Remarks on the progress of general circulation studies

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ABSTRACT

A brief sketch of the development of terrestrial atmospheric general circulation studies is given in such fashion as to illuminate the current thinking and research being performed on the subject. The successive stages of progress are examined from the point of view of the search for better formulations of the problem, which would lessen the chance of erroneous closure assumptions. It becomes obvious that according to this criterion the primitive equation solutions represent the best numerical treatment. Stress is placed upon the necessity of observational studies to give us, ideally at least, the most correct picture of the mechanics of the general circulation—a contention amply attested to by the history of the subject. Some suggestions for future theoretical and observational research are included.

Introduction

The material contained herein appears in continuation of the practice of the writer occasionally to review, from a phenomenological point of view, our progress in the study of the mechanics of the atmospheric general circulation and related matters. By this is meant the degree to which we have obtained a true picture of the hydromechanical actions taking place in the real atmosphere. Generally speaking such knowledge is gained through the use of the appropriate principles of physics in combination with proper observational data; this being true whether the approach is theoretical or more directly empirical. As a matter of fact, reasons could be adduced to render acceptable the view that the two approaches merge continuously with each other, through the citing of various specific investigations. It must at the outset be admitted that the progress we are attempting to discuss has no visible termination, since there is no end to the complexities that lie ahead.

In our discussion we shall limit ourselves to brief statements only concerning those things which came to be accepted as common knowledge as of a few years ago (see, e.g., Lorenz, 1967) and fix more attention on a few aspects of our subject which have come to the fore since that time. In a brief statement like the present one, addressed as it is to persons with at least a fair background in meteorology, no account of the fine structure of theory or the intricacies involved in data studies will be entered upon. Our sole concern is with the gross results and with something of the physical principles through which they were gotten and interpreted. In order to concentrate attention we shall, as already implied, focus primarily on the motions of our system and certain others, for otherwise the scope of the subject material would rapidly grow out of bounds.

It may not be out of place to reiterate some views stated by the writer elsewhere, that in general the gross facts about the large meteorological motions are not susceptible to easy explanation. We observe them, we calculate them as solutions of the equations of motion, we note their various properties, but to explain how the interactions of a well-nigh infinite number of particles results in these motions rather than others is no easy matter—simplistic efforts in this direction notwithstanding. We shall comment further on this phase of investigation in a later section. See also Starr (1968).

A retrospective survey

For purposes about to be expounded presently, we may subdivide the history of the development of our subject into certain stages, selected to facilitate discussion of points to be made later. Although an intrinsic interest attaches to the ideas on meteorology of such ancients as Aristotle, who incidentally ascribed the cause of the winds quite correctly to solar heating, we shall not start our survey until the opening up of the scientific era in western Europe, especially the Galileo-Newtonian period.

(1) With the accelerated development of the science of mechanics, it became at the hands of Galileo and Newton a theory of the universe, albeit that these elaborators were conscious of many of its shortcomings. It was therefore natural that the motions of the atmosphere were examined from the viewpoint of mechanics by the close associate of Newton, namely Edmund Halley and later by John Hadley and others. As seen now with our own eyes, these efforts were attempts to portray a fundamental zonally symmetric convection with rising air in the tropics and sinking somewhere farther poleward. These motions were then crudely thought of as being subject to conservation of angular momentum so as to give rise to zonal components of motion. However, since the doctrine of energy came afterwards and hence the science of thermodynamics was not available, the theories were qualitative and nothing like what we might call a theoretical model was available. Nevertheless, a positive contribution was contained in this material, which has persisted to modern times.

(2) During an intervening period extending until about the middle of the nineteenth century no really fundamental new formulations that survive were contributed. Still during this interval many essential tools were prepared. The science of mechanics, especially as applied to fluids was further perfected by such people as Laplace, Lagrange, Euler, Cauchy, Poisson, Airy and Stokes. Rather separately the use of rotating coordinates in mechanics was promoted by Clairaut, Coriolis and later by Ferrel. The science of thermodynamics came into being particularly as a practical matter due to the development of heat engines. On the observational side the discovery and colonization of the New World and related explorations opened the principal oceans to shipping, so that a vast amount of information concerning global wind systems became known.

