On the Development of the Theory of the QBO

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Abstract

Recollections of the discovery of the quasi-biennial oscillation (QBO) of the equatorial stratosphere, and of the development of our present theoretical understanding of this phenomenon are presented.

1. Introduction

The following article consists of my recollections (supported where possible) of the development of the current theory of the quasi-biennial oscillation (QBO) of the tropical stratosphere. As the theory approaches the age of twenty, it seems advisable to set down these recollections before my own memory becomes less certain. Insofar as these recollections are personal, I will include a description of my own introduction to the topic and some discussion of false starts. It is common today to view the explanation of the QBO as an obvious and inevitable application of wave–mean-flow theory. Historically, this ignores the fact that tropical stratospheric waves were unknown in the mid 1960s, and wave–mean-flow theory as now used was also unknown. The following quote from Lorenz (1967, p. 10) indicates the accepted view at that time:[1]

> "Indeed, we are continually encountering new features whose existence we had not anticipated from years of familiarity with the governing laws. One of the more spectacular of these is the recently discovered 26-month or quasi-biennial oscillation, whose outstanding feature is the appearance of persistent easterly and westerly winds in alternate years, in low latitudes in the stratosphere. There now exists an extensive literature on the subject (see Reed, 1965), but we are still awaiting a satisfactory explanation, which is not surprising when we recall that even the trade winds and the prevailing westerlies at sea-level are not completely explained."

2. First exposure

The picture of the zonal flow of the tropical stratosphere commonly held until 1961 is described in some detail by Panofsky (1961). (Ironically, this review was published substantially after the QBO was discovered.) Briefly, it was held that the circulation consisted in a broad belt of easterlies (Krakatoa easterlies) in which was embedded a somewhat irregular belt of westerlies (Berson westerlies). That this was actually a downward propagating, fairly regular oscillation of easterlies and westerlies with an average period of 26 months was independently discovered by Reed (1960), and Veryard and Ebdon (1961). These articles and others by Angell and Korshover (1962), Belmont and Dartt (1962), and still others helped expose many of us to this phenomenon.[2] I first heard the phenomenon from Richard Goody (my Ph.D. adviser) at a seminar at Harvard University in 1961. Goody then believed that the key to the phenomenon lay in the fact that the photochemical relaxation time for ozone at 25 km in the tropics was also about 26 months. He suggested that I look into the matter for my thesis. Then as now, I had difficulty seeing why there should be any connection between a photochemical relaxation time and an oscillatory period, however, other extant suggestions (gestation period of elephants, fifth harmonic of the sunspot cycle) seemed even less plausible. Moreover, since the problem I was then working on (Malkus' theory of turbulent Benard convection) seemed to be going nowhere, I welcomed the suggestion.[3] Sometime in 1962, I began to parameterize the radiative processes and photochemical processes of the stratosphere in order to study how they would interact with the "dynamics." These days, such studies are fairly central to programs dealing with the stratosphere (or more popularly, the "middle" atmosphere), but they were unique in 1962. The work led to a thesis in 1964 and to a series of publications in the *Journal of Atmospheric Sciences* (Lindzen and Goody, 1965; Lindzen, 1966a,b,c). These papers showed how ozone photochemistry could accelerate radiative cooling, and how radiative damping could destabilize fluids with respect to both inertial in-

[1] It is assumed that the reader of these notes is already familiar with the morphology of the QBO. If not, an adequate description may be found in Holton (1975), although Reed (1965) is still a pretty good reference.

[2] Jim Holton has brought to my attention the fact that what might be considered a depiction of the QBO appeared in 1959 (*Meteor. Mag.*, **88**, 113–119). The article in question was a transcript of a U.K. Meteorological Office discussion of tropical meteorology. In this discussion, a P. Graystone presented a time-height section of zonal wind at Christmas Island for the period October 1956 to August 1958 (23 months) and the height range 55 000 ft. to 100 000 ft. With hindsight, one might claim to see clear evidence of the QBO, however, its identification with a 23-month record was clearly impossible. Graystone, himself, only remarks on the absence of any clear annual cycle, and on the presence of strong shears. It seems highly unlikely that Graystone suspected the presence of a QBO. Had he done so, it would have been an easy matter to look at a longer time series. Indeed, the *a priori* likelihood of a 26-month periodicity was so slight that one could not reasonably expect anyone to guess its existence from a 23-month record. Discovery must require some identification on the part of the discoverer of what it is that was discovered. Under the circumstances, there can be no doubt that Reed, and Veryard and Ebdon all deserve credit for the discovery of the QBO.

[3] Malkus' (1954) theory of turbulent convection, which emphasized eddy–mean-flow interactions, probably helped condition my eventual thinking on the QBO.

stability and baroclinic instability. Unfortunately, the part dealing with the QBO (Lindzen, 1966a) was largely a failure. This paper showed that radiation and photochemistry could contribute (albeit ineffectively) to the downward propagation of long-period oscillations in the zonal wind, but in no way accounted for the origin or period of the oscillation.

