interesting recognition (discussed in detail in the appendix) that nonlinear correction to (4) may be needed. There is also an awareness that thermal forcing was not well known.

CD therefore introduce the expedient of forcing by specifying w' at $z = 0$. This is, so to speak, a first cousin to Matsuno's approach (1970) of forcing stratospheric stationary waves with observed stationary wave fields at 500 mb.

Much of this section is devoted to the derivation of interfacial conditions in layer models. It is in this section that CD conclude that a layer in which $u_0 = c$ will act as a perfect reflector. As shown by Dickinson (1969c), in a continuous model such a level will act as an absorber—at least in the context of linear theory. A similar error was made by Pitteway and Hines (1965) in the use of layer models for internal gravity waves. In the case of gravity waves the correct result was derived by Booker and Bretherton (1967).

The section also includes a very lucid discussion of the radiation condition and introduces one of the earliest models of a "sponge" layer.

2.5. Standing waves in layer atmospheres; some numerical solutions of layer atmospheres (pp. 94–99)

These sections are devoted to the description of layer atmosphere algorithms and numerical results from such models. Buried in these sections is the interesting result that discontinuities in n^2 associated with the tropopause in such cases where $n^2 > 0$ both above and below the tropopause do not significantly reduce transmissivity. Frankly, I would have missed this result had not CD specifically referred to it on p. 102.

There is also a very suggestive result concerning the upward energy flow due to stationary waves propagating into the stratosphere. CD estimate this flux to be about 4 W m^{-2} which, as CD note, is about the order of tropospheric energy dissipation. To the best of my knowledge, the ramifications of this point have not yet been thoroughly explored.

Both the above results constitute a very persuasive argument for caution in the use of lids or even barotropic models.

2.6. *Nonlinear theory (pp. 99–101)*

This rather tortuous section is where the first version of the now famous Charney–Drazin Theorem is developed. What CD attempt to do in this section is to ascertain to what extent linear theory is valid when waves reach large amplitude. Their approach is to evaluate the effect of waves at second order on the mean flow. Under many restrictions (some unneeded) they arrive at the remarkable conclusion that the waves will not change the mean flow. This is the first version of what have become known as noninteraction theorems.

Simultaneously with this paper and independently, Eliassen and Palm (1961) showed under very general circumstances that there was a wave flux (known now as the Eliassen–Palm Flux and essentially proportional to wave action flux) which, in the absence of damping or critical surfaces, is divergence free. This is known now as the Eliassen–Palm Theorem. The fact that this implies that neither u_0 nor \bar{T} is altered by the wave is now referred to as the Charney–Drazin Theorem.

For approximately six years, little use was made of these results, but subsequently interest has become widespread. Certainly, Dickinson's (1969a) paper which clarified and extended the results in CD—and also coined the name Charney–Drazin Theorem—was important in this regard. Among the subsequent treatments are Boyd (1976) and an array of papers by Andrews and McIntyre (1978 is but one). Recent interesting extensions are due to Palmer (1981) and Plumb (1986).

2.7. *Application to the upper atmosphere (pp. 101–107)*

In confronting the actual upper atmosphere, CD basically eschew explicit wave calculation. Instead they adopt two procedures. Primarily they take observed distributions of zonal wind and use (3) to calculate u_c for various wavenumbers in order to see whether propagation is possible. They also average fields over the latitude band 30N–60N and calculate n^2 as a function of height. The use of n^2 as a diagnostic remains a primary tool of stratospheric (and even tropospheric) dynamics to the present.

Using the meridionally averaged n^2 , CD conclude that planetary-scale waves will be largely trapped except in spring and for a brief interval in autumn. They therefore conclude that upward propagating planetary waves will not play a major role in sudden warnings. As Dickinson (1968) noted, however, propagation is possible in winter at sufficiently high latitudes. Dickinson (1969b) noted that damping due to infrared cooling is primarily responsible for preventing propagation into the thermosphere. Finally, Matsuno (1971) was able to show that these plan-

etary waves interacting with the mean flow near critical surfaces could indeed account for sudden warmings.

That CD missed such points is not surprising. What is important is that CD first identified vertically propagating Rossby waves as important components for stratospheric dynamics. That this process proved even more important and central than CD anticipated is hardly a matter for regret. What is perhaps most important and also most easily overlooked is the fact that this paper first recognized in a clear way that vertical propagation is a real property of Rossby waves—whether they be in the lower or upper atmosphere. It is from this recognition that the numerous implications of this paper stem.

