CHAPTER 1

The Evolution of Dynamic Meteorology

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Introduction

Philosophers have pondered the vicissitudes of the weather for millenia, but it is only during the twentieth century and part of the nineteenth that our understanding of the physical laws that govern the atmosphere has been thorough enough to enable us to account for what we observe. As meteorologists well know, our eventual grasp of the laws merely made it *possible* to account for things; the explanations themselves were not immediately forthcoming, and some of them elude us even today. In this exposition I shall be examining how the search for these explanations—the work of the dynamic meteorologist—has advanced during the lifetime of the American Meteorological Society (AMS).

What constitutes the *state* of an evolving scientific discipline—dynamic meteorology or something else—at a particular moment in history? Does it encompass the ideas taking shape in the minds of the most forward-looking scientists? Does it include only those ideas that have found their way into the refereed literature? Is it the knowledge that is regularly imparted in the classroom in institutions of higher learning, available to all who have the opportunity to enroll? Is it limited to the material appearing in textbooks, available to a still greater audience?

More generally, does the state depend upon the activities of a handful of leaders or a multitude of followers? Is it also partly determined by rather different factors, such as the existence and availability of intricate and often expensive equipment—giant telescopes for astronomers, instrumented seagoing vessels for oceanographers, and supercomputers for almost everybody?

I would maintain that in addition to describing the tools of the trade and noting such details as the physical locations where the relevant activities take place, a comprehensive account of a scientific discipline ought to cover the specific activities of the innovators and the typical activities of the ordinary practitioners. Some qualifying comments are in order. Even recently published scientific textbooks are often sadly out of date, if their authors are not currently active in the special fields about which they write, and errors sometimes seem to propagate without viscous dissipation from one book to the next. Nevertheless, textbooks, good or mediocre, and also programs of study, exert a distinct influence on what is taking place within a discipline; surely they are part of its state. At the other extreme, a leader's advanced idea that has yet to reach a journal may not influence anyone at all, unless it has been presented in a lecture or offered to a protégé as a possible topic for a dissertation. Even published works can lie dormant, but does this mean that they are not part of the state? Perhaps a satisfactory assessment of the state of a discipline needs to include some combination of ideas, publications, classes, and texts.

Next, what constitutes dynamic meteorology? Clearly the term alludes to methods of attack rather than specific atmospheric phenomena. For example, one can learn much about extratropical cyclones through careful examination of the extensive observational data that have been accumulating for many years. The task of the dynamic meteorologist, however, is to account for the observed characteristics of these cyclones by means of the physical laws that govern their behavior. More generally, the laws of hydrodynamics and thermodynamics constitute the foundation of dynamic meteorology.

There is no rule that says mathematical equations must be used in a dynamical study; witness, for example, Victor Starr's lucid equationless discussion of nearly 50 years ago on the controlling influence of angular-momentum transport on the general circulation (Starr 1948). Nevertheless, the more involved problems are often rather unapproachable if properly formulated equations are not put to quantitative use; some problems, in fact, have proven intractable even with equations.

The intricate laws of absorption and emission of radiation are often considered to be in the realm of physical rather than dynamic meteorology. Indeed, there is no universal agreement as to where dynamic meteorology stops and another branch takes over. In introducing a recent volume of the well-established journal *Tellus,* the newly installed editor has written (Sundqvist 1992), "the theme of *Tellus A* will continue to be *Dynamic meteorology and oceanography.* The series will encompass [all of] dynamic meteorology and oceanography, including numerical modelling, synoptic meteorology and weather forecasting, and dynamic climatology." As a dynamicist, I naturally think that synoptic meteorologists should be proud to be included among us, but I suspect that many synopticians do not share my feelings.