(3) The period from about 1850 and extending into the twentieth century saw the introduction of mathematical models of the general circulation by such people as Oberbeck, Ferrel and others. The basic action in these was assumed to be axisymmetrical, so that the observed irregularities were assumed to be accidental, in a manner of speaking. The role given to these inequalities was then taken to be unimportant or else looked upon as a gross turbulence feeding on the mean flow.

Two symptoms of trouble appeared for these models. The first was the realization from data and perhaps ground friction theory which latter had made its appearance, that there was a reverse mean meridional circulation in middle latitudes, contrary to simple ideas about convection. Thus in the region of strongest mean solenoid concentration the mean motions were such as to have cold air rise and warm air sink. The other discomforting finding came from synoptic data studies which appeared with the advent of various weather services, after the invention of telegraphic communication. These studies indicated large local appearances of kinetic energy especially in the cyclonic centers. The implication was that a local convection process accounted for these kinetic energy concentrations (an idea investigated by Max Margules in a quantitative fashion) without requiring a source from the global mean circulation.

(4) As we approach modern times, the resolution of our perspective becomes finer. The period from before the first World War and for several years after was marked most by the development of frontal concepts and the introduction of the theory of Bjerknes and others that the observed cyclones are due to the growth of unstable wave disturbances on pre-existing frontal discontinuities. The theory indicated an increasing sophistication in the growth of dynamic meteorology, and entailed a closer examination of data from more and more dense station networks. The use of information from self registering balloon instruments likewise was increasing. The beauty of the theory was (and still is) its direct appeal to, and visible correspondence with obvious day-to-day happenings on the daily weather maps. More modern theory is still unable to encompass frontal phenomena to much better advantage.

(5) Starting with the work of Jeffreys (1926) although others were also involved, the adequacy and the correctness of the axisymmetri-

cal models of the general circulation was formally called into serious question. A contributing factor to the views of Jeffreys was the increasing experience with synoptic charts and the associated introduction of frontal theory. Another factor was a theoretical argument developed by him.

As is perhaps inevitable with new basic ideas, not much attention was paid to the suggestions of Jeffreys for more than two decades. Often his views, when cited, were incorrectly expressed—another fairly common phenomenon when new basic ideas are at issue. During this era (and to a good extent even now) any "reliable" discussion of general circulation mechanics proceeded in essence on the basis of the early symmetrical models going back to shortly after Newton. This was (and is) the vogue in more popular expositions.

In the late nineteen forties the view was expressed by Rossby (1947) that the profile of the hemispheric mean zonal wind at the jet stream level was a result of the horizontal transports of vorticity across latitude circles—a concept perhaps not unrelated to the notion of the average meridional transports of angular momentum as conceived by Jeffreys.

During this entire time period the direct measurement of the free air motions and other properties by means of balloon techniques became more extensive. At the close of the second World War a hemispheric network of sounding stations became operational on a regular schedule. It was at this time that several investigators set about studying the angular momentum balance and related questions. Among these was the present writer who had considered the problem for a number of years, but had no access to the needed data. By mid-century it was for the first time quite clear that Jeffreys' main contention was vindicated, although this fact was bound up with many resulting problems. We witnessed here the first observed and measured instance of a sustained negative eddy viscous action, supporting the zonal mean flow instead of degrading it as a positive such action would do. Practically all subsequent research on the problem corroborated this result. See Starr & White (1951) and references there given.

(6) The next step was, as might be expected, the formulation of theoretical models containing negative viscous action of the large horizontal eddies. The approach used most effectively was that of numerical integration of the dynamic equations with the aid of electronic computers, using a finite difference mesh or by the use of orthogonal functions in a wave number resolution of the variables. These techniques also had their uses for numerical weather prediction, and were first applied to limited subregions of the hemisphere.

After about 1950 the main efforts toward obtaining solutions were along the lines of the quasi-geostrophic approximation. In accordance with this hypothesis the large scale motions are assumed to be geostrophic in many but not all terms which arise in the computations. For the present, suffice it to say that by this means dynamic models were obtained by Charney & Phillips (1953) in which for the first time there appeared a spontaneous development of negative viscous action in the horizontal as an essential process in the maintenance of the mean differential rotation of the atmosphere. This together with the prior observational findings to the same effect immediately placed into discard the symmetrical solutions which had relegated the action of the large eddies to that of a dissipative role merely.

