History of Meteorology and Physical Oceanography Special Interest Group

Newsletter 1, 2010

ANNUAL REPORT

We asked in the last two newsletters if you thought the History Group should hold an Annual General Meeting. There is nothing in the By-Laws or Standing Orders of the Royal Meteorological Society that requires the Group to hold one, nor does Charity Law require one.

Only one person responded, and that was in passing during a telephone conversation about something else. He was in favour of holding an AGM but only slightly so. He expressed the view that an AGM provides an opportunity to put forward ideas for the Group's committee to consider.

As there has been so little response, the Group's committee has decided that there will not be an AGM this year.

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CHAIRMAN'S REVIEW OF 2009
by Malcolm Walker

I begin as I did last year. Without an enthusiastic and conscientious committee, there would be no History Group. Many thanks to all who have served on the committee this past year (see page 2). Thanks especially to Sara Osman, who stepped down as Hon Secretary in April 2009. She left the Met Office in January 2008 and has since worked in the library of Kingston University. At the 2009 AGM, we thanked her very much indeed for all she had done for the Group during her time as Secretary, and that was, believe me, a great deal. Thanks also to our Treasurer, Mick Wood, who has once again kept our accounts healthy (see page 2). Martin Kidds succeeded Sara as Hon Secretary, and we thank him for nobly taking on this job. He too has recently (February 2010) moved to pastures new, from the National Meteorological Library to the National Gallery, to become their Systems Librarian. We welcomed to the committee in 2009 Graham Bartlett, who worked in the National Meteorological Library for many years and retired from there in December 2008.

As I said at the AGM last year and the year before, I am very keen to see a growth in membership of the Group, and we have, indeed, welcomed several new members during the past year. Sadly, however, two people who have supported the Group for many years died during 2009. David Limbert passed away on 3 May, and Jean Ludlam died in October (see page 2).

With 81 members at the end of 2009, we were easily the largest of the Royal Meteorological Society's Special Interest Groups. But, as I said last year and the year before, I should like to see a massive growth in membership. When I talk to meteorologists and oceanographers, I find no lack of interest in the history of their subjects, but why do not many more of them join our Group? Perhaps they do not know exactly what our Group is for and what it does. I ask you, please, to publicize the Group's work whenever and wherever you can.

Repeating further what I said last year, I ask you all, please, to spread the word that our Group is very active and well worth supporting and arranges meetings which are full of interest. We need especially to convince students that the origins and growth of the atmospheric and oceanic sciences are not only fascinating but also important. All too many research students are now discouraged from reading anything more than ten years old and, moreover, do not appear to want to read anything that is not on the Web.

In 2009, four excellent meetings were held, one in January, one in March, one in June, the other in November.

- The meeting on 15 January was a joint one with the Retired Members' Group of the London and South East Branch of the Institute of Physics and was held in London at the Institute of Physics. It focused on the development of upper-air meteorological observations. The attendance was 95, including about twenty members of our Group.
- The meeting on 28 March was held in London at the Royal Astronomical Society and was another joint one, this time with the Society for the History of Astronomy. The meeting was concerned with links between meteorologists and astronomers. The attendance was 42.
- The meeting on 6 June was at the Rothamsted Research Station, Hertfordshire, the subject 'Agricultural Meteorology'. For various reasons, the attendance was only 19, but those who missed the meeting missed a very good day.
- The meeting on 18 November was the second of the 'Classic Papers' meetings and was held at the University of Reading. The subject was 'Dynamicists versus numerical modellers: a growing divide?', and a contentious approach was taken. The attendance was 161, making this the best-attended History Group meeting of all time, though I should perhaps say that it was actually a Royal Meteorological Society National Wednesday Meeting organized by the Group.

We thank all who have organized, and those who have spoken at, the Group's meetings.

Three more occasional papers were published in 2009, these being:

- No.7. *Weather services at war*, by K.D.Anderson, based upon Mr Anderson's personal diaries during a period on active service in May 1940.
- No.8. *The Met Office grows up in war and peace*, by Maurice Crewe.
- No.9. *'An experimental measure' – the first meteorological office at South Farnborough and the Meteorological Office Radio Station, Aldershot, January 1911 to December 1918*, by Brian Booth.