Finally, no account of dynamic meteorology can be complete without acknowledging that some of those who tackle and solve its problems do not even consider themselves to be meteorologists; often they are fluid dynamicists or more general applied mathematicians. Likewise, a complete account would have to recognize the field of dynamic oceanography, both because the ocean and the atmosphere have rather similar dynamics, so that results established for one system often apply to the other, and because the two systems are so strongly coupled physically.

As for the remaining word in my title, "evolution" is indeed an old term with a fairly general meaning, but it signifies, to many people, the specific process through which new living species come into being. In the case of our own species, early primates did not change continually until they all became human beings; there are still plenty of apes and monkeys around. More generally, older species sometimes survive even as new ones evolve from them. Sometimes the appearance of new environmental conditions will favor a mutated form of an established species, while, if the original conditions are still to be found at nearby geographical locations, the earlier form can continue to flourish.

This is what appears to have happened in dynamic meteorology. A new species of investigation has developed in response to the change in the scientific environment brought about by the advent of the computer, while those dynamicists who have preferred to make minimal use of computers have had no difficulty in keeping the older species alive. I shall presently consider this matter in greater detail.

The early years

For an overview of the state of dynamic meteorology in the early years of the AMS, let me turn to my own first experiences with the subject. The time was 1942, a few months after the United States had entered World War II, and I suddenly found myself transplanted from the Mathematics Department at Harvard University, a school located about two miles up the Charles River from the Massachusetts Institute of Technology (MIT), to MIT itself. The program at the MIT was the regular graduate course in meteorology, but, to accommodate the armed services in their effort to acquire a large number of weather forecasters within a short time, two ordinary academic years had been crowded into a fraction of one. Lectures filled the mornings, with map analysis and forecasting in the afternoons.

One subject that I found particularly inviting, with my mathematical background, was called "Dynamic Meteorology." Our teacher was the late Bernhard Haurwitz, already one of the universally acknowledged experts in the field. The textbook that he chose was also called *Dynamic Meteorology,* and, in fact, he was the author. Certainly he was in a position to write an up-to-date book, and he evidently took great pains to do so. Published in 1941, the book even contains a derivation of Rossby's celebrated formula for the speed of large-scale waves superposed on a westerly wind current, announced only two years previously (Rossby et al. 1939). It also mentions the even more recent suggestion of Starr and Neiburger (1940) that potential vorticity might be used as a tracer of atmospheric motions.

Much of the more recent material of the book was, however, offered only in more advanced courses, and most of the dynamics that we learned could have been taught when the AMS first came into being. We learned the equations of motion in their Eulerian form in a coordinate system rotating with the earth—the form still favored in the bulk of today's studies. We learned how to apply the equations to account for specific features—for example, the manner in which the wind typically varies with elevation through the lowest kilometer of the atmosphere in extratropical latitudes. This is of course the familiar Ekman spiral, a structure first deduced in an oceanic context (Ekman 1902). I had been looking forward to the moment when we would learn how to use the equations for forecasting—the task to which our accelerated program supposedly was primarily dedicated. That moment never arrived. Had I looked ahead in the book more carefully, I would have guessed that this would be so, for, at the opening of a chapter devoted to a rather different topic, Haurwitz (1941, 180) states, almost as an aside, "an attempt to compute the future weather by direct application of the equations of thermodynamics and dynamics seems at present not promising." After mentioning the well-known attempt by Richardson (1922), Haurwitz concludes the paragraph by saying that "a computation of the future weather by dynamical meth-

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ods will be possible only when it is known more definitely which factors have to be taken into account under given conditions and which may be neglected." Here Haurwitz anticipates what would be realized a decade later, when the first moderately successful numerical forecasts would be produced by neglecting most of the conceivably important factors (see Charney et al. 1950).

Extratropical cyclones are arguably the most prominent features of sea level weather maps, and explaining their existence constitutes as fundamental a problem in dynamic meteorology as forecasting their behavior. The most commonly accepted hypothesis before World War II, championed by Vilhelm Bjerknes and his collaborators, was that they originated as small growing disturbances on the polar-frontal surface (see Bjerknes et al. 1933). Acceptance was often tempered by some reservations, since some of the observations suggested that a frontal surface and an accompanying cyclone would appear simultaneously rather than the former preceding the latter.

