MET ÉIREANN
The Irish Meteorological Service

Historical Note No. 2

LEIPZIG—BERGEN

Jubilee address on the 25th anniversary of the Geophysical Institute of the University of Leipzig (1938)

by V. BJERKNES

Zeitschrift für Geophysik **14** (3/4) 1938

Translated from German, with an introduction

by LISA SHIELDS

Met Éireann, Glasnevin Hill, DUBLIN 9, Ireland

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Vilhelm Bjerknes (1862–1951)

Vilhelm Bjerknes was born in Christiana (now Oslo), Norway, the son of the mathematician Carl Anton Bjerknes. He trained as a physicist and worked with his father on hydrodynamic research until his final year of study at university. After studying in Paris and a spell in Bonn he became professor in Stockholm, and later at Christiana. During this time he continued developing his father's ideas. In 1905 he visited America and received support from the Carnegie Foundation to work on the problems of weather forecasting, using data from the recently organized upper-air ascents.

In 1912 Bjerknes accepted a chair in Leipzig in charge of the newly founded Geophysical Institute. In his inaugural lecture he took pains to emphasize his theoretical approach and unwillingness to be distracted by practical considerations. All their energy and resources were to be concentrated on purely theoretical research: a combination of hydrodynamics and thermodynamics applied to the problems of forecasting. Bjerknes was an unusually charismatic figure who inspired great enthusiasm and zeal among his co-workers. They continued very productively until the outbreak of World War I, when most of the staff were taken by the army, and physical conditions became worse and worse.

In 1917 Bjerknes returned to his native Norway (a neutral country) to take up a chair in Bergen. There, with his his son Jacob, he was joined by the Swede Thor Bergeron and his original Carnegie assistants (T. Hesselberg and H.U. Sverdrup). The Bergen team continued the theoretical work started in Leipzig, but because of wartime constraints were forced into a good deal of practical forecasting for fishermen and farmers. One result of this was that Bjerknes succeeded in getting Government funding for a remarkably good dense observational network. The success of their frontal theory made the 'Bergen school' of forecasting internationally famous. Their achievements have been very well documented — notably by Friedman (1989) and also by Nebeker (1995), Bergeron (1981) and by various authors in Grønas and Shapiro (1994) and in the centenary volume of *Geofysiske Publikasjoner* (1962).

It has been suggested (Bergeron 1981) that Bjerknes with his scientific optimism inspired L.F. Richardson towards his ambitious attempt at numerical forecasting. The poor result of Richardson's forecast would not, however, have given Bjerknes much encouragement to progress from a map-based forecasting technique to a purely numerical one.

The particular interest of the speech here translated is that it explains the very close relationship between the work initiated in Leipzig and its completion in Bergen.The high-flown optimism of Bjerknes's address seems strange in retrospect — it was delivered five years after Hitler's coming to power, not so long before the beginning of the second World War. The recent upsurge of interest in the historical development of scientific weather forecasting justifies the publication in English of this little-known speech.

Lisa Shields, Librarian / Translator, Met Éireann, January 1997

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Bergeron, T. (1981) 'Synoptic meteorology: an historical review', *Pure and Applied geophysics*, **119**, 443–473.

Friedman, R.M. (1989) *Appropriating the weather: Vilhelm Bjerknes and the construction of a modern meteorology*, Ithaca and London, Cornell University Press.

***Geofysiske Publikasjoner*, 24** , Oslo (1962) 6–37.

Grønas, S. and **Shapiro, M.A.** (1994) editors, *The life cycles of extratropical cyclones: proceedings of an international symposium, Geophysical Institute, University of Bergen, 27 June–1 July 1994, Bergen – Norway* , Vol. 1, Bergen.

Nebeker, F. (1995), *Calculating the weather: meteorology in the 20th century*, San Diego etc., Academic Press.

Leipzig —Bergen. Jubilee address on the 25th anniversary of the Geophysical Institute of the University of Leipzig

by V. Bjerknes

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It is a great honour and a great pleasure for me, at this jubilee celebration, to speak about the Geophysical Institute that I had the privilege of establishing twenty-five years ago. Allow me to give a brief account of the scientific situation from which the foundation of the Institute emerged, to outline the special plan that I was following at the time, and to describe briefly the resulting developments.

At the time of the foundation of the Institute I was working towards the solution of one particular problem—the problem of forecasting the weather. The problem is as old as the observation and thinking of mankind. From the outset this thinking was riddled with all kinds of superstition. Even today not all traces of this superstition have disappeared. The following quotation from Herodotus gives an excellent picture of the conflict between superstition and the awakening critical sense:

> The storm lasted three days. But by dint of sacrificing prisoners to the winds, by ingratiating themselves with them by swearing oaths, and by making sacrifices to Thetis and the Nereids, the sorcerers eventually succeeded in calming the storm four days after it had started.
>
> Or [adds Herodotus] — perhaps the storm abated by itself.