These papers are all available via the Royal Meteorological Society's website (http://www.rmets.org/about/history/index.php), and paper copies are held in the National Meteorological Library at Exeter.

Three issues of the newsletter appeared in 2009 and we hope you found them interesting. The newsletter has included substantial articles and increased in size to 24 pages. Do, please, send us snippets or longer pieces for the newsletter, including book reviews. We want it to be **your** newsletter. The assistance and support of the Royal Meteorological Society in printing the newsletter is greatly appreciated.

We congratulate Dr Dennis Wheeler of the University of Sunderland, who was awarded the Group's Jehuda Neumann Memorial Prize for 2009. He received at the prize at the Royal Meteorological Society's Awards Dinner at Caversham on 1 July.

As chairman of a Royal Meteorological Society Special Interest Group, I attend meetings of the Society's Meetings Committee. I attended three such meetings in 2009 and took every opportunity to point out the importance of historical context.

Committee members 2009

Malcolm Walker, Chairman
Martin Kidds, Secretary (since March 2009)
Sara Osman, Secretary (until March 2009)
Mick Wood, Treasurer and Vice-Chairman
Graham Bartlett
Margaret Deacon
Alan Heasman
Joan Kenworthy
Julian Mayes
Howard Oliver
David Pedgley
Vernon Radcliffe
Dennis Wheeler

Finances

Thanks to Mick, the Group's finances remain in good shape. Income for the year was £735.98 and expenditure £644.31. At the end of the year, the Group's balance totalled £2,040.92.

MRS JEAN LUDLAM

We were extremely sorry to hear that Jean Ludlam, widow of Professor Frank Ludlam, had passed away in late October 2009. She had been a member of the History Group from the beginning and typed for the Group in the 1980s and '90s many a transcript of the tape-recorded interviews of distinguished meteorologists. For many years she was almost an honorary member of staff of the National Meteorological Library (when it was at Bracknell), and she often supplied very nice biscuits when she visited.

THE 2010 SUMMER MEETING

18-20 July 2010

The two-day Summer Meeting used to be a highlight of the Society's calendar. This year's will be the first for almost twenty years. The meeting will be based in the historic city of Exeter in the glorious county of Devon. It will be an informal meeting which we hope will prove attractive to many. The programme is full of topical interest.

The meeting begins with dinner and a talk on Sunday 18 July and finishes at about 6.00 pm on Tuesday the 20th. The talk will be given by Professor Julia Slingo OBE, who is President of the Society and Chief Scientist of the Met Office. She will speak on today's challenges in atmospheric science.

There will be overnight accommodation at Exeter University, which has a beautiful campus with landscaped botanical gardens containing a great many plants and trees and extensive views across Exeter.

In the first half of each morning, there will talks, given at the university. Afterwards, there will be visits, for which coach transport will be provided.

On the Monday morning, the talks will focus on recent developments in weather forecasting, including modern presentations of forecasts and the changing requirements of users. On the Tuesday morning, to mark the 20th anniversary of the Met Office's Hadley Centre, talks will focus upon recent advances in climate science.

The Monday visits will be to the Met Office (Exeter), the National Meteorological Archive (Exeter) and Barometer World (at Merton, near Great Torrington). At the Archive, some of the treasures of the Met Office and Royal Meteorological Society will be on show. Barometer World is a specialist firm which sells and restores barometers and also has on display various instruments of olden days, including Hooke's otheometer, a sympiesometer, a thunder bottle and a leech barometer. It also has an exhibition of interesting and amusing meteorological curiosities. The day will be rounded off with dinner in Merton's splendid village hall.

On the Tuesday, there will be lunch in a thatched pub followed by a visit to the Lockyer Observatory near Sidmouth, which is basically an astronomical observatory but also has strong connections with climate change and climate variability through Sir Norman Lockyer's work on sunspots.

The meeting will end in time for participants to catch trains that leave Exeter between 5.30 and 6.00 pm. However, those who wish to stay another night can do so in the university accommodation.

If you wish to attend the Summer Meeting, please contact the
Royal Meteorological Society, 104 Oxford Road, Reading, RG1 7LL.