Haurwitz emphasized to us that the equations of motion were nonlinear, with the nonlinearity, in the absence of external heating and internal dissipation, arising from advective processes, which show up as quadratic terms in the equations. He devotes a chapter to the perturbation method, where linear equations are derived from the nonlinear ones, and he applies the method to such problems as the instability of a horizontal surface of discontinuity, but subsequently, in discussing the wave theory of cyclones, involving the postulated instability of a sloping surface of discontinuity, he states (p. 307): "the mathematical problems arising out of a study of these instability conditions are very complicated, and it cannot be claimed that a complete solution has been reached."

Certain nonlinear equations are readily solved analytically, but the equations of motion are not among them. Indeed, no workable method for solving the equations in their full nonlinear form was currently in use.

The great mutation

Long before World War II came to an end, meteorologists became aware that automatic electronic computers were in the process of development and that there would undoubtedly be attempts to use them to forecast the weather by numerically determining particular solutions of the dynamic equations. As to whether such attempts could ever succeed, there was no unanimity of opinion.

It would be easy to conclude that once that war had ended, meteorologists finally had time to become seriously involved with the problem, so that, soon afterward, numerical weather forecasting became a fact, but this does not seem to be the way that things generally happen. If during wartime a new development is perceived as being important to the war effort, it will probably progress faster rather than slower than it would in peacetime; witness the sudden implementation of radar. Numerical weather forecasting, whatever its importance, had to await the digital computer, which, even with such influential champions as John von Neumann, probably could not have been developed much sooner. Most likely it was coincidence that the first numerical integration of the barotropic vorticity equation (Charney et al. 1950) followed the war by only a few years.

The story of this integration, and of the continual advances in numerical forecasting that have filled the 45 years since that time, will be told elsewhere in this volume. What most of us may not have anticipated in the early days of computers, when they were a luxury to which few meteorologists had access, was the role that they would play in ordinary research. This possibility was forcefully revealed when Norman Phillips (1956) performed his first numerical experiment, where his numerical integration, extending over a simulated month, produced a general circulation, from which he could compile climatological statistics. In the years that followed, theoretical studies built around numerical solutions of the dynamic equations became more and more abundant as computers became available to more and more potential users. Gradually a new standard format for a dynamical investigation took shape.

First, in such a study, one formulates a specific hypothesis. Next, one constructs a model; this is typically a considerably simplified form of the dynamic equations, arranged for numerical solution. The model must not be so highly simplified that it cannot possibly represent the anticipated outcome, and in fact, it should be general enough to be able to represent other possible outcomes as well. Next, one obtains one or several numerical solutions of the model equations; often these are time-dependent solutions originating from selected initial states. Finally, one interprets the results and observes whether the hypothesis is supported.

Of course the format has a number of variants. In particular, despite what is sometimes maintained, a working hypothesis is not essential; one may simply ask a question. It seems perfectly legitimate, for example, to seek the stability of a specific flow pattern without first hypothesizing that it is stable or unstable.

Questions may be of various types. One that refers to the real atmosphere presumably cannot be answered unequivocally by a study that uses a model. However, a question may refer to what is in fact a model; for example, it might ask about an adiabatic frictionless atmosphere, or flow on a beta plane; it might also ask about a model that has been so simplified that analytic solutions are easily found.

Sometimes the computer is simply a convenience (or an inconvenience) rather than a necessity. If a model is moderately simple and only a steady-state solution is sought, the needed computations might require a few milliseconds on today's computers, but they might also be performed by hand in a week or so. Since several weeks would presumably be needed to write up the results afterward, the absence of a computer would not greatly increase the total effort. If instead the model is rather large, and if a number of time-dependent solutions are needed, what the computer could do in a few hours might take a lifetime to do by hand. Effectively the study would be impossible without the computer. Even if one were willing to devote a lifetime to a computation, the relevance of many questions does not endure for a lifetime.