A little later more detailed finite difference and wave number models were constructed which made no quasi-geostrophic assumption. These so-called primitive equation models with multi-level resolution in the vertical are much more complicated to set up. They will be referred to later. This brings to a close our retrospective review.

Some current developments

Since the initial studies of the writer and his co-workers (see Widger, 1949), greater and greater assemblies of data were put together for the northern hemisphere and also (one year) for the southern, the latter being something of an experiment conducted by Obasi (1963, 1965). The purpose continued to be to study the dynamic properties of the atmosphere from observations. Two main findings ensued. It was found, as expected, that horizontal eddy processes in the southern hemisphere gave rise to a negative viscous action supporting the mean differential rotation of the atmosphere, as in the northern hemisphere. Secondly, it was

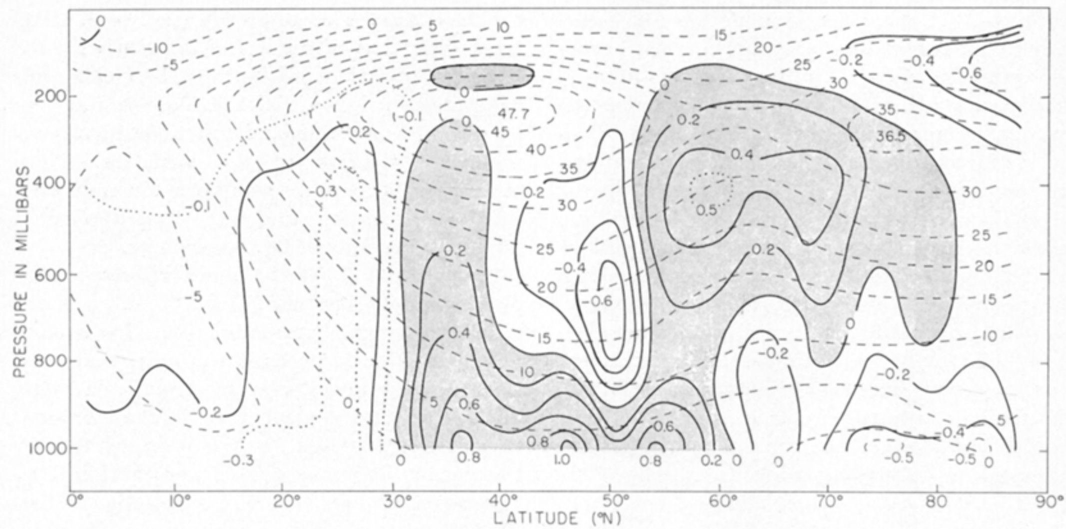

Fig. 1. Reynolds or eddy stress distribution across horizontal surfaces in dynes per square centimeter. Negative values indicate an upward momentum transport. Dashed lines show relative angular velocity of mean differential rotation in units of 10^{-7} sec^{-1}.

found by Newell (1964) that the lower stratosphere is essentially passive dynamically, being mechanically driven by energy derived from the troposphere. These two added results were chiefly the consequence of the utilization of the IGY data compilations. A theoretical model generating the passive lower stratosphere was devised by Peng (1965*a*, *b*). It generated the observed reverse temperature gradient and also the countergradient horizontal heat transport first reported by White (1954), as consequences of dynamic actions in the upper levels.

The first hemispheric compilations of data revealed the negative viscous action of the large eddies. The increased data resources made possible by the IGY revealed similar processes in the southern hemisphere and also the passivity of the lower stratosphere. It is therefore a logical expectation that the much expanded compilation for a five year period included in the MIT General Circulation Data Library should reveal further basic facts concerning general circulation dynamics. This has indeed been the case in at least one most important respect.

According to the geostrophic models, the large scale vertical eddy transports of angular momentum in the atmosphere should be negligible. For many years the writer doubted this, since there are systematic departures from geostrophic balance in the large eddies themselves and also in other respects. These large (and meso scale) departures might then lead to an upward eddy flow of momentum into the jet stream from the underneath. The great problem was of course how to secure vertical motion measurements to correlate with the zonal motion components.

For a time the writer became partially reconciled to the conclusion of geostrophic theory, since a fair sample (two months) of adiabatically computed vertical velocities used by him (Starr & Dickinson, 1963) indeed gave small effects. However, the painstaking work of Dopplick (1971) who computed the non-adiabatic components of vertical motion at upper levels pretty much destroyed any faith that one might have had in these results derived by the adiabatic method.