You can book online, via http://www.rmets.org/events/detail.php?ID=4385

NATIONAL MEETING OF THE ROYAL METEOROLOGICAL SOCIETY ORGANIZED BY THE HISTORY GROUP
University of Reading
Wednesday 18 November 2009

Maybe it was the provocative title for the meeting, "Dynamicists versus numerical modellers: a growing divide?". Maybe it was the provocative preamble. Whatever. The meeting certainly attracted a large audience, 161 people, including at least six members of the Group. The preamble read as follows:

This meeting will consider first the classic contributions of Rossby and Charney that were crucial to the successful development of numerical weather prediction (NWP) techniques in the 1940s and 1950s and then focus on the scepticism of some in the UK who argued at that time that improved weather forecasting depended not on numerical methods but on greater understanding of atmospheric processes. Later in the meeting, we shall explore the reasons why modellers and dynamicists now appear to be talking to each other less and less, not only in NWP but also in general circulation modelling. Some think understanding of the atmosphere has been sidelined in favour of simulation and wonder to what extent this is detrimental to the progress of modelling or, indeed, meteorology as a whole. How many of today's modellers are really computer engineers who tweak models but do not fully understand the underlying dynamics? If this is the case, how have we reached this state of affairs and how should we remedy it, if, indeed, we need to remedy it? Have we strayed from the pioneering principles of Rossby and Charney? If so, does it much matter now? The last session of the afternoon will be a Panel Discussion, involving all of the day's speakers.

The first to speak was **Malcolm Walker,** Chairman of the History Group, who introduced Rossby, Charney, Sutcliffe and others who published the papers in the 1940s and 1950s that made this a 'Classic Papers' meeting. Here, in full, is what he said.

In the minds of meteorologists, Rossby is associated with waves. What or who was Rossby? Well, quite simply, Carl-Gustaf Rossby was a giant of meteorology.

He was born in 1898 in Stockholm, Sweden, studied mathematical physics at the University of Stockholm and then spent two years in the Bergen Meteorological Institute, where he became well acquainted with the polar-front theory of mid-latitude depressions. He was not only a competent mathematician and physicist; he also had a thorough grounding in synoptic meteorology, an important combination.

He joined the headquarters staff of the US Weather Bureau in 1926 and remained in the US for almost a quarter of a century. He helped set up a weather service for a trial airway service in California in 1928 and later made his mark on meteorology as an organizer, director, promoter and researcher. He was largely responsible for the establishment of meteorology programmes at MIT and, later, the University of Chicago. During World War II, he was the leading figure in the meteorological training programme of the US Army Air Forces.

During the 1930s, Rossby rejected the view of most weather forecasters that dynamic meteorology had no relevance to them. To him, "one of the greatest obstacles to progress in meteorology was to be found in the wide gulf between mathematical theory on the one hand and applied science on the other". He set about trying to get numerical answers out of theory.

His fundamental ideas on long-waves in the westerlies were published in two papers which appeared in 1939 and 1940. In the second of them, he advanced the concept of constant-vorticity trajectories of winds, and he began to make hand-calculated numerical predictions for a one-layer atmosphere.

Let us now wind the clock forward to 9 January 1946, when an important meeting took place in Washington DC. Convened by Francis Reichelderfer, the US Weather Bureau's Chief, it was supposed to be secret, but a detailed account of it appeared in the *New York Times* two days later!

There were a dozen meteorologists at this meeting, some of them military meteorologists; and there were two eminent guests: John von Neumann, a brilliant mathematician from the Institute for Advanced Study in Princeton, and Vladimir Zworykin of RCA, who had invented the scanning television camera.

These guests had come to explain their startling proposal, that Neumann's planned electronic digital computer be used to forecast and ultimately ***control*** the weather.

Neumann had been born in Budapest in 1903 and moved to Princeton in 1930. From 1943 to 1945 he had worked on the Manhattan (atomic bomb) Project at Los Alamos where, mainly in connection with explosion shock waves, he had needed to solve complex problems of hydrodynamics. He had thereby become interested in numerical methods of solution and in the use of calculating aids.

In 1944, he became aware of the first general-purpose electronic computer, the Electronic Numerical Integrator and Computer, ENIAC, and he became involved in the planning of a second, more powerful computer, the Electronic Discrete Variable Arithmetic Computer, EDVAC.