From earliest times there was a strong belief in the direct intervention of supernatural powers. Nevertheless, those people who depended on the weather—sailors, fishermen, mountain dwellers and farmers—were not entirely given up to superstition. They had made their observations, they had applied their thought, they had observed the course of successive weather conditions and noticed how similar situations had similar results. Possessing a great store of mental images of previous weather situations and their consequences, they had become competent, practical meteorologists. In the Norwegian book *The King's mirror* written around 1250, at a time when there was still a lively traffic with Iceland, Greenland and even Vinland (i.e. North America), we read the following passage:

> Long sea journeys are best made in the summer half of the year. But shorter journeys, such as over to England, can be ventured in any season. For these one only needs to know if the weather will hold for a few days, and that is not difficult to judge for those who understand the weather.

As long as only visual observations were available, it was not possible to give more than a qualitative description of the state of the atmosphere. Then, around 1600, Galileo invented the thermometer. A generation later his pupil Toricelli invented the barometer. People began to find out about atmospheric pressure, and observed the connection between pressure changes and changes in the weather. However, nothing much could be achieved as long as observations were available only at one place. Just over a hundred years ago hardworking scientists, especially Brandes here in Leipzig, began to assemble observations from a great number of different places and to plot the representation of the weather situation on *the first synoptic charts*. But the observations, sent by post, came too late to be of use for practical weather forecasting. When a telegraphic network began to be set up, the idea of a *telegraphic weather service* had to be mooted.

Governments react all too slowly, however, to scientific stimuli. Then came more powerful arguments. On the 14th of November 1854, during the Crimean War, a storm in the Black Sea caused great damage to the fleets of the allied western powers, and the French liner Henry IV went to the bottom. Then science was consulted. Leverrier, the famous discoverer of the planet Neptune, was asked to hold an inquiry as to whether the storm could have been forecast. He assembled observations from all over Europe for the days before the storm, plotted synoptic charts and ascertained that the storm could have been forecast if there had been a telegraphic weather service. The day after this report was presented to the Emperor Napoleon III, Leverrier was instructed to set up a telegraphic weather service for France. Most of the other civilised countries soon followed this example, all the more because in some of them, especially England and the USA, introductory steps had already been taken. There resulted two independent weather networks, a European and an American. On both sides they began with false expectations.

The methods of meteorologists after the introduction of weather maps were not in principle different from the methods of the old folk weather prophets: both worked with mental pictures. The folk prophets had mental pictures of what they saw in the sky, while the meteorologists had mental pictures of what they saw on the weather charts. The discovery of the highs and lows drifting over the weather maps was a distinct success for the forecasters. They hoped soon to find regular patterns in the movement of these formations, corresponding to those of the heavenly bodies. Initial failures did not have a discouraging effect: anything unexpected that happened must have had its causes outside the area of the still limited charts. With the expansion of the telegraph network everything would improve. However, in spite of all the expansions of both telegraph networks they were still not masters of the unexpected events. They consoled themselves nevertheless with the thought that as soon as a *transatlantic cable* joined both networks in one, they would be able to forecast all the storms that reach the European harbours. It may seem surprising, but this very optimism of the meteorologists was one of the most effective arguments for the laying of the first transatlantic cable. But even the cable did little to help them.

Still the meteorologists did not allow themselves to become discouraged. They concluded that the secret of the weather must lie over the oceans and it is from there that the weather is controlled. If, therefore, they assembled weather reports from ships' logs, and plotted daily synoptic charts for the sea, they would understand the relationship in a purely scientific manner, even if these belated weather maps could not be of direct use to practical weather forecasting.

Thus, the so-called Hoffmeyer maps for the Atlantic were produced by the Danish Meteorological Institute and the German Naval Observatory (*Deutsche Seewarte*) in collaboration. After the meteorologists had had these charts on their desks for some years, they gave up all hope. As the doyen of English meteorologists, Sir Napier Shaw, explained to me '*They could not understand the language of the weather maps*'. The pioneers of meteorology, who had started out with such great dreams, began to feel discouraged. There was no hope for the progress of weather forecasting. They allowed the weather service, which had already been set up, to run on mechanically, serving day-to-day needs. And their institutes, specially established for forecasting the weather, turned their scientific interests towards a more placid branch of meteorology, namely climatology.

At the turn of the century, however, there was to be one last venture. So far observations had been produced only at the surface of the earth. Could it be that the weather was controlled from above? If so, upper-air observations had to be made. Attempts were made in Germany with free balloon ascents, in America with kite ascents, in France with registering balloons. Soon in various countries *Aerological Institutes* using all three methods came into existence. Hergesell succeeded in organising the upper-air ascents internationally, so that on certain days or in certain weeks as many simultaneous ascents as possible would be made in the participating countries. This aerology [the study of the free atmosphere] brought us much new knowledge of our atmosphere. First, the *stratosphere was discovered.* But the secret of the weather was not found even in the upper air. The upper-air observations did not initially have much effect on weather forecasting; even with the use of these new observations the language of the weather charts could not be understood.

Where was this language to be learnt then? To consider this question more closely, we have to look back into the history of science, and follow the threads of the development of the *fundamental sciences* themselves.

