

CHAOS AND THE WEATHER FORECAST

It is indeed a pleasure to be able to be able to speak to this great gathering, and to talk about a subject of which I am very fond: chaos. Let me begin by quoting a few lines of verse.

This isn't the first time that they have been quoted, and it isn't the second, and when I mention that they were written long ago and are probably familiar to many of you, and have been cited in connection with chaos, some of you may guess what they are:

For want of a nail the shoe was lost,
For want of a shoe the horse was lost,
For want of a horse the rider was lost,
For want of a rider the battle was lost,
For want of a battle the kingdom was lost,
And all for the want of a horseshoe nail.

These lines certainly describe something big arising from something little, but I'd like to propose that what they are describing is not chaos. Rather, it is a form of unstable equilibrium—in dynamical-systems terminology, proximity to a basin boundary. That is, there are sometimes two distinct sets of states to which the state of a system will ultimately converge, and a perturbation as seemingly insignificant as the loss of a horseshoe nail, can be sufficient to change the present state from one that would proceed to one set of states to one that will proceed to the other.

Why isn't this chaos? If, after your enemy has secured the kingdom, he should also lose a horseshoe nail, the kingdom will not come back to you; nor, if you should lose a second nail, will things be any worse for you. In a truly chaotic system, a disturbance as small as a lost nail occurring at *any* time will profoundly affect the distant future. Moreover, it will generally be impossible to tell in advance whether a specific disturbance will alter the

future for better or for worse. If I were adept at verse, I might pen some lines recounting how, through some plausible chain of events, losing a horseshoe nail makes you win the battle, only to be faced in due time with more and more battles.

How new are these ideas? Certainly unstable equilibrium, even in fluid systems, had been well studied in the nineteenth century, and values of quantities like the Reynolds number and the Rayleigh number, named for some of the more noteworthy investigators, have long served as criteria for instability. The concept of chaotic behavior dates back at least to Poincaré; in a 1912 essay on chance he uses the motion of individual colliding molecules in a gas as an illustrative example, and at one point he states, “It suffices, we have just seen, to deflect the molecule before the collision by an infinitesimal, for it to be deflected after the collision by a finite quantity. If then the molecule undergoes two successive collisions, it will suffice to deflect it before the first collision by an infinitesimal of the second order, for it to be deflected after the first collision by an infinitesimal of the first order, and after the second collision, by a finite quantity.”

Today it is generally recognized that chaos is neither the rule nor the exception; some systems are chaotic and others are not. A properly thrown boomerang is presumably not; otherwise it would be unlikely to return to the thrower. There is now overwhelming evidence that the atmosphere is chaotic. What I want to do in this talk is to examine the effect that the recognition of chaos has had on the development of meteorology, and in particular on the practice of weather forecasting.

Let me begin by going back to the early 1940's, when I first became a meteorology student, and first encountered such words as “geostrophic” and “pseudoadiabatic.” It was during World War II, and I was one of a large group who were being turned into weather forecasters by the Army Air Corps. Our course of study was the regular graduate course in

meteorology at M.I.T., although what would normally take two years was being crowded into eight months. We learned the state-of-the-art methods of forecasting, which were subjective; we would analyze weather maps, identifying air masses and fronts as well as high and low pressure centers, and we would forecast by displacing these systems and adjusting their intensities, and sometimes introducing new systems or dropping old ones, according to rules based on the experience of forecasters already well versed in the art. Of course our forecasts contained the usual errors, but in post-mortem discussions our instructors were usually able to point to a feature of the analysis, such as a rapidly falling pressure at one station, which they maintained would, if considered more carefully, have led us to make a correct prognosis. In retrospect it would appear that there were usually other features which, if relied on more heavily, would have led us to make even worse forecasts, but this was not mentioned, and possibly not recognized. The big picture that we received, which was certainly compatible with military thinking, was that forecasting could be done; our failures stemmed from the intricacy of the atmosphere. All that was needed was the ability and determination to take more and more indicators into account.

Everyone recognized, of course, that the analyses themselves were imperfect, but there was no suggestion that errors would amplify as the range of the forecast increased. Indeed it is likely that in that day the amplification of errors, that is, chaos, was a minor consideration; there were vast areas, including much of the Pacific Ocean, where, because of the scarcity of observations, the analysis was little better than a guess. The mere propagation of this uncertainty into the regions of interest, without any amplification, would have been enough to make the forecast go bad after a few days.