In order to obtain some light on the subject, it proved possible to balance the entire kinetic energy equation for the mean differential rotation of the atmosphere, provided that all modes of vertical eddy transport of momentum (including what we generally call friction) are lumped together. This was done using the five years of data. The picture of the Reynolds stress distribution in dynes per square centimeter averaged with respect to longitude and time is shown on the cross section in Fig. 1.

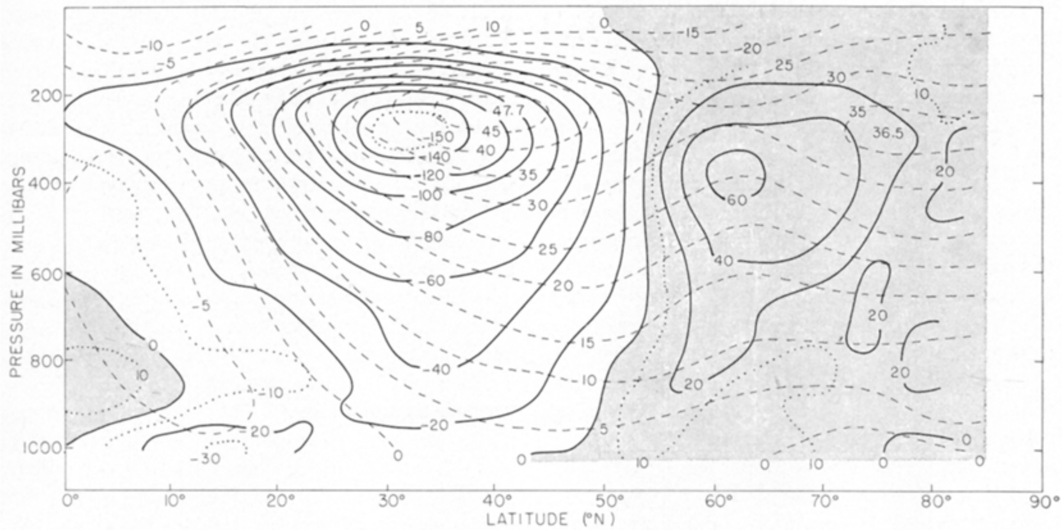

Fig. 2. Reynolds or eddy stress distribution across vertical surfaces in dynes per square centimeter. Negative values indicate a northward momentum transport. Dashed lines show relative angular velocity of mean differential rotation in units of 10^{-7} sec^{-1}.

The dashed contours give the corresponding mean differential rotation in terms of the angular velocity distribution with respect to the earth. As has been just hinted, since direct molecular viscosity effects are small, the vertical flux of momentum in the picture is due almost entirely to the whole spectrum of eddy processes. A separation of a portion of this spectrum into what one might call true friction must remain rather arbitrary and uncertain pending direct actual measurements. Reference may here be made to Starr, Peixoto & Sims (1970) and Rosen (1972*a*).

In Fig. 2 there is portrayed a cross section similar to the previous one, save for the fact that it gives the Reynolds stress across vertical surfaces of constant latitude, again against a background of the differential rotation in terms of relative angular velocity.

Owing to the fact that the stresses in Fig. 2 are computed from direct wind measurements, there is a lower cutoff of the spectrum corresponding to the limit of resolution of individual balloon wind values. This is not deemed to be serious. The numerical values of the stress in Fig. 2 are, of course, much larger than those in Fig. 1. However due to the geometrical factors in the actual atmosphere, the convergence of angular momentum into the jet is of the same order for both components. It therefore follows that the vertical transports are as important in maintaining the jet as are the horizontal ones. The total is then of course offset by the divergence of angular momentum flux due to the reverse mean meridional circulation in the jet vicinity for the long term mean condition.

In terms of the kinetic energy of the mean differential rotation the total hemispheric generation by all eddy stresses combined (including friction, that is) turns out to be $+6.4 \times 10^{20}$ erg sec^{-1}, according to Starr et al. An equivalent amount is destroyed mainly by the effects of the mean meridional circulation. The total generation by the vertical fluxes is only -1.0×10^{20}, so that probably almost all the losses due to true friction are compensated by the effect of the larger vertical eddy action. There appear to be extremely large seasonal variations. Thus in spring the same two quantities in the above order are $+13.7$ and $+7.6 \times 10^{20}$, while in the fall they are -2.0 and -11.1×10^{20} erg sec^{-1}. We see then that in spring the larger vertical eddies generate enough not only to overcome friction but to provide a surplus of 7.6 units. In fall the large vertical eddies no doubt act negatively, augmenting friction to provide a net loss of 11.1 units. The seasonal values are derived from composite 15-month assemblies of data (see also Starr, Peixoto & Gaut 1970).