After finishing my Ph.D. at Harvard, I went off to a brief, pleasurable, and educational postdoctoral position with Dick Reed at the University of Washington (September 1964–March 1965) followed by a North Atlantic Treaty Organization Postdoctoral Fellowship at the University of Oslo with Arnt Eliassen (also pleasurable, educational—and too brief). Inspired by Reed's observational analyses of tides in the middle atmosphere, I began my own research on atmospheric tides at this time. However, the QBO was never far from my thoughts, and eventually what I learned in tidal theory also proved useful in understanding the QBO. It became increasingly clear to me at this time that the generation of westerly jets at the equator might prove to be almost as hard to explain as the period, but at the time, I had almost no suggestion for either. Though I was not personally aware of it, the question of the momentum budget of the stratosphere was apparently a matter of concern even before the discovery of the QBO. It was discussed at a meeting in Moscow in June of 1965 that I attended (Monin, 1965)—though without any suggestions emerging.

3. Early progress

There were three independent areas of inquiry being pursued in the mid sixties that jointly led to the theory of the QBO:

a) The observational and theoretical study of equatorial waves. The theoretical study of this topic was facilitated by and even characterized by the use of the equatorial β plane.
b) The theoretical study of the behavior of mountain waves at critical levels where the mean flow speed went to zero.
c) The semi-empirical study of the momentum budget of the QBO.

It was my good fortune that I was involved in and/or actively interested in each of the above. In retrospect, only items (a) and (b) were really essential, but item (c) helped put things in perspective. In this section I will briefly review the efforts in each of these three areas.

a. Equatorial waves

A clear discussion of this topic is by no means easy since various individuals working on the topic were differently motivated. For example, Taroh Matsuno and I were independently studying wave solutions on the equatorial β plane. To the best of my knowledge, Matsuno (1966) was primarily motivated by the desire to see what happened to the quasi-geostrophic approximation at the equator. My own motivation was somewhat diffuse as is evident from Lindzen (1967). My familiarity with the equatorial β plane began through personal contact with the oceanographers, Bob Blandford, Merle Hendershott, and Dennis Moore at Harvard (Blandford, 1966; Hendershott, 1964; Moore, 1968).

When I began working on mesospheric tides at Seattle, it seemed reasonable to begin by also using the equatorial β plane. I soon discovered that the equatorial β plane as commonly used (namely, an unbounded domain without polar boundaries) was inadequate for tidal calculations, and one point of Lindzen (1967) was to explain this inadequacy. More importantly, my work on tides had made it evident to me that the Charney-Drazin (1961) conditions (which were based on quasi-geostrophic theory) for the inhibition of the vertical propagation of planetary-scale waves were inappropriate in the neighborhood of the equator. In Lindzen (1967), it was noted that waves of any period and zonal direction could propagate vertically as internal gravity waves equatorward of their critical latitudes (where the coriolis parameter equalled the waves' doppler-shifted frequency). This finding was central to the subsequent development of the theory of the QBO.

Matsuno (1966) was first brought to my attention by Warren Washington at the National Center for Atmospheric Research as I was readying my paper for publication. (Washington had picked it up when he attended a numerical-weather-prediction conference in Tokyo.) While the papers had much in common, it was evident that Matsuno was unconcerned with vertical propagation (He was using the shallow-water equations.), which was one of my central concerns. However, Matsuno was aware of the observational discovery of five-day easterly waves in the equatorial stratosphere by Maruyama and Yanai (Maruyama, 1967; Yanai and Maruyama, 1966), and he related these observed waves to equatorial β plane solutions by associating a shallow-water depth with the vertical scale of the observed waves. It was evident to me that these were upward-propagating waves of the sort envisaged in my paper—a point that Jule Charney and I discussed at length at University of California at Los Angeles (UCLA) during the spring semester of 1967 (I was at that time giving a course on upper atmospheric dynamics at UCLA; Charney was visiting UCLA at the same time, and, indeed, I had come there largely at his urging.). In order to make this point clearer I prepared a short note that was submitted to the *Journal of the Meteorological Society of Japan.* Matsuno was the anonymous referee of that note, and his thoughtful remarks caused me to substantially expand the note. In resubmitting the (then) article, I asked the editors to ask the referee to share authorship of the paper with me. Matsuno, who turned out to be the referee, agreed and the paper appeared as Lindzen and Matsuno (1968). Matsuno and I did not actually meet each other until a year or two after we coauthored that article.

At about this time, Wallace and Kousky (1968) were discovering that at times the equatorial stratosphere contained westerly waves instead of the easterly mixed-gravity-Rossby waves discovered by Maruyama and Yanai. These waves were of much larger zonal extent than the easterly waves and their periods were longer (around 12 days). Jim Holton and I immediately identified these waves as equatorial Kelvin waves (Holton and Lindzen, 1968). Oddly enough, however, I had developed the theory of the QBO before learning of

Wallace and Kousky's discovery. I will, therefore, delay further discussion of this work until section 4.