Neumann's objective was to build a powerful computer to advance the mathematical sciences, but he was not a meteorologist. It appears that only a very few weeks before that meeting in Washington on 9 January 1946 did he come to consider meteorology a principal area for application of the proposed computer. By May 1946, however, he had decided that the weather forecasting problem certainly was a principal area for application.

For the most part, the meteorological community ignored the idea of controlling the weather, keen though Zworykin was, but many meteorologists were excited by the idea of forecasting the weather by numerical means.

Neumann had met Rossby in 1942, and Rossby, being Rossby, would surely have discussed the forecasting problem with him. And the Chief of the US Weather Bureau, Francis Reichelderfer, also appears to have discussed with Neumann, in early 1945, the possibility of using EDVAC in meteorological research and weather prediction.

However, it is likely that the most important influence on Neumann was Zworykin, who believed that computing technology would make possible both prediction and control of the weather. He had, in fact, in 1945, proposed a computer specifically for meteorology.

Early in 1946, Neumann began studying the meteorological literature and soon developed an excellent insight into the physical and mathematical problems of a numerical approach to weather forecasting.

Enter Rossby again. In the spring of 1946, this entrepreneur of meteorological ventures used his connections and influence to encourage academic institutions and agencies to establish a group including Neumann and various meteorologists to progress the idea of numerical weather prediction. Thus, the Meteorology Project was born, supported by funding from the US Navy's Office of Research and Inventions.

Soon, with a young meteorologist called Hans Panofsky, aged about 30 at the time, Neumann developed algorithmic methods of weather-map analysis, in particular fitting streamlines, isobars, and so on to observed wind and pressure maps. But the machine they used, a ten-equation, ten-variable electrical linear-equation solver developed by RCA, was an analogue computer, and its accuracy, three significant figures, was not sufficient.

An attempt was made to produce forecasts using real data by numerical integration, but this attempt was also abandoned. For a couple of years, the Meteorology Project seemed to be making little progress towards the goal of NWP.

Enter now Jule Charney, another brilliant mathematician, born in 1917, who had been greatly influenced by Rossby at the University of Chicago and continued to be influenced by him whilst working under Neumann on the Meteorology Project.

Charney became the leading meteorologist of the Meteorology Project in July 1948 and introduced his quasi-geostrophic model. He also requested that the Norwegian Arnt Eliassen join him for a year, as Arnt possessed knowledge not only of dynamic meteorology but also of synoptic meteorology, considered an essential combination. Another Norwegian meteorologist, Ragnar Fjørtoft, joined a year later, in 1949.

The first results of NWP were made by hand computation using Charney's model and were disappointing, but he persevered, and by early 1949 believed his method sufficiently accurate to justify its use in day-to-day forecasting. The first forecast made by means of ENIAC was produced in April 1950. Rossby thought Charney's method "extraordinarily promising", and Jerome Namias, another giant of meteorology, was keen to introduce Charney's method in the US Weather Bureau.

However, the method did not at that stage become a part of standard forecasting procedure, but it did bring home to many meteorologists and others that successful numerical forecasting would probably not be long coming. At last, the work of L.F.Richardson was no longer merely of academic interest for want of computing power, and Charney himself wrote in 1950 that Richardson's 1922 book on weather prediction by numerical process had become of the greatest importance. For years, practising forecasters had considered this book nothing more than an interesting curiosity. It is now considered a classic work of the meteorological literature.

In the United Kingdom, developments in the US had not passed unnoticed. The possibilities of using electronic computing machines in meteorology had been discussed formally on 25 May 1948 at a meeting held jointly by the Met Office and Imperial College, the meeting attended by George McVittie, Reggie Sutcliffe, Charles Durst and Eric Eady. And a Met Office Forecast Research Division had been set up in 1949 at Dunstable, headed by Sutcliffe under the overall direction of Archibald Goldie, who was then the Office's Director of Research. Others in the Division then or soon after included Ernest Knighting, Arthur Forsdyke, Hubert Lamb and a gifted young mathematician, a graduate of Imperial College, Fred Bushby.

In October 1951, there was an important development for British NWP: Fred attended a course at Cambridge University on the use of their EDSAC computer, EDSAC standing for Electronic Delay Storage Automatic Calculator.