For these purposes even the five years of data is none too much in view of the natural variability of atmospheric processes. Longer accumulations with more and better data in certain respects will be subjected to analysis presently. In the southern hemisphere the sparsity of observations does not seem especially encouraging, but the situation will be examined nevertheless. We view with interest, however, the results of the primitive equation model of Smagorinsky, Manabe and Holloway (1965) which in general agrees with our long term mean findings.

Granting that there still may be a fair amount of correction needed to the values given as more and better data are brought into play, it nevertheless seems true that the geostrophic models unfortunately fail to give a correct picture of the more complete mechanics of the earth's atmosphere, since they suppress the vertical eddy momentum fluxes, negative or positive save for what is due to an (assumed) positive coefficient of friction. This does not preclude the usefulness of such models for certain types of forecasting and certain research procedures. From the standpoint of our present interests the fact that they capture the horizontal negative eddy viscous character of the atmosphere is of paramount importance as a stage in the development of realistic concepts for atmospheric mechanics, but we must now recognize further evidence from observations and from less restrictive models.

Patterns of progress

A review of progress in a given subject may take the form of an examination of the successive research treatments of some key problem. In our discussion this key problem has been the elucidation of the mean differential rotation of the atmosphere. The ramifications involved in the treatment of the topic then lead us to the mechanics of the general circulation as a whole. From our specialized account of past and present views on the subject, certain generalizations and comments appear to be in order. These may be stated as follows.

(*a*) In any attempt to describe the mechanical actions in the actual atmosphere, whether this attempt be a physical one or in the form of a more mathematical theory, it is obvious that we cannot take into account all the details of the spectrum of eddy motions present. It is inevitable that the motions we consider explicitly represent averages resulting from some kind or other of a filtering device. To that extent we deal explicitly, not with the actual motions, but certain of their statistics merely. In order to incorporate the influence of the remainder of the spectrum upon these statistical variables, we introduce what we shall here call *closure assumptions*. Viewed in this light the history of our subject, including its present state, may be looked upon as progress, very slow at times, in formulating the problem in such a way as to incorporate better and better closure assumptions.

As a first example, the early symmetric models of the global circulation involved the grossest species of averaging, retaining only the symmetrical differential rotation itself and the corresponding mean meridional circulations. The remainder of the (spatial and temporal) spectrum was involved in the closure assumption that it was of negligible effect or else constituted in toto a (positive) eddy viscosity. This viscous dissipational action was then to be overcome by the convective action of the mean meridional motions. The error in such a closure assumption was, as we now know from observational evidence, that at least the more gross eddy components exert an intense negative viscous action—so much so as to force, on the whole, a reversal of the mean meridional convective action. It must be said, however, that at the time models of this type were originally instituted and for a long period thereafter, it was within reason hardly to be expected that the particular more correct closure assumption could have been made. Only direct observational measurement could have provided a proper guide for so unorthodox a link as the agency of negative eddy viscous action, and this was unattainable until modern times.

(*b*) The filtering device resorted to in the next species of models was the quasi-geostrophic assumption. One object of these models was to ascertain whether this device would permit the appearance of the horizontal negative eddy viscous action which by this time was known from data studies. They indeed did do this, as has been stated, and hence their important contribution to our subject.

The closure assumption implied in the quasi-geostrophic models is no longer quite of the same variety as with the symmetrical models. No definite spectral range of eddies was explicitly averaged out in the principal variables. Rather, in various places the effect of the nongeostrophic components of motion were assumed to have negligible consequences. Thus the vertical large eddy transport of momentum turns out to be negligible, as already pointed out. The specific closure assumption then made was that all the remaining neglected nongeostrophic motions would comprise a positive viscous effect exerting a corresponding stress across horizontal surfaces. But now it turns out from data studies that on the whole there is a net negative eddy viscous action also in the vertical, friction included, converging momentum into the jet as vigorously as the horizontal negative eddy viscous action, in the long term average.