Galileo, who gave empirical meteorology the thermometer, also initiated the development that led to the foundation of mechanics and physics. His law of falling bodies led to the building up of mechanics, the theory of the motion of bodies. Newton gave the principles of this theory their definitive form. Building on Newton and Galileo, Euler developed the special science of the movement of liquid and gaseous bodies—*hydrodynamics.* The hydrodynamic equations give, in implicit form, the relationship between two of the fundamental meteorological values: air pressure and air motion. Then in the second half of the last century, through Mayer, Helmholtz, Carnot, Kelvin and Clausius, there followed the development of the science of thermodynamics. Allied with hydrodynamics, thermodynamics gives additionally, in the same implicit form, the relationship of air pressure and air motion with air temperature. The combined *thermo-hydrodynamics* must be our textbook for learning the language of weather charts.

But progress in this direction was slow. Mathematicians were already finding pure 'classical' hydrodynamics, separated from thermodynamics, difficult enough and yet very beautiful in its classical austerity. This hydrodynamics reached its peak in two famous theorems: Helmholtz's theorem of *vorticity conservation* and Kelvin's theorem on the *conservation of circulation*. But nothing could be done with these theorems in meteorology, where observations showed us a perpetual formation and destruction of circulations and of vorticity changes. It would be a hopeless task to approach the more general field of thermo-hydrodynamics: so all hydrodynamicists thought, and I thought so as well.

I became a hydrodynamicist in the school of my father, who in his time had studied hydrodynamics in Göttingen, with the great mathematician Pierre Lejeune Dirichlet as his teacher. Continuing my father's work, I was led, compelled as it were, towards the almost inevitable generalization of the theorems of Helmholtz and Kelvin. These theorems included the *Principle of the formation and destruction of circulation and vortex motions.* Without having initially noticed it, I had crossed the frontier between hydrodynamics and thermodynamics. The theorems turned out to be directly suitable for a discussion of the basic kinds of meteorological phenomena.

The successes achieved led me to the thought: *Perhaps the time is ripe to combine observational meteorology and theoretical thermo-hydrodynamics, which up till then had been developing in isolation.* And that is best done, thought I, by boldly and directly addressing the central task of meteorology: *that of deducing the future state of the atmosphere from its present state.* In other words, *the problem of weather forecasting as a mathematical problem* had to be stated and, if possible, solved.

Mathematical formulation of the problem is in principle simple. The state of the atmosphere can be defined through five variables: one vector variable: velocity, and four variables of a scalar nature: pressure, density, temperature and humidity. And we possess five corresponding equations: the vectoral hydrodynamic equation of motion and four scalar equations, one for conservation of mass, one for conservation of energy, the gas equation and the thermodynamic equation for the processes of condensation and evaporation. Proceeding from the given initial conditions and the conditions of external influences, the future state of the atmosphere is then determined mathematically. We are thus implicitly in possession of the knowledge needed to solve the problem of weather forecasting. A way has to be found to make this knowledge explicitly useable.

To this end we present the problem in the simplest possible form, by asking only for a forecast for *very short intervals of time.* In the short interval the air masses are only slightly displaced and undergo only slight changes in their state. The problem can thus be reduced to two calculations which are, in principle, simple: *the calculation of the new positions of the displaced air masses*, and *the calculation of the physical states in which they arrive there.* In these calculations, however, all the moving air masses have to be taken into consideration, the high as well as the low, the ones over the sea as well as the ones over land — the equations do not contain preferred places, from which the evolution of the flow is controlled.

Since all the air masses are involved, all the demands concerning the observations can never be completely met. One has to create a three-dimensional observation network which is as extensive and dense as possible, and then make the best possible use of the observations.

The raw material of the observations is, therefore, always incomplete. It is also much too disorganized to be of direct use for the forecasting programme. Using this raw material one first has to draw up pictures of the current atmospheric situations which are clear, operationally useable and, especially, as *accurate* as possible, and one has to draw all possible conclusions from these observations. That is the process of *diagnosis,* a problem of the greatest proportions; its methods are still to be established and practised with the finest discrimination. When diagnosis has been accomplished, there follows the problem of *prognosis* according to the procedure mentioned already: calculating the new positions of the air masses and finding their physical states when they arrive.

I published this programme of meteorological research in 1904 in the journal *Meteorologische Zeitschrift*—perhaps really more to get rid of the problem than to tackle it myself. I did not consider myself to be the right man for the job. I had no training as a meteorologist, and the problem was a colossal one, for which help was necessary. But when in 1905 I had been invited to America to lecture on my old hydrodynamical work and on my father's work I had the opportunity also of giving a lecture in Washington, in which I explained this meteorological research programme.

The programme excited the interest of the Americans, perhaps because of its boldness and colossal dimensions. The Carnegie Institution of Washington, which had then just been set up, decided to give financial support to my work on this programme. This was also done very generously: from the year 1906 I had, thanks to an annual grant, the possibility of taking on one or more personal scientific assistants.

Thus my fate was sealed. I entered, as an interloper, into a science strange to me, meteorology. With my Carnegie assistants I immediately began to work out the basics of the forecasting problems, by applying all the available observational material of the days of the upper-air ascents.

Around this time the legendary Otto Wiener, later to be my colleague in Leipzig, began to take a lively interest in my work. He wanted to see it transferred to Leipzig, in connection with the setting up of a specialized institute. In the year 1912 I received the call which did me such honour. The Institute was named *Geophysical* after the already existing institute of that name in Göttingen.