But back to Charney. Independent tests of his method were carried out at Stockholm in 1950 under Rossby's supervision, and by two members of the British Met Office, John Sawyer, another gifted mathematician, and Fred Bushby. They were not as enthusiastic as Rossby and Namias, reporting that "significant success had been achieved but the success was definitely less than that achieved by conventional forecasting methods on the same charts".

Charney's model was initially too demanding for British computers, and the Met Office used a model based on Sutcliffe's development equation. An issue now arose: Charney and Rossby favoured a barotropic model; the Met Office preferred a baroclinic approach.

Sutcliffe, by the way, was the founder of Reading University's Meteorology Department. He had read mathematics at Leeds University and gained a PhD in statistics at the University College of Wales, Bangor. He became initially a climatologist and then an authority on aviation meteorology, publishing a classic work, *Meteorology for Aviators*, in 1938. At the same time, he became convinced that weather forecasting techniques could be improved substantially by applying a more scientific approach; and he formulated, as early as 1939, dynamical equations based on the fundamental roles of divergence and acceleration. His classic "contribution to the problem of development" was published in the *Quarterly Journal of the Royal Meteorological Society* in 1947.

For a few years of the early 1950s, beginning in 1950, an international argument raged over the merits of the barotropic approach over a baroclinic approach. In 1950, Dick Scorer, a mathematician then in the Imperial College Meteorology Department, attacked the approach of Charney in a provocative letter to the *Journal of Meteorology*. He certainly needled Charney and Rossby, as Charney's indignant and scornful reply showed. The argument continued at a meeting on dynamical methods in synoptic meteorology held at the Royal Meteorological Society on 17 January 1951, when Scorer, always a combative character, called Charney's approach "utterly useless"; and Charney, in a letter to Rossby, called Scorer "a fool"!

Scorer was not the only British meteorologist critical of the work in the US. Graham Sutton was another, and so was David Brunt, one of the foremost dynamical meteorologists in the world. Indeed, in 1954, Sutton believed the likelihood of Richardson's dream coming true was remote. Two years earlier, in 1952, the greatest weather forecaster of all time in Great Britain, C.K.M.Douglas, had said the same in a review paper published in the *Quarterly Journal of the Royal Meteorological Society*.

Well, time does not allow <u>me</u> to explore the barotropic-baroclinic controversy, but I believe you will hear more about it later this afternoon. I simply refer you to the list of essential references which I have prepared and placed on the desk outside [**see page 9 of this newsletter**]. Do please take a list and follow up the fascinating and exciting story of the early days of NWP. See particularly the paper by Thompson (1983), in which the original proposal for the Meteorology Project was reproduced.

As we shall hear later this afternoon, Sutcliffe's work underpinned the Met Office's baroclinic model which was developed by Sawyer and Bushby in the early 1950s.

As I have indicated, there was great debate in the early days of NWP over the appropriate mathematical foundations, and, I should add, simplifications and approximations.

In those early days, modellers and dynamicists worked very closely together. Is this still true today? To what extent do modellers and dynamicists talk to each other now? Have we strayed from the pioneering principles of

Rossby, Charney, Sutcliffe, Sawyer, Bushby and others? If so, does it much matter now?

The preamble on the programme for this meeting was intended to be provocative. I hope it has lit the blue touch-paper. I now retire from the platform and leave the subject to others.

After Malcolm's introduction, the next to speak was **John Methven** of the University of Reading's Meteorology Department, who spoke on "Early theories for extra-tropical weather system development".

He began by pointing out that the fundamental equations of fluid dynamics were known to Vilhelm Bjerknes, Johan Wilhelm Sandström and others in the 19th century, but there are still, even now, very few solutions to them. Bjerknes had been convinced that dynamic meteorology had only one fundamental task, to predict future states of the atmosphere. The equations, John said, support a spectacular variety of phenomena, including waves and coherent vortices across an enormous range of scales.

During World War I, in the Bergen Geophysical Institute, Vilhelm Bjerknes, with his son Jack and another young pupil, Halvor Solberg, advanced a new conceptual model of extra-tropical depressions, one based on air masses and fronts. At the same time, Vilhelm continued to work on dynamical meteorology, publishing in 1921, in *Geofysiske Publikationer*, a seminal paper "On the dynamics of the circular vortex with applications to the atmosphere and atmospheric vortex and wave motions".