Again it is unlikely that the inadequacy of the closure assumption made in quasi-geostrophic theory could at the time have been rectified. The situation would have required too much courage, if one should call it that (one could think of better terms), in the absence of observational evidence which has become available only more recently. In any event, the step forward from the symmetrical models to the quasi-geostrophic ones was so great that dissatisfaction with the latter now must be viewed in proper historical perspective.

(*c*) Coming closer to the details of methodology in this connection, it is instructive to consider the role of nondimensional numbers and associated procedures in our subject. In particular the quasi-geostrophic procedures involve the smallness of the so-called Rossby number for the large horizontal motions of the atmosphere, except in the vicinity of the equator. This fact is then used in various ways considered proper in order to simplify by approximation the hydrodynamical and thermodynamical equations governing the general circulation, so as to make the problem simpler to solve. The philosophy is in some respects similar to the use of the hydrostatic simplification in hydrodynamics. The fact to be recognized here is that the smallness of the Rossby number for the hemisphere as a whole does not preclude rather large and systematic departures from geostrophic balance at various points in time and space. Order of magnitude estimates computed on the basis of the smallness of this number are therefore prone to miss special phenomena which are capable of exercising a profound influence on the behavior of the system as a whole.

Therefore from what we now know the use of this and similar devices for so complicated a system as the whole atmosphere is attended by most delicate and crucial questions—their nonchalant application may lead to gross shortcomings in the results. In Starr (1963) it is demonstrated that even the hydrostatic assumption may in a particular application lead to a cumulative error of spectacular proportions.

(*d*) Closure assumptions in our present context are usually expressed in terms of the concept of "parameterization" procedures. Thus the eddy motions not explicitly included in direct fashion are assumed, for example, to be allowed for by the introduction of a viscosity coefficient linking them to the motions for which a solution is sought. There is not much wrong in doing this, provided we know with sufficient detail in advance, from observations perhaps, what the parameterized motions really do in actuality. If we do not have such information, the process almost always carries the danger that our constructed parameters are incorrect, and in some instances may falsify the results most seriously. It is to be observed that for both of the two instances which we have described, the error in the closure assumptions was that the residual eddy effects were parameterized in terms of a zero or positive eddy viscosity coefficient, whereas in reality it is largely negative. One may well wonder whether further examples of this sequence of events may not appear for the atmosphere as more extensive measurements are made.

What has been said has its importance not only for the parameterization of eddy viscous effects but of other effects as well, such as those pertaining to the heat balance, etc.

(*e*) Formally at least, the so-called primitive equation models that have come into use in recent years involve the least extensive closure assumptions regarding the motion field. They therefore allow vertical transports of momentum by the eddies into the jet levels. In this respect they correspond more with observed fact than is otherwise possible. There still are closure assumptions involved in the portion of the eddy spectrum not resolved by the finite difference gridwork used, or by the finite number of harmonics retained in a resolution of the variables in the wave number domain. One may hope however that this restriction is less serious than in previous solutions. The generation of these models entails an enormous amount of computing as illustrated by the work of Smagorinsky and his co-workers, for example.

Some recent observational studies of meso and small scale systems in relation to vertical eddy momentum flux on these scales, for example by Houze (1972), tend to corroborate the plausibility of the parameterized friction effects ordinarily made for the sub-grid-size disturbances in these models for general purposes. Other studies like those of Gray (1970) cast doubt upon this. The deleterious effect of small scale turbulence on forecasting accuracy discussed by Lorenz (1969*a*) in any case still remains, however.

Some suggestions for future work

One may appropriately perhaps conclude our discourse with a few suggestions in regard to the observational and theoretical development of our subject. As usual in systems as complicated as ours, there is no royal road to success, and progress is to be expected only from hard work with attention to details. Only occasionally may we look to some brilliant thought that will ease the needed effort, but such strokes of good fortune do come—unpredictably usually, as is the case with many meteorological phemena themselves.

The entire drift of what has been said here points to the reliance that must needs be placed upon the observed behavior of the atmosphere. Were we but able to describe it thus perfectly, the model so created could hardly be incorrect, and its dynamics might then be described from such standpoints as we might desire. It would of course benefit us but little to look at it merely; its properties would still have to be examined in the light of the principles of dynamics and statistics.