b. Mountain waves and critical levels

That mountain lee waves were, in large part, vertically propagating internal gravity waves was evident from the work of Lyra (1943); Queney (1947); and Eliassen and Palm (1961) among others. However, the question of what became of such waves at critical levels (in the case of stationary lee waves, where the mean flow passed through zero) seemed shrouded in mystery. The question of what happened to linear waves at a singular point in the linear wave equation was generally avoided. Hines and Reddy (1967) attempted to deal with the question using multiple-layer models and concluded that gravity waves would be reflected, but the model was clearly suspect and eventually proved incorrect. I first was introduced to the problem by Larry Larsen (now an oceanographer at the University of Washington). Larry and I were postdocs at the University of Oslo at the same time, and Larry was working on the critical-level problems. I became sufficiently interested in the problem to keep up with the literature to some extent, and when Booker and Bretherton (1967) appeared, I read it with great interest. Their treatment of the problem made it almost disappointingly simple. At least within the context of linear theory, they unambiguously demonstrated that gravity waves would be absorbed at critical levels (at least for large Richardson Numbers). In retrospect, the notion that any damping, however small, would absorb a wave as its group velocity went to zero seems almost obvious.[4] To be sure, there remained questions about the applicability of linear theory, but Booker and Bretherton clearly demonstrated that there would be readily achieved circumstances when linear theory would be applicable. What was particularly exciting to me was that at critical levels, Eliassen and Palm's result that the vertical-momentum flux due to internal gravity waves was nondivergent (and hence could not accelerate the mean flow) was violated. Not only were internal gravity waves effective transporters of momentum, but there was also a mechanism for depositing this momentum in the mean flow. Eliassen and Palm's results did make it clear that the absorbed momentum flux would be such as to accelerate the mean flow toward the phase speed of the absorbed wave. Of course, the waves I was thinking about at that time were the vertically propagating equatorial waves discussed in 3.a., not mountain waves. My first picture of how the QBO might work followed almost immediately, but before discussing this, some diagnostic studies by Wallace and Holton should be mentioned.

c. Momentum budget of the QBO

As I have already noted, my thesis investigated the role of radiation and photochemistry in stratospheric dynamics. Implicit in this study was the notion that the QBO might be thermally forced. Mike Wallace pointed out to me that it was almost impossible to force tropical temperature changes with heat sources (the induced vertical velocity would almost cancel the effect of the heating); momentum sources would be much more effective. Wallace published his arguments in Wallace (1967). Shortly after Wallace and Holton arrived at the University of Washington, they began to set up a numerical model of the QBO. Wallace and Newell (1966) had found some evidence of a 26-month periodicity in the horizontal eddy flux of momentum at 30 km. Holton and Wallace proposed to use the model to see if such a momentum source at 30 km might produce the downward-propagating QBO. They found that their momentum source was too small to produce a realistic QBO even at 30 km. Using their model in a diagnostic mode they next attempted to determine the needed momentum source. It is my recollection that they were disappointed to discover that no momentum source fixed in space could produce a downward-propagating QBO. Instead they found that the momentum source would have to follow the QBO downward as it propagated (Wallace and Holton, 1968). This situation is described in section 1 of Lindzen and Holton (1968) (subsequently referred to as LH). I was delighted to hear this result since it fit in perfectly with my notion that critical-level absorption was the momentum source. Afterall, critical-level absorption would alter the mean flow and hence the critical level would descend and so would the momentum source due to critical-level absorption. I soon discovered that things were not quite so simple as this—but they were almost as simple.

4. The basic solution

Soon after reading Booker and Bretherton (1967), I began parameterizing the critical-level interaction of vertically propagating waves with the mean flow in order to model the time evolution of the mean flow. My initial aim was simply to find some way of showing that critical-level interactions could lead to downward-propagating oscillations in the mean flow. The initial results of this search are in sections 3 and 4 of LH. In this initial work, I restricted myself to the "pure" critical-level interaction in the limit of vanishing dissipation. It, therefore, appeared that a single gravity wave with unique phase speed would be difficult to deal with since it involved "infinite" acceleration at a single level (ignoring the line broadening due to the accelerating flow). On the other hand, a collection of gravity waves with a continuous distribution of phase speeds worked beautifully insofar as it immediately led to a simple, nonlinear first-order wave equation for the mean flow.[5] The downward phase speed at any level was the spectral density of the waves having critical levels at that level. An example of the flow evolution arising from such a situation is shown in Fig. 1a. There is a clear downward propagation of the shear zone leading to an eventual flow discontinuity (or "shock" as Dunkerton (1981) referred to it) at the lowest level at which the waves had a critical level.

[4] This simple explanation makes it clear that critical-level absorption is just a special case of absorption due to dissipative mechanisms. However, at the time, this was not so clear, and for at least a year or two critical-level absorption and dissipative absorption tended to be considered distinct mechanisms (viz. Lindzen [1973]). This was also the situation with respect to the critical-surface absorption of Rossby waves (Dickinson, 1969).

[5] Equation 14 in LH; note, however, that eq. 14 has a sign error: the right-hand side should have a plus sign.