The name presented the opportunity or perhaps the duty of including all branches of geophysics in the programme of work, if not immediately at least in due course. Wiener's original plans were also heading more or less in this direction. But I was determined to apply all my efforts to the single great problem, in the hope of reaching a breakthrough. For that reason I put everything else aside for the time being. I was invited to take over the seismograph which was already working in the cellar of the Geological Institute. I declined the offer. Equipment for upper-air soundings was proposed. I answered that, rather than setting up a second Lindenberg Observatory, I would prefer to establish the first institute to apply strict theory to meteorology. I also wanted to emphasize the direction in which the work was moving by inaugurating a series about the diagnoses of atmospheric conditions in order to provide a basis for future forecasts.

In the autumn of 1912, as a preparation, I had already arranged for the man designated as principal assistant of the Institute, Dr Wenger, to come to Oslo. This was so that he could draw up the plans for the publication and begin working out the first diagnosis, together with my Carnegie assistants of the period—Dr Hesselberg (now Director of the Norwegian Meteorological Institute) and Dr Sverdrup (later famous as a polar explorer with Amundsen, then professor in Bergen, and now engaged in a prestigious oceanographic undertaking in the USA).

On the 1st of January 1913 I arrived in Leipzig, and the asistants arrived shortly after that. Work started in the old premises in Nürnberger Straße. Soon there were twelve post-graduate students there, initially occupied with exercises and then with their doctoral studies, all related in various ways to the main problem.

In my inaugural lecture on the 8th of January 'Meteorology as an exact science', I expounded my plan of work, *emphasizing most strongly that I was attacking the forecast problem as a purely scientific one.* I did not want to be distracted by thoughts of a directly practical application. I concluded my lecture with the following words:

> I would be more than delighted if I could take my work so far that my years of calculations permitted me merely to forecast the weather from one day to the next. If only the calculation were to be correct, then the scientific victory would have been won. Meteorology would then have become an exact science, a real physics of the atmosphere. And if we had achieved only that, the practical consequences would soon become apparent as well.
>
> It can take years to drill a tunnel through a mountain. Many workers will not live to see the day of the breakthrough. But that does not prevent others from being able to travel through the tunnel later at the speed of an express train.

So much for the program with which the Institute began.

The first issue of the Institute's publication 'Synoptic representation of atmospheric conditions over Europe' deserves special mention. Because of international collaboration, meteorology has, unfortunately, a *political* side. Success in theoretical work necessarily demands a rationalization of the units used in meteorology. Here there reigned a dreadful confusion. In the year 1912 the International Aerological Commission had sat in Vienna, and there I succeeded—not without a struggle—in getting the Commission to recommend the use in aerology of rational units taken from the c.g.s. (centimeter-gram-second) system. But now it was a question of defending the reform recommended by the Commission before a higher authority in the international meteorological hierarchy at the session of the International Meteorological Committee which was to be held in Rome at Easter, 1913. Thanks to the efforts put into their work by all participants, I was able to bring the first issue of the Institute's publication with me to Rome. With the use of this issue I could present clearly the advantages of the rational units and the disadvantages of the irrational ones. Once again, it was not without a struggle that victory was gained. In fact it was the first decisive victory in a long campaign that eventually led to the adoption of completely rational units based on the c.g.s. system, not only in aerology but also in general meteorology. It is worth noting in this regard that the millibar is the only unit of the metric system that has been introduced all over the world.

Success gave 'strength through joy' (*Kraft durch Freude*), if I may so express myself. Work at the Institute continued with increased zeal. Each trainee in turn was assigned a particular international day to work on, after the model of the first volume. In this way several volumes were produced, all fairly similar to one another at first. However this uniformity was not our final aim. The first analyses aimed only at finding a suitable framework into which the as yet unknown new findings were to be fitted, as and when they appeared. In consideration of this, a *large map format* was chosen and expansion planned. For the surface charts in particular not only the directly available telegraphic material of the daily weather maps, but all available observations would be assembled and employed.

I still ask myself: 'What would have resulted if we had been able to continue this programme? The great atmospheric layers of discontinuity and their lines of intersection with the earth's surface could hardly have eluded us. The whole 'polar-front meteorology' or 'air-mass meteorology' would then, as far as I am able to judge, have come into being in Leipzig. But fate had decided otherwise. War intervened. One after another the doctoral students were conscripted, five to perish. One of these was H. Petzold, who had had as his doctoral thesis an investigation of convergence lines that would perhaps have paved the way for polar-front meteorology if it had not been interrupted. Then came the turn of the Institute's assistants. Eventually there were at work only two women and my two still very young Norwegian Carnegie assistants. One of these was my son, J. Bjerknes, now professor in Bergen, and the other was H. Solberg, now professor in Oslo.