The vertical propagation of Rossby waves is so widely recognized today that it is difficult to imagine that this was not always the case. The continuing popularity of barotropic and two-level models, for example, shows that we have not completely assimilated this result. Indeed, even Charney could be said to have frequently ignored it. Nevertheless, I believe it is fair to say that this was the first paper to identify internal Rossby waves as observable entities. That the identification of a mathematical entity occurs separately from its identification as an observable phenomenon is not uncommon. The Rossby wave, itself, is a good example. None would seriously question the crediting of Rossby with this discovery even though the mathematical entity was discovered by Margules (1893).⁶ In the present instance, however, both the mathematical discovery of internal Rossby waves (Charney, 1947, 1948) and the identification of such waves with observed phenomena (Charney and Drazin, 1961) are due to the same individual.

3. Concluding remarks

Before finishing this essay, I would like to give a brief and somewhat personal indication of the broad impact of Charney and Drazin. No attempt at completeness will be made. It would be a hopeless task to merely mention the hundreds of papers which have cited Charney and Drazin. Instead, I will merely list some of the broad research areas that have developed out of Charney and Drazin and give a few representative references in each area. The small number of references is misleading. In each of these areas one could have given dozens of references. The choices may seem idiosyncratic, but an attempt was made to include at least those papers where fundamental approaches were initiated. There is, perhaps, inordinate reference to my own work. This is not simply a manifestation of vanity. Rather, it is a personal expression of the extent of my own scientific indebtedness to Charney.

3.1. *Theories of stationary waves in baroclinic atmospheres*

The following papers continue Charney's efforts to calculate the stratospheric response to stationary Rossby waves forced in the troposphere: Dickinson (1969a),

⁶ It might even be argued that Rossby waves are implicit in Laplace's Tidal Equation which dates back to Laplace (1799).

Matsuno (1970), and Schoeberl and Geller (1977). The same methodology is increasingly used to calculate the stationary wave response in the troposphere to the topographic and thermal forcing. Among the relevant papers here are Hoskins and Karoly (1981), Simmons (1982), Jacqmin and Lindzen (1985), Lin (1982), and Rong-Hui and Gambo (1982a,b). The current popularity of this field stems from the recognition that stationary waves form a very large part of the total eddy field in the troposphere and are central to interannual variability of climate and its potential predictability.

3.2. Sudden stratospheric warmings

As has already been mentioned, we currently believe sudden warmings arise from the interaction of vertically propagating stationary Rossby waves with the zonally averaged mean flow. Some papers in which this theory is developed are Matsuno (1971), Holton (1976), and Schoeberl and Strobel (1980). The matter was recently reviewed by McIntyre (1982).

3.3. Noninteraction theorems

As already mentioned above, in Dickinson (1969a) the work of CD on wave-mean flow noninteraction was greatly streamlined, extended, and clarified. Dickinson showed that noninteraction could be violated if there were a transport of potential vorticity, and that this could occur for Rossby waves only in the presence of critical surfaces or dissipation or both.

In recent years the Charney–Drazin noninteraction theorem has been rederived with additional generality and rigor which, in particular, allowed application of the theorem in equatorial regions. Additional attention has also been given to the specific conditions under which the theorem is violated and important interactions take place (damping, diabatic heating, transiency, and critical surfaces are among the factors whose presence lead to the breakdown of noninteraction). Among the papers dealing with these matters are Boyd (1976), Andrews and McIntyre (1978), Uryu (1980), and Lindzen (1973). Several attempts have been made to extend the Charney–Drazin Theorem to nonzonally averaged basic states. Among these is Plumb (1986).

3.4. Vertically propagating equatorial waves

The connection of this area to Charney and Drazin is not generally recognized. My own work in this area, however, was significantly motivated by a desire to find ways in which the constraints on vertical propagation described by Charney and Drazin could be overcome. One way was to move to the equator where f , the Coriolis parameter, goes to zero. Among the relevant papers in this area are Lindzen (1967), Lindzen and Matsuno (1968), Holton and Lindzen (1968), Lindzen (1971), Boyd (1977), and Plumb and Bell (1982a). The interaction of the equatorial waves with the mean flow is currently believed to cause the quasi-biennial oscillation of the tropical stratosphere. This mechanism is described in Lindzen and Holton (1968), Holton and Lindzen (1972), and Plumb and Bell (1982b).

3.5. Baroclinic instability

Lindzen, Farrell, and Tung (1980) have analyzed baroclinic instability in terms of the interaction of vertically propagating Rossby waves with the mean flow. As we have seen in the preceding section, this view was in some measure anticipated by CD.