The computer has thus given rise to a new type of investigation seldom encountered in yesterday's dynamic meteorology. In effect, the change in the scientific environment produced by the computer has enabled a mutation—the substitution of numerical for analytical procedures—to produce a flourishing species.

Meanwhile, the original species still thrives. Particularly when computers were rather new, there was a widespread feeling among mathematicians—von Neumann was the most notable exception—that numerical computation was not a legitimate method of solving problems. This attitude extended to a number of mathematically minded meteorologists and other fluid dynamicists.

We have seen that the perturbation method—linearizing the dynamic equations, or some simplification of them, about a known steady solution—will often produce a system of equations that may be solved analytically. Ordinarily it will offer a sound means for testing a flow pattern for stability. As time progressed, however, it became widespread practice to treat the solutions of perturbation equations as approximate solutions of the nonlinear equations themselves, regardless of whether their departures from any known steady solutions were in some sense small. Indeed, for many dynamicists who found solving the equations the most satisfying way of putting them to use, linearization became the method of choice.

The method acquired a number of refinements. One can formally express each dependent variable of a system of equations as a power series in some quantity, say ϵ; this may be any scalar that is representative of the amplitude of the perturbations. These series may be substituted into the equations, whereupon the coefficients of separate powers of ϵ form a sequence of systems of linear equations. The known steady solution satisfies the zero-order system, and with this solution, the first-order system becomes the usual set of perturbation equations. Having solved these, we can in principle solve in turn the second-order system, the third-order system, etc., thereby obtaining corrections to the perturbation solution. It appears that in many cases the series will converge for small values of ϵ. The possibility that they may not converge for the values of principal physical interest has not appeared to disqualify the studies for publication.

In a variant of this procedure, no basic flow is assumed, and the quantity ϵ is simply some quantity that may in some sense be considered small. A favorite choice is the Rossby number. Here the first-order system need not be linear, and instead of solving it one often chooses it as a new model of the original system (which may itself have been a model). Various quasigeostrophic systems, regularly used in the early days of numerical forecasting, and still extensively used in research, may be derived in this manner.

Numerous more elaborate techniques for handling the dynamic equations have been introduced, and I shall mention just one, which typifies the ingenuity that dynamicists often exhibit in their attempts to pursue an analytical approach. It is used when the solutions are expected to contain oscillations with contrasting timescales. It is known as two-timing, and it consists of replacing the independent variable, time, by two independent variables: fast time and slow time. Some dependent variables, perhaps divergence, are treated as functions of fast and slow time, while others, such as vorticity, may be treated as functions of slow time only. The procedure leads to logical complications, which appear, however, to have been reasonably well resolved.

The two species of investigation that I have described, along with their subspecies, are distinguished from one another by their methodology rather than by the particular meteorological problems they aim to solve. The upshot of the many developments is that, for many problems, linear and nonlinear systems now stand a more or less equal chance of yielding to solution by one method or another. It might be anticipated that linear theorists and nonlinear theorists would occupy opposing camps and denounce each other at scientific meetings, but this is by no means the rule. There have been numerous papers, often by single authors, that develop, in separate sections, the linear theory and the nonlinear theory of one and the same problem.

One may legitimately ask what is to be gained by working out the linear theory of a problem if the supposedly more accurate nonlinear theory is to be worked out in any case. There are actually a number of potential benefits. Linear and nonlinear methods agree fairly well when applied to certain problems and rather poorly when applied to others. If, by applying both types of method when this is feasible, one can get some idea of the sort of problem where linear methods do work well, one will have some idea of how much confidence to place in them when the nonlinear approach is not convenient. Perhaps more importantly, inspection of a linear solution, obtained by analytical procedures, often leads to some understanding of what is taking place to produce a particular effect, whereas observing a sequence of numerical steps may yield little insight. On the practical side, any particular numerical solution applies to only one set of values of the parameters of a system, while a single analytic solution can cover a wide range of values. I suspect, however, that many dynamicists continue to pursue analytical methods, which are likely to demand a linear approach, because they hold these methods in higher esteem than numerical ones.