So far, I had downward propagation—but no oscillation. In order to obtain an oscillation, I noticed that as the shear zone propagated downward, it sheltered the region above from the further action of the gravity waves. Thus, for example, if there were any restoring mechanism acting on the basic flow, it would be free to reestablish the original flow above the shear zone. An example of such an evolution is shown in Fig. 1b. It seemed intuitively plausible to me that the infinitesimally thin jet at the bottom of Fig. 1b would break down, allowing the gravity waves to again propagate upwards and repeat the whole development; i.e., the system would oscillate. The final question at this stage was how might this function in the equatorial stratosphere? As it turned out, Dick Reed (1966) had recently discovered that the zonal wind in the equatorial stratosphere above 30 km underwent a semiannual rather than a quasi-biennial oscillation. Such an oscillation could easily replace the hypothetical restoring mechanism providing alternating positive and negative shear zones, which could be "propagated" downward by the upward-propagating shorter-period waves. All that was needed was a spectrum of waves with both westerly and easterly phase speeds. At this point, the only short-period waves that had been discovered in the equatorial stratosphere were the easterly waves described by Maruyama and Yanai. Nevertheless, assuming the existence of westerly waves, I was able to sketch the schematic of how the QBO might work shown in Fig. 2, in the very early summer of 1967. (A discussion of Fig. 2 is given in section 5 of LH) I had been invited to spend August of that summer at the University of Washington, and I was naturally eager to see how the model developed by Holton and Wallace would respond to my wave forcing. In particular, I wanted to see whether the schematic picture would actually work. I called Holton and Wallace to inform them that I "knew" how the QBO worked. I no longer recall whether I gave them any details over the phone or whether I waited until I arrived in Seattle. In any event Wallace was particularly enthusiastic about the theory because he and Kousky had just discovered the required westerly waves in the lower stratosphere. A recapitulation of my theory was inserted into the paper in which they described their westerly (Kelvin) waves (Wallace and Kousky, 1968). This was, in fact, the first description of my theory of the QBO to appear in print.

Jim Holton did insert my mechanism into his model and it did indeed confirm the schematic. It should be emphasized that Holton's model was a dynamically consistent nonlinear two-dimensional (latitude and altitude) model with rotation. The wave forcing not only accelerated the mean flow but also forced a meridional circulation that maintained balance. Nonlinear advections served to enhance equatorial westerlies and confine the latitude extent of the QBO. (These results are described in section 7 of LH.) However, at the equator, the results were not significantly different from those obtained with a one-dimensional (altitude) model. In subsequent work we, therefore, stuck to the simpler one-dimensional models to study the evolution of the mean flow.

LH was hardly the last word in the theory of the QBO. However, I think it is fair to say that it established the fundamental picture of the QBO as forced by upward-propagating waves where the rate of downward propagation of the QBO is determined primarily by the intensity of the upward-propagating waves while the amplitude of the QBO is largely determined by the phase speeds of the upward-propagating waves. The QBO was possibly the first example in meteorology of an important phenomenon resulting from wave–mean-flow interaction; it remains one of the best examples.

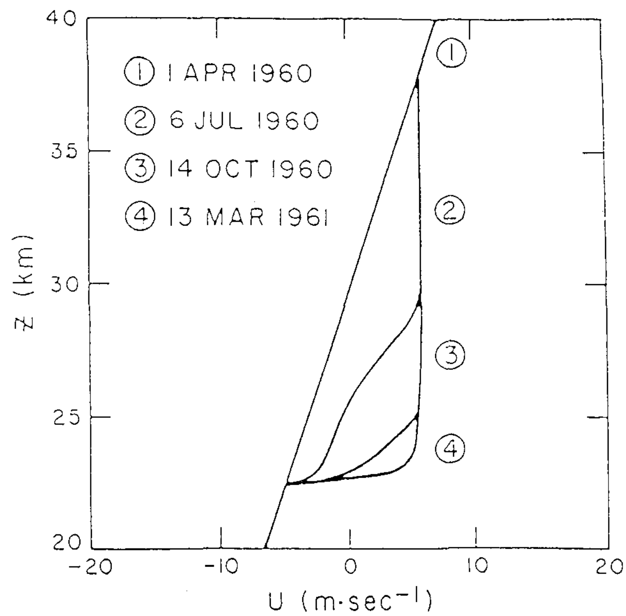

FIG. 1a. The "downward propagation" of a shear zone due to the critical level interaction of a spectrum of gravity waves with the mean flow.

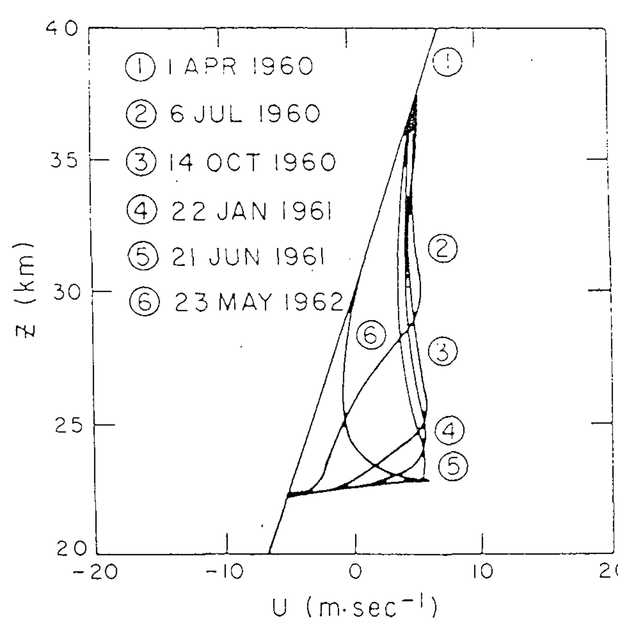

FIG. 1b. The same as in (a), but in the presence of a linear restoring mechanism for the mean flow. Both figures are taken from LH.