One could continue at much greater length, but the above summary will have to suffice for this essay. Even a more thorough study of the work which followed from Charney and Drazin would not alter the curious fact that Charney's name is notably absent among those who pursued this subject. Charney even reverted to barotropic and two-level models (Charney and Devore (1979) and Charney and Straus (1980), for example) despite the fact that such models specifically suppressed vertical propagation. Charney, of course, understood better than most how complicated the atmosphere is. His own work frequently showed that much of the complexity was unavoidable. Charney and Drazin is a good example. Nevertheless, Charney has that remarkable combination of naive romanticism and courage to seek answers to nature's questions in simple models even when he was acutely aware that simplicity, itself, was impossible.

References

- Andrews, D. G., and M. E. McIntyre, 1978: Generalized Eliassen-Palm and Charney-Drazin theorems for waves on axisymmetric mean flows in compressible atmospheres. *J. Atmos. Sci.*, **35**, 175-185.
- Booker, J. R., and F. P. Bretherton, 1967: The critical layer for internal gravity waves in a shear flow. *J. Fluid Mech.*, **27**, 513-539.
- Boyd, J. P., 1976: The noninteraction of waves with the zonally averaged flow on a spherical earth and the interrelationships of eddy fluxes of energy, heat and momentum. *J. Atmos. Sci.*, **33**, 2285-2291.
- , 1978: The effects of latitudinal shear on equatorial waves. Parts I and II. *J. Atmos. Sci.*, **35**, 2236-2267.
- Charney, J. G., 1947: The dynamics of long waves in a baroclinic westerly current. *J. Meteor.*, **4**, 135-162.
- , 1948: On the scale of atmospheric motions. *Geophys. Pub.*, **17**(2), 17 pp.
- , 1949: On a physical basis for numerical prediction of large-scale motions in the atmosphere. *J. Meteor.*, **6**, 371-385.
- , and J. G. deVore, 1979: Multiple flow equilibria in the atmosphere and blocking. *J. Atmos. Sci.*, **36**, 1205-1216.
- , and P. G. Drazin, 1961: Propagation of planetary-scale disturbances from the lower into the upper atmosphere. *J. Geophys. Res.*, **66**, 83-109.
- , and A. Eliassen, 1964: On the growth of the hurricane depression. *J. Atmos. Sci.*, **21**, 68-75.
- , and M. E. Stern, 1962: On the stability of internal baroclinic jets in a rotating atmosphere. *J. Atmos. Sci.*, **19**, 159-172.
- , and D. M. Straus, 1980: Form drag instability, multiple equilibria and propagating planetary waves in baroclinic, orographically forced, planetary wave systems. *J. Atmos. Sci.*, **37**, 1157-1176.
- Dickinson, R. E., 1968: Planetary Rossby waves propagating vertically through weak westerly wind wave guides. *J. Atmos. Sci.*, **25**, 984-1002.
- , 1969a: Propagators of atmospheric motions, 1 and 2. *Rev. Geophys.*, **7**, 483-538.
- , 1969b: Vertical propagation of planetary Rossby waves through an atmosphere with Newtonian cooling. *J. Geophys. Res.*, **74**, 929-938.
- , 1969c: The theory of planetary wave-zonal flow interaction. *J. Atmos. Sci.*, **26**, 73-81.