New concepts

Wholly irrespective of the influence of the computer, which has brought about such remarkable changes in methodology, dynamic meteorology has, both before and after the advent of the computer, witnessed the appearance of a continual stream of new concepts. Some of these have been properties or processes that were waiting to be discovered. Some have been the dynamicists' own creations.

Let me offer, in approximate chronological order of their appearance, a rather incomplete list of terms denoting concepts that were unknown in the dynamic meteorology of 1920 but are much in evidence today. Some are now so familiar that it is hard not to think of them as always having belonged to the meteorological language. Time will tell whether some of the newer terms will acquire the same status. The list follows:

- isentropic analysis
- Rossby waves
- the beta plane
- potential vorticity
- baroclinic instability
- vacillation
- the balance equation
- available potential energy
- quasi-biennial oscillation
- low-order models
- CISK (conditional instability of the second kind)
- chaos
- global circulation models
- semigeostrophy
- enstrophy
- waveguides
- wave overreflection
- Eliassen–Palm flux
- gravity wave drag
- the surf zone.

I have purposely limited the list to 20 entries; it could easily have been made twice as long. Any discussion that I could present here, covering even 20 items, would have to do injustice to all or most of them. I have therefore chosen a sample of three for special consideration.

High on anybody's list of concepts that have altered the course of dynamic meteorology must be baroclinic instability—the instability of a flow possessing a continuous poleward decrease of temperature, and an accompanying continuous upward increase of westerly wind speed, as opposed to a flow with a sloping discontinuity in temperature and wind. It is hard to imagine any phenomenon that, following its discovery, has formed the subject of more papers and dissertations. The original works of Charney (1947) and Eady (1949), which first identified the phenomenon, used simple models and chose basic-flow patterns whose decreases in temperature occupied the entire width of the region involved, while the increases in wind occupied the entire depth. Subsequent studies have applied numerous models of varying physical complexity to numerous temperature and wind profiles. Other studies have attempted to assess the importance of baroclinic instability for the workings of the real atmosphere.

Dynamically, polar-frontal instability and baroclinic instability are perhaps equivalent phenomena, the former being a limiting form of the latter as the zone of transition shrinks to a single surface. It is noteworthy that in separate laboratory experiments, involving flow in rotating containers (see Fultz et al. 1959), the breakdown of a surface of discontinuity separating two homogeneous fluids of different densities, rotating at different rates, closely resembles the breakdown of a baroclinic zone. The mathematical details, however, are much more simply handled in the case of baroclinic instability—a situation that has undoubtedly favored the proliferation of studies of that phenomenon. Meteorologically, the phenomena are different in that polar-frontal instability, if it is present at all, is most pronounced in the lower troposphere and produces cyclones of limited horizontal extent, while baroclinic instability is more prevalent in the upper troposphere and gives rise to longer waves. A set of cyclones and a set of waves, if the latter is present above the former, are necessarily coupled in agreement with the hydrostatic relationship, but individual waves and their associated cyclones may move at different speeds and may even become decoupled as the intervening temperature field undergoes continual deformation. It was undoubtedly the meteorological rather than the mathematical distinction that Charney had in mind when, in recalling his early work, he emphatically maintained that the two phenomena were not the same (see Platzman 1990).

For idealized, zonally symmetric flow patterns, baroclinic instability is virtually an established fact, but it is likely not the answer to the origin of all large-scale tropospheric disturbances, even in temperate and polar latitudes. One seldom sees, even temporarily, essentially symmetric flow on which waves can proceed to grow—at any chosen time the waves are already present. Do new waves form because of the instability of flow patterns already endowed with waves? Do they instead form because the natural distortion of existing patterns produces details that are wavelike in structure? Are these two alternatives really the same thing looked at from different perspectives?