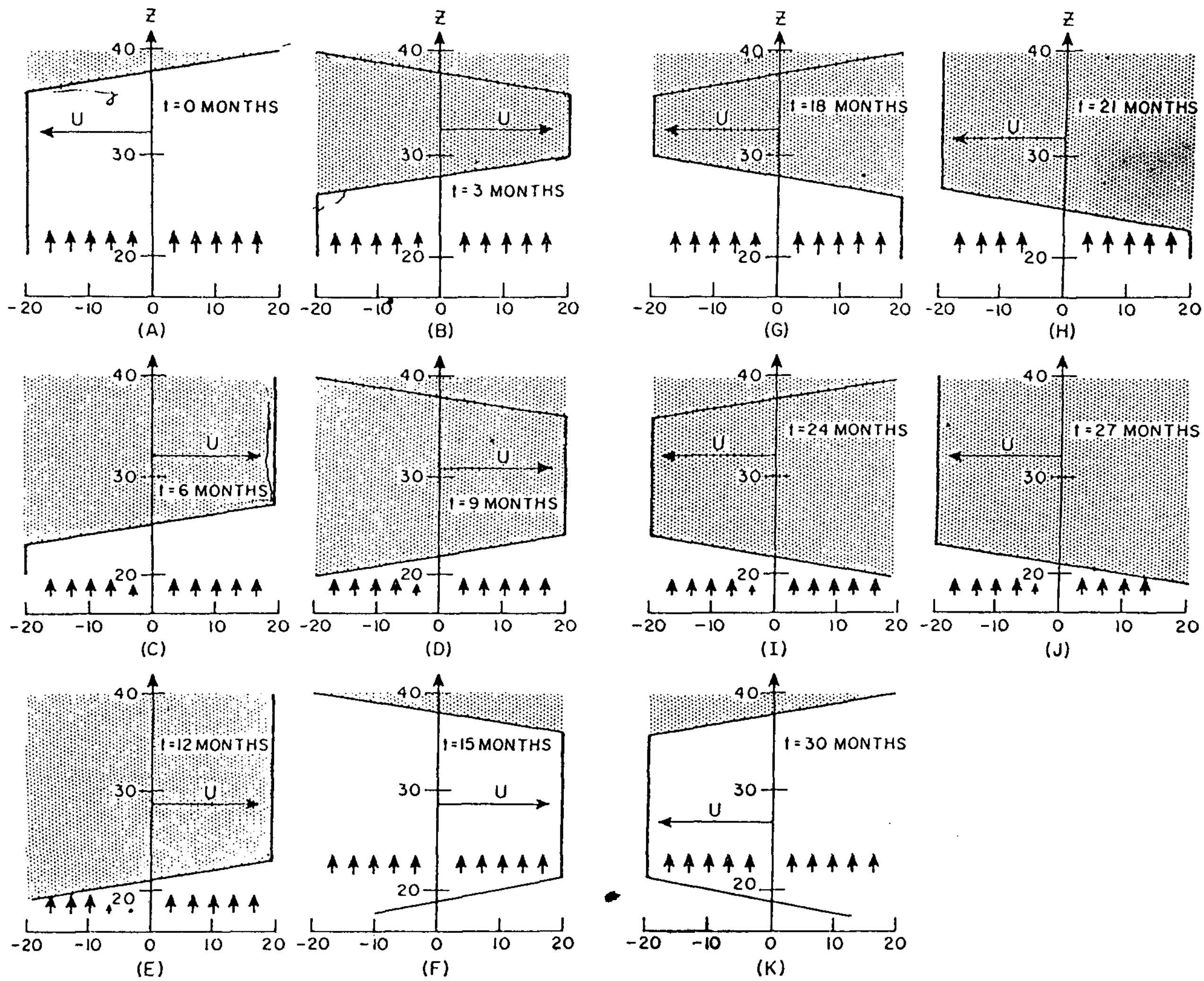

FIG. 2. Schematic illustration of how the QBO might work (taken from LH).

5. Some loose ends

The above theory had more than a few loose ends—a number of which were mentioned in LH. The most important loose ends dealt with the nature of the upward-propagating waves. LH were extremely cavalier on this matter. Most of the results in LH were based on simple internal gravity waves. It was recognized that the easterly propagating mixed–gravity-Rossby waves and the westerly propagating Kelvin waves were important (probably the most important) components of the upward-propagating–wave picture and that these waves were not exactly simple internal gravity waves, but no explicit account was taken of this. It was clear that the investigation of the nature of these waves, their origin and their behavior in the presence of mean shear were important next tasks. Logically, I suppose, they should have been done before any theory of the QBO was developed, but that was not how things happened to go.