Coming back to the rough rocks of reality, the writer's efforts along observational lines have been based in recent times mainly upon the five years of global records starting in May 1958 contained in the MIT General Circulation Data Library. Our experience has been that this corrected and refined data base is the best that we have thus far worked with, generally speaking. As already stated each sizeable increase in our data resources seems to have uncovered a new vital link in the observed dynamics of the atmosphere. A sample derived from the five year period was contained in Fig. 1. However, the more terms we wish to measure, the greater is the resolving power required and even more data are needed. It would therefore appear desirable for a few interested organizations to pool resources in order to place in usable form an additional ten year period for both hemispheres. The total fifteen year period would then include more recent improvements in observational facilities and would, among other things, permit us to examine more effectively interannual variabilities, which latter might in certain ways be related to the phase of the solar magnetic cycle. The labor involved would however be rather imposing, even with our accumulated experience thus far. In the third five year period especially, effort should be made to include all available novel type data from satellites, ghost balloons and the like.

We have made mention of the sub-grid-size eddies in finite difference formulations as being the forefront currently of the closure problem. It is therefore most important to investigate as fully as can be done the momentum transports due to the smaller features in the spectrum of atmospheric disturbances not excluding the effects of moisture. The feeling one gets from the efforts in this direction like, for example, the one by Houze mentioned earlier is that here we finally may parameterize the vertical transports as a positive viscosity, but the subject may still contain some surprises. The horizontal transport by convective eddies of momentum equatorward is another matter, as follows from the recent discussion by Starr & Rosen (1972), although with enough data this contribution should be caught by the usual measurements.

And now as to the physical and theoretical problems. In spite of the great theoretical activity taking place in meteorology, many of the important questions of a fundamental nature, some of which were raised many years previously, still go unanswered. Yet it is of much importance for us to hold them before us, especially in our present discussion, because unless we find proper enlightenment concerning them, our theory of meteorology will remain grossly insufficient as to fundamentals. We may list some of these as follows.

A. We still do not know why it is that in the lower troposphere we have westerlies in middle latitudes and easterlies toward the equator, rather than the reverse. In addition to being observed, this state of affairs results also from the integration of the most complete models which can now be devised. Looking at either the atmosphere or the models we see the same thing. But we cannot say why this situation obtains (see Lorenz 1967, 1969*b*). The state of affairs in the sun and in Jupiter is quite the opposite—we have an equatorial mean jet of westerlies.

It seems to me that effective simple arguments about this matter will most likely be derived through the use of integral methods. The beginnings of such an approach are contained, especially for the solar case, in the article by Starr & Rosen (1972), but more work should be done to sharpen and amplify the procedures. For the sun we are now in a position similar to that in meteorology, namely that there have been produced some satisfactory models as for example those of Gilman (1971). But, as has been said, looking at them does not give us any notion of why they or the sun work the way they do—this is a separate problem requiring special methods.

The question concerning Jupiter is complicated by the special feature that the profile of the zonal winds seems to be, according to Chapman (1969) and others, essentially a step function with extreme shear zones especially at about 10° N and S zenographic latitude. The possibility of the existence of such rather sharp convective vortex sheets in rotating fluids heated from below is currently discussed by Rosen (1972*b*) and Starr (1972).

In a way the demonstration of the proposition that a given assumed regime is impossible should be easier than the demonstration of its possibility which latter may depend upon many details. If the regime violates one necessary condition, it is impossible. A tradition of such research was started probably for the first time by Cowling, as early as 1934, who showed that an axisymmetric solar dynamo is impossible. This has stood as a guidepost for solar magnetic studies since then, and served its purpose in helping to suggest the recent work of Gilman (1969) and Gordon (1972) on the solar dynamo problem. Following in part the precedent of Cowling (1957), I have during the past year succeeded in demonstrating fairly satisfactorily (Starr, 1971) that a steady axisymmetrical circulation cannot sustain a closed equatorial maximum of angular velocity. A similar view has been expressed in more general terms by several writers, notably Hide (1970). Further work is indicated also here, if possible to broaden the range of forbidden regimes.

B. We still do not know why fronts are present in the atmosphere. The true dynamics of their formation probably involves the behavior of large portions of the atmosphere, and is no doubt an aspect of general circulation dynamics. It has always seemed to me that, at lower levels in the atmosphere, the frontal shear zones are in reality uplifted friction layer vortex sheets. More study is required to explore various possibilities such as this. Squall lines also need more research (see Starr, 1972).