Contrasting with baroclinic instability in its nature, but rivaling it in its influence on dynamical thinking, is the concept of potential vorticity. Indeed, a recent paper (Holopainen and Kaurola 1991) even opens with the sentence, "potential vorticity is the backbone of dynamic meteorology." Whereas baroclinic instability is a *quality,* which the whole atmosphere, or a substantial section of it, may or may not possess, potential vorticity is a *quantity,* whose value, like temperature, varies from point to point throughout the atmosphere. It was identified by Rossby (1940) and in a more general form by Ertel (1942) as a quantity whose value at individual parcels of air does not vary under adiabatic flow. The proposal of Starr and Neiburger (1940) that it might prove useful as a tracer of atmospheric motions actually bore fruit more than a decade later when Reed and Sanders (1953) investigated an upper-level frontal surface and found that within the narrow transition zone the potential vorticity had high values typical of the stratosphere, while, on either side, the values were lower and typical of the troposphere. They came to the rather striking conclusion that the two boundaries of the transition zone were actually a section of the tropopause sharply folded over and enclosing air of stratospheric origin.

It might appear that an outpouring of papers centered on potential vorticity, like the outpouring of papers on baroclinic instability that later followed Charney's and Eady's discoveries, should have quickly followed Rossby's and Ertel's findings, but evidently this was not the case. Whereas it was easy to test additional and presumably more relevant basic-flow patterns for instability, one had to think of specific problems where potential vorticity could be put to use. Not the least of the obstacles was the difficulty of determining actual detailed distributions of potential vorticity on any regular basis with the data then available, especially in the stratosphere where the flow was most likely to be nearly adiabatic.

Although potential vorticity is a fairly complicated function of the observable variables—wind, pressure, and temperature—it assumes a simpler form in an isentropic coordinate system. An important finding has been that a field of potential vorticity can be inverted; that is, if one knows the distribution of potential vorticity on each isentropic surface, together with the total atmospheric mass above each isentropic surface, and the temperature distribution at the base of the atmosphere, one can, under the assumption of hydrostatic and quasigeostrophic balance, determine the complete fields of wind, pressure, and temperature. The actual inversion process may be awkward to implement but it becomes fairly simple in a quasigeostrophic model where the potential vorticity assumes a rather simple form and can even serve as the basic prognostic variable.

Once the routine use of large global circulation models, extending to considerable heights, had become standard practice in operational weather forecasting, it became possible to evaluate the potential vorticity on a point-by-point and day-by-day basis from the analyses constructed by the models for use as initial states. This in turn made it possible to construct sequences of synoptic charts showing the distribution of potential vorticity on various isentropic surfaces.

The individual studies that have benefited from the availability of these charts are too numerous to list, and the reader is referred to the comprehensive discussion by Hoskins et al. (1985), with its 176-entry bibliography. Here I shall mention only one of the more spectacular applications, that of McIntyre and Palmer (1983). By examining a succession of charts, they have detected the breaking of Rossby waves in the stratosphere. That is, the waves become irreversibly deformed, like ocean waves as they approach the shore, and ultimately bring about an injection of air from lower latitudes into the polar vortex; in doing so they dissipate a substantial amount of kinetic energy.

A final concept to which I have personally devoted much attention is what is popularly called chaos. A chaotic system is one that exhibits sensitive dependence on initial conditions; that is, a small alteration of the state at one time will lead eventually to a state differing considerably from the state that would have occurred if the alteration had not been made. The atmosphere cannot be examined directly for chaos, but numerical studies with models with various degrees of complexity leave little doubt that the atmosphere is chaotic. Indeed, the nonlinearity introduced by the presence of advective processes—the only nonlinearity in some of the simpler models—is by itself quite sufficient to produce chaos.