The nature of these waves was studied in Lindzen and Matsuno (1968) and in Holton and Lindzen (1968). A disturbing problem was uncovered in Lindzen and Matsuno (1968): namely, for the upward-propagating mixed gravity-Rossby waves, the momentum flux, $\rho_0\overline{u'w'}$, was positive (Maruyama, 1968, also noticed this). This seemed to suggest that the easterly propagating mixed–gravity-Rossby waves were carrying westerly rather than easterly momentum upwards. Jones' (1967) work on internal gravity waves in a rotating system, however, had shown that the complete expression for the upward momentum flux was $\rho_0(\overline{u'w'} - f\overline{\eta' w'})$ where f is the coriolis parameter and η' is the northward displacement of a fluid element associated with the wave. For the westerly propagating Kelvin wave, $\eta' = 0$, and the neglected term is of no consequence, however, this term for the mixed–gravity-Rossby wave is larger than the first term and has the opposite sign (Lindzen (1970)). Thus, to our relief, easterly propagating mixed–gravity-Rossby waves had an easterly momentum flux afterall.

The study of the behavior of Kelvin waves and mixed–gravity-Rossby waves in shear flows proved rather difficult at first. Though Holton and I approached this problem independently, both of us ended up with a boundary value problem, which when "finite differenced" led to the solution of a large inhomogeneous linear system of equations. Prior to the 1970s, the standard approach to the solution of such large linear systems was via relaxation methods. Unfortunately, neither Holton nor I could get any relaxation methods to converge for our problem; similar problems were being encountered at this time by Matsuno, Kuo, and others in connection with other physical problems. By this time (1968), I had moved to the University of Chicago, where Kuo and I discussed this problem at length. Observing that the linear operator was a banded tridiagonal matrix, we noted that a matrix form of the "up-down-sweep" algorithm for Gaussian elimination that was commonly used for second-order ordinary differential equations (where the relevant operator was a simple tridiagonal matrix; viz Richtmyer, 1957) would work—providing a direct rather than an iterative solution. We published this algorithm in Lindzen and Kuo (1969).[6] The direct algorithm worked on all the problems we were interested in. I was able to immediately calculate the behavior of equatorial waves in shear (Lindzen, 1970). I also communicated the algorithm to Holton who in turn shared it with Matsuno. Holton (1970) produced his calculation of the behavior of equatorial waves in shear using the Lindzen-Kuo algorithm and Matsuno (1970) used the algorithm to calculate stationary planetary waves in the stratosphere.

The results obtained by Holton and I certainly confirmed critical-level absorption for equatorial waves; however, the numerical solutions seemed too clumsy for direct inclusion in a model of the QBO.[7] Instead, I attempted an analytic solution of the problem that I felt would lend itself more readily to a parameterization of the wave-mean-flow interaction. It was, in fact, clear from the numerical solutions that the wave structure (meridional and vertical) depended fairly simply on the local Doppler-shifted frequency; this suggested that a two-dimensional variant of Wentzel-Kramers-Brillouin (WKB) might be possible. With some difficulty, I developed such a method, and it successfully duplicated the earlier numerical results (Lindzen, 1971, 1973); more importantly, the analytic solutions were very simple, and permitted not only a simple parameterization of the wave-mean-flow interaction but also a more-thoughtful examination of the actual behavior of the equatorial waves.[8] In particular, it became possible to examine the effect of infrared radiative "damping" on the vertical propagation of these waves, and to carefully consider the relation of the calculated waves to observed waves. It was found that the waves were sensitive to infrared damping, and that the damping that led to the best fit to the data was about the value expected *a priori*. Most importantly in connection with the QBO it was observed that in the presence of damping, the momentum flux associated with a single wave was now deposited over a broad region below the critical level—not at the critical level. Quoting from Lindzen (1971):

> "A consideration of the available data for the equatorial stratosphere shows rather unambiguously the existence of waves with phase speeds corresponding to the maximum easterly and westerly quasi-biennial velocities. However, there is virtually no evidence for the existence of all intermediate phase speeds. The results in this paper eliminate the need for such intermediate phase speeds; realistic levels of dissipation are sufficient to account for the deposition of mean zonal momentum throughout the region below a critical level. At the same time, levels of maximum easterly and westerly quasi-biennial velocity still act as barriers to the further passage of waves."

This constituted the final major revision of the theory of the QBO. I spent the summer of 1971 back in Seattle, where I convinced Holton to rerun the QBO model with my new parameterization. He agreed to do this and in the autumn of 1971 he sent me the results from a one-dimensional model.[9] The results confirmed that the revised parameterization still led to the QBO. These results appeared in Holton and Lindzen (1972). In looking over the results, it was noticed that although the semiannual oscillation still affected the phase of the QBO, it was no longer necessary. Quoting from Holton and Lindzen (1972):

> "The mesospheric semiannual oscillation, while important, is no longer absolutely essential to the overall theory."

Holton was uneasy with this statement since he had not run this case. Spurred by an inquiry from Sig Fritz, Holton did finally run this case and confirm the contention. We also discovered that for the realistic simulation of the QBO, the fact that the radiative relaxation time decreases with height had to be included. This feature was later studied in considerably greater detail by Hamilton (1981).

6. Subsequent history

Following Holton and Lindzen (1972), I concluded that there was little point in refining the theory of the QBO until we had a better handle on the nature and generation of the upward-propagating waves, as well as some observational details of the wave-mean-flow interaction. An observationally based attempt in this direction is described in Lindzen and Tsay (1974). That was my last direct contribution to the study of the QBO. Although my interest in the subject has remained, my familiarity with subsequent developments has been more limited. However, in this section I will attempt to describe at least some of the steady flow of interesting new contributions over the last ten years or so.