Under these circumstances an expansion of the work was unthinkable. In hope of better times, we tried simply to continue the work already started as best we could. The reduced format of the publication indicates our more modest pretensions. However, I must mention a special activity from this time of setback which contained the germ of something valuable. In the seminars held regularly at the Institute during its brief heyday, Helmholtz's 1888 paper *On atmospheric movements: Part I* was presented not once but repeatedly. Repeatedly, because the speaker thought that he had discovered an error in Helmholtz's calculations. As a result I myself was led to a thorough study of this paper, which is full of ideas but obscurely written. The mistake lay with the speaker and not with Helmholtz. But if we read the paper on the basis of our present knowledge, it is in fact very interesting. Helmholtz had, so to speak, *seen the 'polar front' in his mind's eye,* albeit in a schematic form, as a simple parallel (of latitude) that continually takes shape only to be annihilated by vortical motion. This 'polar front' did not make any deep impression on me at the time: it was not visible on the daily charts, and apparently Helmholtz himself looked for it in vain on Hoffmeyer's *charts* of the Atlantic Ocean. But at the end of the paper, in connection with this polar front, he drew attention to the fact '*an eddy must always begin with a wave formation*'. It could appear from this that he had anticipated the wave theory of cyclone formation. But this was certainly not the case, for in the same paper he speaks of cyclones and anticyclones as independent formations, without connecting them to his polar front. And in the two following papers, where he develops the theory of Helmholtz waves formed as a result of instability, he makes absolutely no mention of cyclones. He was obviously thinking only about small eddies.

But Helmholtz's obscure words roused in me the question: can there be a connection between waves and the large eddies that we call cyclones? *Can cyclones begin as waves?* On the one hand the inevitable conclusion seemed to me to be that every disturbance must start off small, precisely in the form of a wave, as Helmholtz said. On the other hand, it seemed to me that the leap from a wave to a cyclone was inconceivably great; it was only after the empirical discovery of the polar-front layer that this paradox was to be resolved. But the paradox would not leave me in peace. I hoped to find an explanation by examining all possible kinds of waves, and I began by dealing with the theory of internal gravity waves; an introductory paper appeared in the *Berichte der Leipziger Akademie*. But work became harder and harder for me. It was the 'Swede turnip winter' (*Kohlrübenwinter*) of 1916–17 and there was no more nourishment to be had to maintain intellectual powers.

This was the situtation when, in the spring of 1917, I was offered a chair in Bergen in Norway. This embarrassed me greatly. On the one hand I asked myself if it was right to forsake the newly founded Institute at a critical time. On the other hand, could I do any more in Leipzig for the solution of the task which I had come to do? And when I had succeeded in getting my first assistant and real co-founder of the Institute, Dr Wenger, recalled from his service as army meteorologist to replace me as Professor and Director of the Geophysical Institute, I decided to return to my native land. In August 1917 I moved from Leipzig to Bergen. We agreed that, as soon as better times came, an intimate Leipzig-Bergen collaboration should ensue, on the implementation of the Leipzig programmes.

What could I now achieve in Bergen? Wenger had the intention, as soon as the necessary working conditions were restored in the Geophysical Institute, to take up work on three-dimensional diagnosis. There was no point in duplicating this work in Bergen. As well as that, everything in Norway was abnormal because of the war. The old Weather Service was functioning quite imperfectly. The basic telegrams from abroad were not coming in during the war. But such a weather service was more desirable than ever. For all supplies from abroad were hindered, and the vital business of agriculture and fishing needed every conceivable support. I had to consider carefully whether it were not my duty to attempt something in a practical direction. I had severe misgivings. I had had no training as a practical meteorologist. In Leipzig I had, accordingly, done my utmost to emphasize my purely scientific and even wholly unpractical aim. Furthermore, in the absence of basic weather telegrams from abroad we could expect only a very limited success. From theoretical considerations, however, and supported by my experiences in Leipzig, I thought I could count on the possibility of a limited success for very short-term forecasts, provided that we could install in our own country *a sufficiently dense observation network, so as to bring the weather under the microscope, so to speak.* And there were also scientific novelties to be hoped for, especially as we had before us the weather on the stormiest and most meteorologically eventful coast of Europe.

The partial mobilization of neutral defense forces was a favourable circumstance. Our navy had set up observation stations on a close string of the outermost islands, where experienced seamen observed everything occurring in our waters. These posts were equipped with binoculars and with an azimuth dial so as to be able to find out, by combining the readings from two locations, if anything illegal was taking place in our waters. As a first introductory attempt, my immediate aim was to get these stations to send weather information to Bergen three times a day by telephone. The information in question comprised wind direction from the azimuth instrument, wind strength and present weather as judged by the seamen. When the Government later put some means at my disposal an observation network was also set up in the interior of the country, *ten times as dense as the network which existed before the war.* Although the instrumental equipment of the newly set up meteorological stations was meagre, seldom had a more valuable set of stations existed than the ones along the coast. For all the stations were of the highest grade, what we now call *representative,* that is, not influenced by local conditions.

In this station network the lines of convergence, which had already been studied in Leipzig, could be investigated more closely and related to changes of temperature and of weather. The allied concepts of the *line of discontinuity* or of the *front*, which were proving very fruitful for the understanding of weather events, were developed. Of course the fronts were by no means new in meteorology. Singularly strongly developed cold fronts had often been observed and described (Durand-Greville, Köppen, v. Ficker et al.) But, thanks to increasingly sharp analysis of weather charts, observed lines of discontinuity, which previously made rare appearances on the weather charts, now occured there as daily visitors.