The most obvious and most familiar consequence of atmospheric chaos is the limitation that it places on the possibility of forecasting most aspects of the weather pattern at long range, say two weeks or more in advance, in view of the impossibility of starting from a perfect analysis or using a perfect extrapolation procedure. However, there are more fundamental changes in dynamical thinking that the recognition of chaos has brought about.

For example, we now realize that many of the equations that we would like to solve—even some rather simples ones—do not possess general solutions that can be expressed in analytic form. If we have succeeded in analytically determining particular solutions, presumably steady or periodic ones, and if our equations are realistic enough to have captured the atmosphere's chaotic nature in their general solutions, our solutions will be highly specialized ones, and their relevance to the real atmosphere will at least be suspect.

Should we conclude, for example, at least in the context of a model that we are using, that the long-term average transport of angular momentum across middle latitudes is poleward, if a particular solution that we have found analytically should show a poleward transport? The real atmosphere does not exhibit a continual poleward transport but instead possesses periods of poleward and also of equatorward transport, with the former dominating. An extended solution of any realistic model should be expected to behave likewise. One property of chaotic solutions is that if one can identify a segment, and there should be many such segments, where the initial and final states are very much alike, a slight alteration of the initial state will produce a segment where they are exactly alike, that is, a periodic solution, albeit an unstable one. Hence, we might have happened upon an analytic solution in which equatorward transport prevailed, rather than the solution that we did find. This being the case, can we show that the solutions with poleward transport are in some sense more representative? Are they, although unstable, perhaps less unstable? Here are plenty of problems left for the dynamicist to think about.

It also appears that, more generally, we must meticulously avoid obtaining several numerical solutions for any problem and then concluding without further inquiry that the ones that support a previously conceived hypothesis are the more representative ones. Certainly we must avoid acknowledging only these solutions in our write-up, even though we could do so without falsifying any results. Similarly, if we have been forced to base our conclusions on a single chaotic solution, we should be aware that another chaotic solution of the same equations might have led us to conclude something else.

So far I have been stressing the possibility that a system once thought to be behaving regularly may be found to be chaotic. We should not ignore the possibility that some systems once thought to be *random* may be found to be chaotic, in which case we may be able to discover their underlying dynamics. Chaos indeed has its positive side as well as its negative one.

Concluding remarks

In summary, the aim of dynamic meteorology—the determination and explanation of the observed or observable properties of the atmosphere through application of the physical laws—has not changed during the past three-quarters of a century. The total effort has undergone some shift from explanation to mere determination, if we regard forecasting as the determination of the course of individual states of the atmosphere. The forecasting problem has introduced subsidiary dynamical problems, such as parameterization and initialization.

The most evident change has been in methodology. The construction of models and the subsequent determination of time-dependent numerical solutions, using the computer, has not simply dominated the forecasting process; it has become a favorite research method. Meanwhile, new analytical techniques, as well as new numerical ones, are continually being devised.

Where will dynamic meteorology go in the years to come? I cannot visualize any single new event that will so profoundly affect dynamic meteorology as the development of the computer, although, had I been a meteorologist rather than a small boy 75 years ago, I presumably would not have visualized the coming of the computer. Perhaps the best that I can do is to extrapolate current trends.

What dynamic meteorology seems likely to do is to shift toward regions and aspects of the atmosphere that have been somewhat neglected in the past. Already more attention is being paid to the stratosphere and the tropical troposphere, where typical processes seem to be less nonlinear, and to mesoscale and smaller-scale systems, where they may be even more nonlinear. As for longer-period behavior, we do not yet know to what extent the progressive changes of climate, other than those associated with changing external conditions, are determined by the climate itself and to what extent they are mere statistical residuals of shorter-period fluctuations. I feel confident that dynamic meteorology will provide the answer in the coming years.

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