A number of attempts have been made to explain the origin of the upward-propagating equatorial waves. Holton

[6] At the time we were not aware of other attempts to solve such systems directly. However, reviewers of our paper noted that such approaches had already been considered. The situation was accurately described in the text by Isaacson and Keller (1966). In describing the numerical solution of elliptic-boundary-value problems, they noted that direct solution was possible, but impractical; they therefore restricted their discussion exclusively to relaxation methods. The point was that direct methods required a great deal of computer memory while relaxation methods did not; however, for the occasional calculations we had in mind, this was not such a great problem.

[7] Plumb and Bell, 1982b, did take such an approach in their simulation of the QBO.

[8] Boyd (1978) subsequently extended the WKB solutions to include meridional as well as vertical shear.

[9] Jim Holton, in a personal communication, states that he does not recall the reluctance that I here attribute to him. It may, indeed, be a misperception on my part. I had first asked Jim to redo the calculations at a meeting in Toronto in January 1971, and was rather impatient to see the results. It would be easy enough to confuse my impatience with assumed reluctance on Jim's part.

(1972) showed that pulsations of the major regions of tropical rainfall would lead to the generation of both westerly and easterly waves of roughly the right characteristics. The selection mechanism seemed to involve matching the vertical scale of the cumulus heating with half-the-vertical-wavelength of the upward-propagating waves. A similar approach has recently been taken by Salby and Garcia (1987). There is some problem with this approach since the vertical wavelengths of the observed mixed-gravity-Rossby waves and Kelvin waves are distinctly different. This difficulty may turn out to be fairly minor; in any event, the selection mechanism is not very sharp. Hayashi (1974) has shown that the equatorial waves are also generated in the Geophysical Fluid Dynamics Laboratory's general-circulation model.[10]

Plumb (1977) produced a simplified version of the model described in Holton and Lindzen (1972). Plumb formulated his model for a nonrotating channel and a boussinesq fluid. Damping was taken as due to diffusion, and the westerly Kelvin and easterly mixed-gravity-Rossby waves were replaced by two simple gravity waves. The simplified model had two advantages: namely, it permitted easier analysis of the mechanics of the QBO, and it was more suitable to the experimental simulation of the QBO by Plumb and McEwan (1978). Much of the analysis in Plumb (1977) is almost identical to that in LH (compare Fig. 1 of Plumb (1977) with Fig. 1 here, which is reproduced from LH). Plumb correctly notes that the absorption of a wave at a given level does not depend on the region above that level so that the expression "downward propagation" is technically inappropriate. Plumb also argued that the semiannual oscillation is unnecessary for the QBO, a point already emphasized in Holton and Lindzen (1972). Plumb and McEwan (1978) describes a remarkable simulation of the QBO mechanism in the laboratory—a simulation that unambiguously demonstrated the ability of wave- mean-flow interaction to produce a long-period oscillation in the mean flow. This is arguably the most successful laboratory simulation of any large-scale atmospheric phenomenon. Moreover, as Holton reminded me, a number of individuals were skeptical of the wave–mean-flow model for the generation of the QBO until confronted with this laboratory evidence.

Lindzen and Tsay (1974) had noted that in the absence of mechanical dissipation, the vertical momentum flux due to the mixed-gravity-Rossby wave disappeared at the equator. However, Andrews and McIntyre (1976) showed that the inclusion of a small amount of mechanical dissipation can cause the flux to actually maximize at the equator, while Holton (1979) showed that wave transiency could simulate the same effect. The role of wave transiency in wave–mean-flow interaction was further explored by Dunkerton (1981a,b). In many ways, transiency mimics damping (Lindzen, 1971a; Boyd, 1976)—though damping is ultimately needed.

Hamilton (1981) further studied the importance of radiative damping varying with height in the realistic simulation of the QBO.

Plumb and Bell (1982a) redid the numerical calculations of the propagation of equatorial waves through reasonable two-dimensional distributions of mean flow. Their calculations basically followed those in Lindzen (1970), though they more-carefully examined various parametric dependences and paid more attention to the careful determination of momentum fluxes. Their results were, moreover; in good agreement with the WKB results of Boyd (1976). Plumb and Bell (1982b) then used these numerically calculated equatorial waves to interact with the mean flow in a two-dimensional model of the QBO similar to that in LH.

Most recently, Tanaka and Yoshizawa (1985) have examined the effect of the time variations in the mean flow in broadening the phase-speed "spectrum" of the incident Kelvin and mixed-gravity-Rossby waves. This effect was first noted by Jones and Houghton (1971), and has recently been studied in greater detail by Coy (1983), Dunkerton and Fritts (1984) and Fritts and Dunkerton (1985). Tanaka and Yoshizawa (1985), using a one-dimensional model of the QBO (following Holton and Lindzen, 1972) found that the effect of what they refer to as wave self-acceleration is fairly small for pure gravity waves and Kelvin waves but can be substantial for the mixed-gravity-Rossby waves. This seems to be related to the peculiar behavior of the latitude distribution of momentum flux associated with these waves, and probably needs a full two-dimensional study to be properly evaluated. Tanaka and Yoshizawa make the interesting observation that wave self-acceleration can produce wave phase speeds in excess of those originally excited—and hence lead to larger QBO amplitudes than might have been expected in the absence of this effect.