As I have already noted, our course was the regular graduate course in meteorology. Our teachers were tops in their field, and were interested in imparting a general knowledge

that went far beyond the art of forecasting. This evidently met with the Army's approval; after all, we were to become officers, and this was consistent with the Army's philosophy that an officer is a gentleman. Naturally we had several semesters of dynamic meteorology. I had naively supposed that here we would be learning how to apply the dynamic equations to the forecasting process, but as I waited lecture after lecture and month after month, the moment never came. We were not even told *whether* the equations could be gainfully applied to forecasting, let alone *how*. It was only much later that I realized why; nobody knew.

The end of World War II saw the beginning of the age of computers. Moreover, weather forecasting was destined to become one of the first practical problems to which these new devices would be applied. By the early 1950's it became apparent that the dynamic equations could be used for forecasting, provided that they were handled carefully. Moderately good forecasts soon appeared, although they were not yet competitive with the output of a good synoptic forecaster. We should note that numerical forecasting treats the weather like a deterministic process. This is not to say that its proponents believe that the weather is deterministic; they simply feel that pretending that it is deterministic can give good results. I'm not sure that many of us at that time envisioned the day when a deterministic treatment would outperform subjective forecasting and become the standard operational procedure.

The attitude that forecasting at virtually any range was possible still seemed to prevail, although some prominent meteorologists and other scientists expressed their doubts. Generally they invoked something like unstable equilibrium rather than chaos. However, the eminent mathematician Norbert Wiener, who believed that treating the weather as a deterministic process was bad science, stated, in a 1956 lecture, "It is quite conceivable that the general outlines of the weather give us a good, large picture of its course for hours or

possibly even for days. However, I am profoundly skeptical of the unimportance of the unobserved part of the weather for longer periods. To assume that these factors which determine the infinitely complicated pattern of the wind and the temperature will not in the long run play their share in determining major features of the weather, is to ignore the very real possibility of the self-amplification of small details of the weather map.” Here we see a hint of true chaos, although it is an opinion rather than a demonstration.

Perhaps the first to tackle this self-amplification quantitatively was the late Philip Thompson, in a 1957 paper entitled “Uncertainty of initial state as a factor in the predictability of large scale atmospheric flow patterns.” He began by deriving equations governing the statistical properties of the errors—differences between true and assumed states. As is usually the case when one starts with nonlinear equations and derives equations for averages and other statistics, he was forced to introduce some auxiliary assumptions to make the equations tractable. This he did by introducing typical horizontal scales for the synoptic features being predicted and for the error fields. He found that with the existing density of observing stations, small errors would double in amplitude in about two days, but he noted that if the density of the stations could be doubled—a task that he felt could be prohibitively expensive—the resulting reduction in the scale of the errors would virtually eliminate the error growth.

There was therefore little evidence for chaos as we currently perceive it—error growth that cannot be eliminated—but if someone had jumped in where Thompson stopped, and simply noted that both the synoptic field and the error field were multi-scale, possessing continuous spatial spectra, which would inevitably overlap, he or she would have encountered error growth in all cases, and chaos, by whatever name would have been given to it, might have become recognized as a meteorological phenomenon.

Let me jump a few years ahead to my own first encounter with chaos—one that I was not anticipating. I was interested in testing a proposed forecasting procedure, which I believed would prove somewhat deficient, and for this purpose I needed a system of equations whose general solution was not periodic. It appeared that the way to identify such a system would be to find explicit solutions numerically. Computers were becoming ubiquitous, and, in 1958, I acquired a small computer for my own use. Of course in those days you couldn't go into the neighborhood computer shop, which didn't exist, and buy a PC, which didn't exist, but the Air Force, which was sponsoring our research, did allow us to rent one, and it paid the bill of a few thousand dollars a month. Working at a speed about a thousand times as fast as hand computation, and incidentally about a million times as slow as some of today's PC's, I tested system after system and finally found a suitable one with twelve variables; it was a truncated version of the standard two-level quasi-geostrophic model.

At one time I wanted to rerun one of my solutions. After typing in as initial values some numbers that had previously been printed out as intermediate values, I left the office for a couple of hours while the computer simulated two months of weather. Upon returning I found that the earlier results were not being repeated. At first I suspected computer trouble, but, in looking for the place where things went wrong, I found that the difference between the earlier and the later solution was amplifying at a fairly uniform rate, doubling in about four simulated days. I then realized that I had chosen different initial conditions; the new ones were the printed-out versions of the older ones, which had been rounded from six to three decimal places. There was no computer trouble; the round-off errors simply grew until they drowned the signal.