In this way the reality of 'polar-front' or 'air-mass' meteorology—now so well known to us—was gradually discovered. We became familiar with the great atmospheric layers of discontinuity which, according to the direction of their movement, manifested themselves at the earth's surface as 'cold fronts' or 'warm fronts'. It was noticed that cyclones originated in a a wave-shaped depression in a layer of discontinuity. The depression deepened, with the result that the original wave, passing through Bergeron's *occlusion stage*, gradually transformed itself into a horizontal vortex. The paradox of the wave origin of cyclones was explained and we were able to begin the difficult elaboration of the mathematical theory of cyclone formation. And finally, practical weather forecasting, which had developed on this basis, ran itself almost automatically according to the programme which had been set up at the beginning: namely to determine the changes of position of moving airmasses and the physical states which they reach. All this, however, was from quick estimates, rather than after the years of calculations as I had proposed in my inaugural speech in Leipzig. The mental pictures still remained useful, but were no longer the most important thing. Even if the tunnel was not finished, it was at least possible to travel through it.

That is not to say that the problem has been solved in a definitive, exact way. Since however the programme formulated for the mathematical problem will, in principle, provide the solution, the prospects for further development in the exact direction are promising. We are moving more and more away from the 'art' towards the 'science' of weather forecasting. No matter how future developments will come about, it is already now a reality that the new methods of weather analysis and weather forecasting are spreading throughout the world, from country to country, from continent to continent, from the northern hemisphere to the southern, from east to west.

I now come to the question: *how great is the contribution of the Leipzig school to this revolution in meteorology*? First I remark that there has sometimes been talk about the conflict between the old Leipzig school and the subsequent Bergen school. As the person who probably knows most about the matter, I can say that no such conflict ever existed. One school was the direct continuation of the other. Only the work was forced to take on different appearances.

Furthermore, nothing about the respective contributions of the two schools can justly be concluded from what has been published in the literature. Work in Leipzig broke down as a result of the [First] World War, before final results of a general nature were obtained, let alone published. And it was also the case that what did exist very incompletely in published form in Bergen, did so for a different reason. Our young scientists were working as practical meteorologists, and were so very useful, particularly for the fishing population, that they were rewarded for it increasingly with an overload of 'duty' work. They were left very little free time to publish papers describing the new results abundantly pouring in, or to provide evidence for the results provisionally published, let alone reply to the attacks which did not fail to appear. The results of the Bergen school became well known, not so much from their published papers as through the ever more numerous visits of foreign meteorologists to Bergen, and through the resulting invitations to the Norwegian meteorologists to work for shorter or longer periods in various large meteorological institutions abroad.

Therefore, no proper judgement can be passed, on the basis of documents available in print, on the respective contributions of Leipzig and Bergen to the final result, nor even on the personal credit due to individual participants. This is because the revolution in modern meteorology proceeded in an altogether extraordinary fashion. Only someone who has himself experienced it all is reasonably qualified to judge this. And, when all is said and done, I am the only one who has personally taken part in it *all*, experienced it *all*. As such, I can say that, however slight the contribution of Leipzig to the final result in Bergen may appear when viewed from outside, yet this contribution is not only great, but has been indispensable for the final result.

As for my daring deed in Bergen, where I set up a weather service in such difficult circumstances, I certainly could not have envisaged it had I not had five years of work in Leipzig behind me. Indeed, neither could I have risked it had I not had two collaborators at my disposal who, when they were in Leipzig, had been thoroughly immersed in the Leipzig ways of thought—namely my two Carnegie assisants mentioned earlier. I must add here that a third person joined us in Bergen who was immediately stirred by the prevailing ideas and who threw himself into the work with enthusiasm, burning zeal and penetrating intuition. This was the Swede, Bergeron, principally known as the discoverer of the occlusion. These three created the new Weather Service.

Here I make an important digression. The branching out of science results in ever more widely differentiated demands on the intellectual qualifications of scientist. It is not possible to entrust every scientist, no matter how high his standing, with work on a weather chart, which demands a great deal of responsibility. The modern meteorologist has before him a working chart in which the raw material of observations includes many thousands of different numbers and symbols (on the German weather chart at present about 10,000).