7. Additional remarks

In considering the development of this subject, I have, for some time, regretted the fact that LH is usually ignored. In fact, the discovery that wave–mean-flow interaction could actually occur, and that it could account for the QBO was very exciting. This sense of real discovery is evident in LH. To deemphasize the discovery relative to the subsequent developments is, it seems to me, a disservice to both the field and to the young scientists entering it. Looking back at the two years leading up to LH, I am struck by the disorganized but rapid development. Basic theory, observational analyses, diagnoses, simulations all were performed in tandem. Publications appeared in an almost-random order—unrelated to what might be regarded as a logical sequence. No special funds were provided, and the data used was in the public domain. The main participants were, for the most part, busily involved in other activities as well. Clearly, this was not work governed by grand plans or international commissions. Discovery, in this instance and in many others, stemmed in large measure from a certain lack of specialization: the unexpected coming together of seemingly disparate ideas from fields like mountain waves, tides, turbulent convection, planetary waves, and even mathematics—not to mention stratospheric physics itself. Such unplanned syntheses are what make meteorological research more like fun than work. It seems a pity to keep this a secret.

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[10] It should be noted that no GCM has ever succeeded in producing a QBO. Poor vertical resolution is believed to be the reason.

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under construction, and building-code assessment). The first draft (or chapter outlines) is available for comment, and a completed report is due by 31 December 1987.

The purpose of the group is to apply new knowledge in wind engineering to reduce wind damage to buildings and structures, and to determine deficiencies in current building codes and construction practices with respect to wind damage with an eye to improvement.

Individuals with information or ideas on wind-damage mitigation should contact Earl Turner, 70 West Country Lane, Collinsville, IL 62234.

NCAR Research Aviation Facility Advisory Panel Meeting

The Advisory Panel for the Research Aviation Facility (RAF) of the National Center for Atmospheric Research (NCAR) will meet in Boulder, Colorado in October 1987 to consider requests and make recommendations for flight-hour support. The deliberations in the October 1987 meeting will concentrate on programs scheduled during the period April 1988 through October 1988. Requests for the long-range Electra aircraft will be considered for the period April 1988 through April 1989 to allow sufficient time to organize joint use of the aircraft among several investigators, thereby making each flight hour as economical as possible.

The RAF Advisory Panel is composed of atmospheric scientists from universities, government agencies, and NCAR. It ordinarily meets twice each year, in April and October, to consider the scheduling of NCAR aircraft. Requests for NCAR flight support for programs within the context of National Science Foundation (NSF) grants should include a copy of the NSF proposal. Those proposals for research grants involving RAF aircraft should be submitted to NSF for review no later than mid-June 1987 (four months prior to the Advisory Panel Meeting). Internal NCAR flight requests and proposals not part of the NSF program should submit sufficient information and justification commensurate with that which would be submitted to NSF, in order that a meaningful comparison with NSF-supported programs can be made.

The NCAR Research Aviation Facility operates three aircraft, a Beechcraft King Air, North American Sabreliner, and Lockheed Electra, in support of field projects in the areas of air chemistry, cloud physics, air motion (including mass flow and turbulent flux measurements), radiation, physical oceanography, air-sea interaction, and other programs within the atmospheric sciences. The Super King Air twin turboprop aircraft is pressurized, has an operating ceiling approximately 10 000 meters above sea level and is approved for both Visual and Instrument Flight Rules and for flight into known light-icing conditions. The NCAR Sabreliner is a low-wing, twin jet aircraft pressurized for high altitude flight. The NCAR Sabreliner is not equipped with a pnematic de-icer boot system and is restricted from operation in known icing conditions. The normal operating ceiling is approximately 14 000 meters above sea level.

The Electra is a large, low-wing, long-range, four-engine turboprop aircraft. The Electra can operate in known icing conditions; however, external instrumentation installations may limit such operations. The operating ceiling of the NCAR Electra is approximately 8 000 meters above sea level.

The RAF aircraft are all equipped to measure various parameters including temperature, pressure, dew point, winds, etc. Also, a large variety of equipment can be specified by users for a particular project, including cloud and hydrometeor particle spectrometers, aerosol spectrometers, shortwave and longwave optical radiometers, and remote radiometric surface temperature instrumentation. The RAF assumes responsibility for installing and maintaining this requested instrumentation. In addition, considerable freedom is permitted in mounting user-supplied instrumentation on RAF's NSF-owned aircraft, and the RAF will assist in the installation of all user-supplied instrumentation to ensure compatibility with existing RAF instrumentation systems and to ensure aircraft safety for normal flight operations and for crash-load specifications.

In order to be considered by the Panel at the October meeting in 1987, requests must be submitted in completed form to the Manager, Research Aviation Facility, NCAR, P.O. Box 3000, Boulder, Colorado 80307, not later than 12 August 1987. The precise dates of the October meeting will be established at the April 1987 meeting of the Advisory Panel. Additional information is available at (303) 497-1036, or through correspondence with the RAF. Interested scientists, who may not have earlier completed their requests, are invited to call the same number after 1 May 1987 to obtain the exact dates of the October 1987 meeting.

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