Here was chaos. I soon realized that if the real atmosphere behaved like the model, long-range forecasting would be impossible, since there was no conceivable way of getting analyses devoid of errors at least as large as round-off errors. I lost no time in conveying my results to some of my colleagues.

Although the principal findings of subsequent studies of chaos have been well documented, it is not at all certain how the attitudes of the meteorological community, and particularly of the forecasters, evolved over these years. What I have to say on this matter must therefore be considered an educated guess—perhaps a scenario.

I suspect that if my result had become generally known, but if there had been no follow-up studies, forecasters would have made remarks ranging from “Oh, how interesting” to “I don’t believe it.” Actually it was my good fortune to have as a colleague Jule Charney, who did believe it. In preparing a report for the Global Atmospheric Research Program, he recounted some of my results, and noted that one of the hoped-for benefits of the program, the eventual production of good two-week forecasts, might prove unattainable. He, more than anybody else, was instrumental in persuading some of those who worked with state-of-the-art numerical-prediction models to use these models for “predictability experiments,” in which two or more runs originating from slightly different initial states would be compared. Thus, in 1965, Yale Mintz, working with a quasi-geostrophic model with several hundred variables, found that small errors tended to double in about five days, while, in 1969, Joseph Smagorinsky, using a primitive-equation model with a few thousand variables, reduced the doubling time to about three days. By the 1980’s predictability experiments, which had become numerous, were suggesting two days, while a definitive study in the nineties dropped the figure to a day and a half. The reasons for the continual decrease remain controversial,

but my own opinion is that they stem from the increasing horizontal resolution of the models.

I want to emphasize that I do not believe that my result, or anybody else's similar result that could have impressed the meteorological community, could have been obtained if computers had not appeared on the scene. Perhaps I *could* have ground out a recognizably nonperiodic solution of the twelve equations by hand, in about the time that it would have taken to write up the results afterward. The point is that I would have been unlikely to start with the twelve equations; actually I tried several systems, each with several different sets of numerical values of the adjustable constants, before encountering something suitable; hand computation would have required years rather than months even if I had dropped all other activities, and, with no assurance that a suitable system even existed, I would soon have discontinued the job. Even if I had found a nonperiodic solution by hand, I would have had no reason to make a rerun with rounded-off initial conditions.

Returning to the scenario, I suspect that when chaos appeared in models that looked more like the atmosphere, forecasters began to take notice. The more realistic the models became, the more convinced the forecasters became. With today's operational models with several million variables, virtually all doubt has disappeared.

How did anyone know that the newer and newer models were more and more realistic? They produced better and better forecasts. We are thus faced with the perhaps unexpected conclusion that the better our forecasts become, the more strongly we believe that we cannot forecast as well as we would like to.

What has been the effect of awareness of chaos on meteorological operations and research? Presumably there was no general feeling of discouragement; the first results had to await the appearance of moderately advanced computers, which by then were producing

moderately good forecasts. The ostensibly most discouraging results, those that indicated the shortest doubling time, had to await computers many orders of magnitude more powerful, and these were producing forecasts of a quality not seen earlier.

During the 1980's the principal positive effect may have been the recognition of what might be predictable and what was probably not. Meteorologists were thus aided in allocating their resources and directing their efforts to endeavors that were most likely to bear fruit.

The 90's were the decade when operational ensemble forecasting became a reality. However speculative some of my opinions have been, it seems certain that ensemble forecasting would not have been introduced had not a goodly number of forecasters trusted the predictability experiments. In ensemble forecasting, one makes not just one forecast at a particular time but a whole collection of them, each one originating from a state differing only slightly from the original analyzed state. Before it became operational, ensemble forecasting was sometimes called "Monte Carlo forecasting," taking its name from the famous gambling resort because, among the infinite number of states that resembled the analyzed state and might have been selected as ensemble members, a few were to be chosen at random. The current practice, however, is to use one procedure or another to select the states systematically.

At short range the separate ensemble members may look much alike, but after a few days, when they have diverged, they will constitute alternative forecasts. Features that appear in all or most members may be predicted with fair confidence, while those that appear infrequently are suspect. If the ensemble is reasonably large, perhaps with a hundred members, a simple count of those where a specific event, such as rain in Boston, occurs may yield an acceptable probability forecast. The possibility remains, of course, that the separate

ensemble members may look more like each other than any one of them looks like what actually happens. This danger can be minimized by continually improving the forecasting models and the analysis.

It is apparent that forecasting with large ensembles requires a vast amount of computer time. With the promise of ever more powerful computers, we might as well put the power to use.