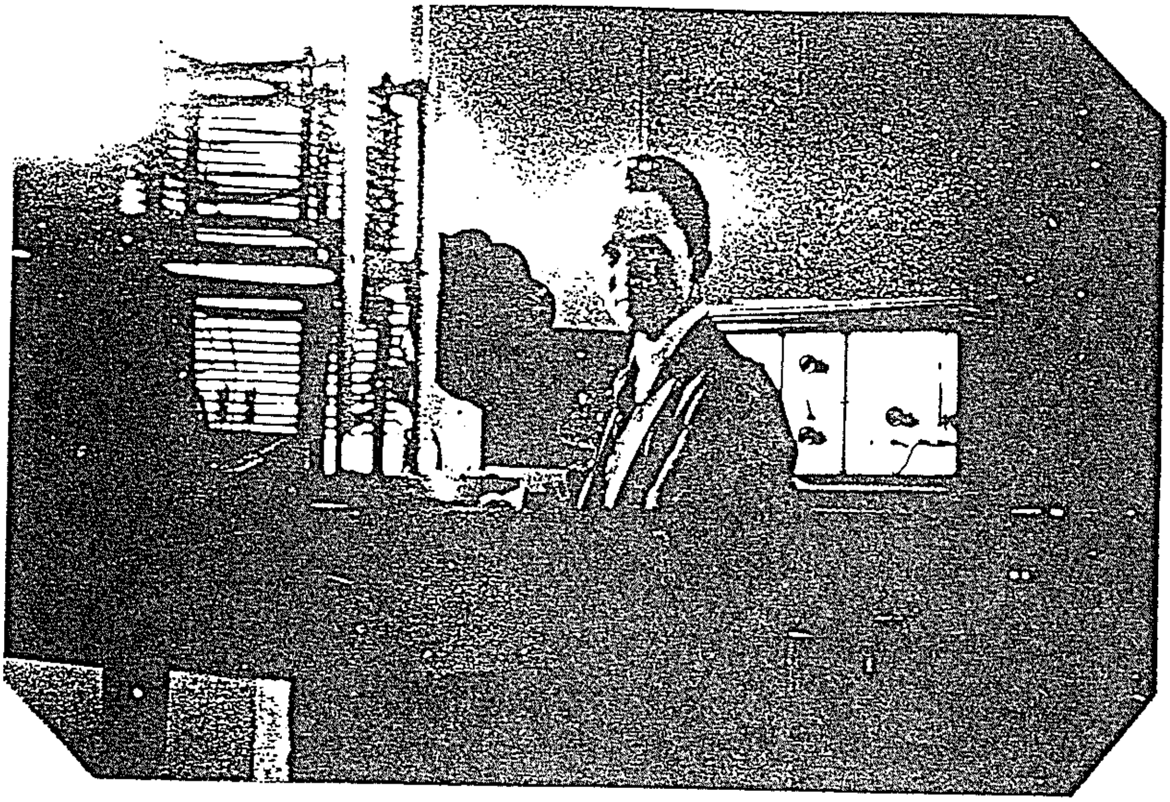
The picture of the weather situation that the meteorologist deduces should not conflict with a single one of the observations regarded as reliable. This job calls for two indispensable qualities: highly developed powers of combination and considerable ability to envisage things three-dimensionally. Even mathematicians do

Allegaten 33, Bergen. The Bjerknes family lived on the lower floor, and the meteorologists worked on the upper floor.

Thor Bergeron remembered that Vilhelm Bjerknes, who was a generation older than his Bergen school collaborators, had once complained (in 1919) that he was a 'poor old buffer', as he so often had to go down into the cellar and shovel coal for warming the little house. His young colleagues were so intensely engaged in their work thaat they did not notice the cold.

– Bengt Dahlstrøm, *PAGEOPH* 119 (1980/81) p. 550

Tor Bergeron (L) and Jacob Bjerknes and an assistant at work, Allegaten 33, 1919



Jacob Bjerknes in the electrostatics laboratory

Jacob Bjerknes working at his weather maps, c. 1920 not always possess both these aptitudes, and there are scientists who are weak in both respects and who yet achieve good results in their sciences. I came across one case in which a young man with remarkable gifts for instrument making and the techniques of physical experiments proved to be quite unable to work at weather maps. Others can acquire the methods of weather analysis through hard work, and become dependable technicians in the subject. But only those privileged ones who combine the aforementioned special gifts together with intuition and creative imagination can lead the way scientifically in this field. In the hands of such researchers the modern weather map has developed into a fundamental—*non-material—instrument of atmospheric physics*, corresponding to the material instruments of laboratory physics. I do not know whether I myself in my younger days had the combination of gifts necessary for working creatively on the weather map. But it soon became clear to me that I could not compete at all in this respect with my young collaborators. Therefore I found it really advantageous for success to entrust these collaborators with the empirical work on the weather map and, while following their results eagerly, to leave to them the full credit for their achievements and to busy myself with the underlying theoretical questions.

But there remains an important lesson for the future: it is a question of choosing the right people to fill meteorologist posts. This should not be forgotten in the future training of meteorologists. When one is in the predicament of suddenly having to undertake large-scale training one should not demand exceptional results from the very start.

After this digression—on what is, in my view, an important issue—I return to the Leipzig-Bergen question. *If the preparatory work in Leipzig was a necessary condition for the final result in Bergen then, conversely, the whole work of the Leipzig Institute in my time would have been overturned, a useless 'stab in the air', had not the continuation in Bergen taken place.*