C. We still do not know much about "frictional" destruction of kinetic energy in the atmosphere, simple views of the subject notwithstanding. Much of what we look upon as friction may involve certain eddies which reconvert kinetic into available potential plus internal energy on some scale, through processes *not directly dependent on molecular viscosity*—a view already known to some extent. After all, we seem to have large eddies which do this in the lower stratosphere.

D. We still do not know in a sufficient manner what the function of water vapor is in the general circulation, although some things have been done by Manabe et al. (1965) and also Peixoto (1965). The incorporation of latent heat into the concept of available potential plus internal energy as was defined by Lorenz is a formidable undertaking. It may nevertheless be possible to do this with benefit when applied to the general circulation.

E. We still do not know after more than twenty years since its existence was demonstrated, why negative eddy viscosity plays so ubiquitous a role, not only in meteorology but also to the extent of our present knowledge in various cosmic circulation systems. There are some reasons to suspect that negative viscous phenomena play a function also in much smaller natural flows. In certain of these cases one rapidly approaches molecular scales, at least as far as large molecules are concerned.

One way of approaching the subject is to consider the flow of a fluid in a horizontal tube or channel of uniform cross section which is reentrant so that there can be no net pressure head or force of gravity acting along its length. The problem is to cause the fluid to start circulating in the tube, and later to maintain its net mass transport as a quasi-steady state, whatever the further details may be which we shall consider presently. Obviously the only way in which the fluid can obtain the needed momentum to start moving is from the walls of the tube (bottom or sides of the channel). Especially in a smooth tube and normal fluid properties, this requires a (mean) countercurrent at least at certain portions of the wall, so that surface friction may pick up the needed momentum. Negative eddy viscous action (assuming it to be supported in a proper way by a thermal energy source) must then arise a little away from the wall to transmit the momentum to the bulk of the fluid and to permit the countercurrent to continue.

Still speaking of the experiment, we know very well that the action described is possible. A suggestion of this general nature was made by J. J. Thomson as early as 1857 and was later verified experimentally by Fultz (1959) in the form of the well known moving flame experiment. A version of this experiment in which the flame impinges on the lower portion of the side wall instead of the bottom was recently described by Whitehead (1972). D. Salstein at M.d.T is investigating numerically possible similar actions with a bottom which is both mechanically and thermally rough in a special way; i.e., without a moving heat source. Again negative viscous action due to convective eddies associated with the "mountain effect" of the roughness elements could cause the mean transport to develop. The obstructions give the fluid a better perch whereby the reaction of the motions tends to build up higher pressure on the downstream faces of these roughness elements in a systematic fashion. The fluid can thus push itself along, like a man rows a boat by pushing back on the environment with the oars (i.e., eddies in our annulus). In either the case of the oars or the eddies an energy source is of course needed.

It is a matter of only small imaginative effort to arrange an analogy of what we have said to the case of the mean westerlies propelling themselves without a net pressure head or net downstream component of gravity, through the lateral eddy effects and the mean surface easterly countercurrents at lower and higher latitudes. However, the vertical upward eddy transport of momentum below the jet stream, shown in Fig. 1, is harder to deal with. This general type of approach may be one way to organize thinking about negative eddy viscous actions, but not the only one. Thus for example in the two dimensional case, the exchange of kinetic energy among nonzero harmonics has been treated by Fjortoft (1953) and among point vortices by Onsager (1949).

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ЗАМЕЧАНИЯ О ПРОГРЕССЕ В ИССЛЕДОВАНИЯХ ПО ОБЩЕЙ ЦИРКУЛЯЦИИ

Дается краткий очерк исследований общей циркуляции земной атмосферы. Особое внимание уделяется сегодняшним воззрениям и текущим исследованиям. Выделяются последовательные стадии исследования с точки зрения лучшей формулировки проблемы с целью избежать ошибочных упрощающих предположений. Становится очевидным, что согласно этому критерию наилучшим численным аппаратом решения проблемы являются примитивные уравнения. Все более настоятельной становится необходимость новых эмпирических исследований, которые обеспечат нас наиболее правильной картиной механики общей циркуляции, — утверждение, многократно подтвержденное историей вопроса. Высказываются некоторые предположения относительно будущих теоретических и наблюдательных исследований.