- Eliassen, A., and E. Palm, 1961: On the transfer of energy in stationary mountain waves. *Geophys. Pub.*, **22**(3), 23 pp.
- Hines, C. O., 1959: An interpretation of certain ionospheric motions in terms of atmospheric waves. *J. Geophys. Res.*, **64**, 2210–2211.
- , 1960: Internal atmospheric gravity waves at ionospheric heights. *Can. J. Phys.*, **38**, 1441–1481.
- Holton, J. R., 1976: A semi-spectral numerical model for wave-mean flow interactions in the stratosphere: applications to sudden stratospheric warmings. *J. Atmos. Sci.*, **33**, 1639–1649.
- , and R. S. Lindzen, 1968: A note on “Kelvin” waves in the atmosphere. *Mon. Wea. Rev.*, **96**, 385–386.
- , and —, 1972: An updated theory for the quasi-biennial cycle of the tropical stratosphere. *J. Atmos. Sci.*, **29**, 1076–1080.
- Hoskins, B. J., and D. J. Karoly, 1981: The steady linear response of a spherical atmosphere to thermal and orographic forcing. *J. Atmos. Sci.*, **38**, 1179–1196.
- Jacqmin, D., and R. S. Lindzen, 1985: The causation and sensitivity of the northern winter planetary waves. *J. Atmos. Sci.*, **42**, 724–745.
- Laplace, P. S., 1799: *Mécanique Céleste*, Paris.
- Lin, B., 1982: The behavior of winter stationary planetary waves forced by topography and diabatic heating. *J. Atmos. Sci.*, **39**, 1206–1226.
- Lindzen, R. S., 1967: Planetary waves on beta-planes. *Mon. Wea. Rev.*, **94**, 441–451.
- , 1971: Equatorial planetary waves in shear: Part I. *J. Atmos. Sci.*, **28**, 609–622.
- , 1973: Wave-mean flow interactions in the upper atmosphere. *Boundary Layer Meteor.*, **4**, 327–343.
- , B. Farrell and K. K. Tung, 1980: The concept of wave over-reflection and its application to baroclinic instability. *J. Atmos. Sci.*, **37**, 44–63.
- , and J. R. Holton, 1968: A theory of the quasi-biennial oscillation. *J. Atmos. Sci.*, **25**, 1095–1107.
- , and T. Matsuno, 1968: On the nature of large-scale wave disturbances in the equatorial lower stratosphere. *J. Meteor. Soc. Japan*, **46**, 215–221.
- , and A. J. Rosenthal, 1981: A WKB asymptotic analysis of baroclinic instability. *J. Atmos. Sci.*, **38**, 619–629.
- , and M. R. Schoeberl, 1982: A note on the limits of Rossby wave amplitudes. *J. Atmos. Sci.*, **39**, 1171–1174.
- Margules, M., 1893: Luftbewegungen in einer rotierenden Sphäroidschale (II. Theil). Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften, Wien, Mathematisch-Naturwissenschaftlichen Classe (IIa), **102**, 11–56.
- Matsuno, T., 1970: Vertical propagation of stationary planetary waves in the winter Northern Hemisphere. *J. Atmos. Sci.*, **27**, 871–883.
- , 1971: A dynamical model of the stratospheric warming. *J. Atmos. Sci.*, **28**, 1479–1494.
- McIntyre, M. E., 1982: How well do we understand the dynamics of stratospheric warmings? *J. Meteor. Soc. Japan*, **60**, 37–65.
- Ooyama, K., 1958: On the vertical propagation of a disturbance through the mesosphere. Unpublished report, Department of Meteorology and Oceanography, New York Univ.
- Palmer, T. N., 1981: Aspects of stratospheric sudden warmings studied from a transformed Eulerian-mean viewpoint. *J. Geophys. Res.*, **86**, 9679–9687.
- Pedlosky, J., 1984: The equations for geostrophic motion in the ocean. *J. Phys. Oceanogr.*, **14**, 448–455.
- Pitteway, M. L., and C. O. Hines, 1965: The reflection and ducting of atmospheric acoustic-gravity waves. *Can. J. Phys.*, **43**, 2222–2243.
- Plumb, R. A., 1986: Three-dimensional propagation of transient quasi-geostrophic eddies and its relationship with the eddy forcing of the time mean flow. *J. Atmos. Sci.*, **43**, 1657–1678.
- , and R. C. Bell, 1982a: Equatorial waves in steady zonal shear flow. *Quart. J. Roy. Meteor. Soc.*, **108**, 313–334.
- , and —, 1982b: A model of the quasi-biennial oscillation on an equatorial beta-plane. *Quart. J. Roy. Meteor. Soc.*, **108**, 335–352.

- Rong-hui, H., and K. Gambo, 1982: The response of a hemispheric multi-level model atmosphere to forcing by topography and stationary heat sources. (I and II.) *J. Meteor. Soc. Japan*, **60**, 78–108.
- Schatzman, E., and P. Souffrin, 1967: Waves in the solar atmosphere. *Ann. Rev. Astron. Astrophys.*, **5**, 67–84.
- Scherhag, R., 1951: Die explosionsartigen stratosphärener Warmungen des Spätswinters 1951–1952. *Ber. Deut. Wetterd.*, **6**, 51–63.
- Schoeberl, M. R., and M. Geller, 1977: A calculation of the structure of stationary planetary waves in winter. *J. Atmos. Sci.*, **34**, 1235–1255.
- Simmons, A., 1982: The forcing of stationary wave motion by tropical diabatic heating. *Quart. J. Roy. Meteor. Soc.*, **108**, 503–534.
- Smagorinsky, J., 1953: The dynamical influence of large-scale heat sources and sinks in the quasi-stationary mean motions of the atmosphere. *Quart. J. Roy. Meteor. Soc.*, **79**, 342–366.
- Uryu, M., 1980: Acceleration of mean zonal flows by planetary waves. *Pure Appl. Geophys.*, **118**, 661–693.