But the fact that the intimate Leipzig-Bergen connection makes so little impression on outsiders is also related to the tragic death of my first successor, Professor Wenger. He died in 1922 of the Spanish'flu, just when he was hoping to resume the interrupted Leipzig work in full consideration of the new Bergen achievements. No further successor from the old Leipzig school was to be had. The second successor, the present Director Weickmann, would also have found it difficult to continue spinning the twice broken thread. He had also more obvious initial tasks. After all, as I have said already, the earth does not consist only of the atmosphere. The *hydrosphere* and *solid earth* also come into it, and geophysics is divided into *three corresponding branches*. I had deployed all the strength of the Institute for the solution of one particular problem, in order to achieve a breakthrough in this particular area. But a geophysical institute cannot continue in such a fashion in the long term. Just as in war, so it is in the peaceful campaigns of science: two ways forward promise great successes —*breakthrough* or *encirclement*. Breakthrough had been to a certain degree achieved already. The third Director found, and rightly so, that the time for encirclement had now come. He saw it as his first duty to make good my old omissions, and *to build up the Institute in an all-round manner.*

He set up the *Kolmberg Model Observatory*. That is where the seismograph which I turned down in my time is now working. There one can register seismologically the pressure of the air on the earth's surface just as well as the effect of the surge on the Norwegian coast, there one can take part in the international aerological ascents, and there one can engage in different forms of micrometeorology and biometeorology. In the Leipzig Institute a daily weather service for research and training purposes is provided.

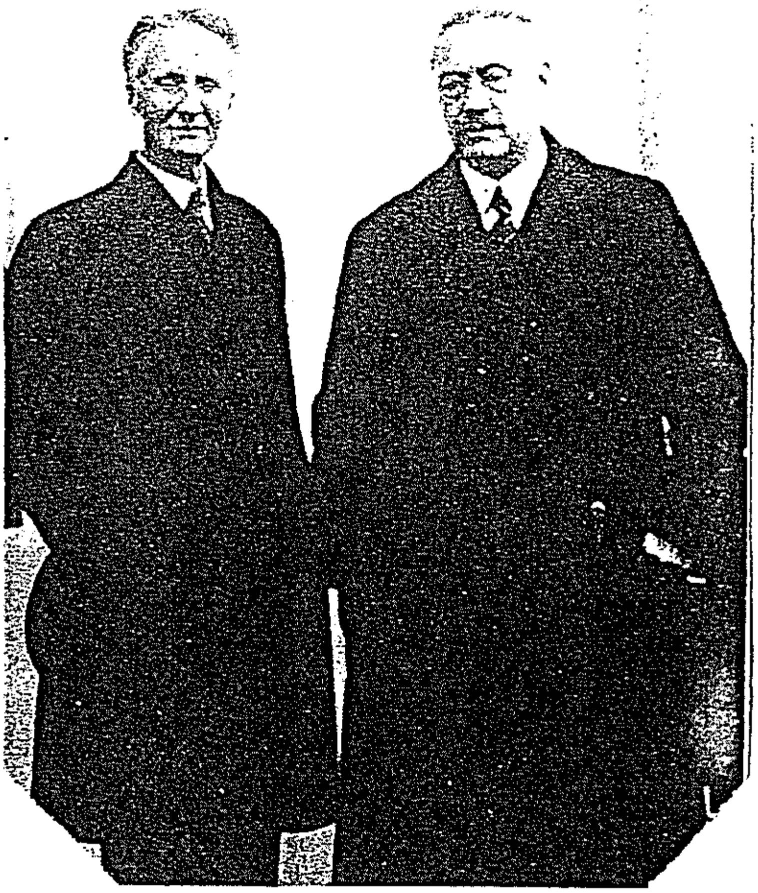
International confidence in the Director is emphasized by his having been chosen as President of the International Aerological Commission. His reputation *nationally* is a testimony to his prestigious appointment two years ago as leader of the State Weather Service. That task accomplished, he returned to his Leipzig post, remaining however as scientific advisor to the State Aviation Ministry.The organization of the new State Weather Service has already been studied by English, Japanese, Italian, American and Finnish delegations. This reorganization has essentially been carried out in the spirit of the Leipzig and Bergen schools.

And finally, as proof of the rapport between the Leipzig and Bergen schools, I have this to tell you: the direct Leipzig-Bergen co-operation, which failed to materialize on account of Professor Wenger's death, is now becoming a reality. The Geophysical Institutes in Leipzig and Bergen are going to work together on processing the results of the international aerological ascents. The task, for which in my time I deployed all the strength of the Institute, will once more become a main topic in the in my time or in Wenger's time: aerological ascent technology has made extraodinary advances. Perhaps a premature resumption of this work would not have been advantageous. But we can now hope that the processing of the results will lead to quite complete diagnoses of atmospheric conditions and to a thorough knowledge of what goes on in the atmosphere. And perhaps on this new basis we can again take up the problem of the strictly arithmetical forecasting method.

For in spite of all advances this problem has not been solved, and perhaps it never will be satisfactorily solved. But at the same time let us not forget this: *in science even distant, perhaps insolvable problems often have a meaning far beyond the directly tangible problems near at hand.* Steering to a distant goal gives a steady course. That is what we learn from seamen, who steer by the stars, not in order to reach them, but in order to ensure a steady course for themselves.

I congratulate the Geophysical Institute on the completion of its first twenty-five years. I express my appreciation to the Director for what has been achieved in his time. I end with my best wishes for the future of the Institute, and with this word of advice: *Do not forget to steer by the stars!*

V. Bjerknes and L. Weickmann
Leipzig, 1938



Translated by Lisa Shields, Met Éireann (Irish Meteorological Service), January 1997