The birth of dynamical meteorology, John said, first involved a careful synthesis of observations, identifying and characterising the structure and scales of extra-tropical weather systems. The typical scales were used to develop approximations to the governing equations which were eventually pieced together as quasi-geostrophic theory (QG). Its crucial aspect is that fast motions are filtered out by the approximations, but the QG equations can be used to predict the evolution of slower "balanced" large-scale motions.

In the 1930s, Reggie Sutcliffe was keen to establish how circulations developed. He considered the value of the polar-front model of depressions limited, for it said nothing about dynamics. Rather, he focused his attention on divergence and the ageostrophic wind and obtained the so-called 'Omega Equation', which relates vertical velocity to existing pressure or contour patterns.

The earliest NWP models were balanced. However, as computers developed rapidly it was realised that it was no more difficult to solve the unbalanced 'primitive equations' numerically than the balanced QG set; and dropping the balance approximations also increased forecast skill. From then on there was no turning back, and to some extent numerical prediction and physical understanding embodied in theory separated at that point. Considering interpretation of numerical predictions, their physical and non-physical aspects and diagnosis of model error, this talk asked, "What has QG theory ever done for us?"

The next speaker was **Lennart Bengtsson** of the University of Reading, whose topic was "The advent of numerical weather prediction". He analyzed the different ideas and concepts behind the development of NWP in the US and Europe in its first decade, 1950-1960, and asked: "What was the interaction between theory, observations and numerical prediction and forecast experiments?" and "What was the importance of having demanding requirements from many users?".

He focused first on the classic paper of Jule Charney, Ragnar Fjørtoft and John von Neumann on "Numerical integration of the barotropic vorticity equation" which was published in *Tellus* in 1950 (Vol.2, pp.237-254), adding that Charney had sent a copy of this paper to L.F.Richardson, who had been too busy to look at it himself and had passed it on to his wife to assess the predictions presented in it. He mentioned that Rossby had returned to Sweden after World War II and in 1947 become the founding director of the Institute of Meteorology in Stockholm. He had also founded the journal *Tellus* in 1949.

A key objective of the Institute was to demonstrate the feasibility of NWP; and in the autumn of 1954 Rossby's group became the first in the world to begin operational real-time numerical weather forecasting. Forecasts for the North Atlantic region were made three times a week on the Swedish BESK computer using a barotropic model, starting in December 1954.

Lennart received his initial education and training as a weather forecaster during his military service in 1957-58 and was fortunate at that time to meet several who knew Rossby well, in particular Bo Döös, Bert Bolin, Phil Thompson and Axel Wiin-Nielsen.

When he graduated from university, in 1961, Lennart worked with Bo Döös in the Swedish

Meteorological and Hydrological Institute to set up a numerical weather prediction function which included the development of an operational forecasting system. The D21 computer he used, built by SAAB, originally had a memory of 12K 24 bit words, whereas BESK originally had only 1K 40 bit words. There was no software, no compiler and no operating system and there was consequently no education needed. All information was included in a twenty-page booklet. The typical time for an operation was 10-50 microseconds.

In the early years of NWP, dynamical theory and weather prediction were fully integrated. With time, however, NWP was more and more integrated in the operational needs of the weather services; and models and data assimilation had to adjust to computer resources and the availability of observations. In Sweden, dynamical research and numerical modelling were fully integrated. In that sense, Lennart said, he and his colleagues were purists and avoided all sort of empirical corrections to the forecasts that were not uncommon at the time. At the same time, though, they had to be pragmatic.

In the 1960s, Lennart recalled, there were lively discussions about numerical forecasts among the meteorologists involved, as some model deficiencies were obvious and could in part be corrected for. This was now, of course, impossible to do, in particular for predictions beyond a few days. So, in a sense, numerical models have completely taken over and one might even ask whether weather forecasts could be provided via the Internet from an automated prediction facility directly to users. In fact, Lennart pointed out, this is already being done. So meteorologists might be following bank clerks who are gradually *de facto* being replaced by an automated system. It was obvious, he said, that the way meteorology is being organized today (not very different from the 1960s) will change in the future when a new generation that has grown up with the Internet will play a dominant role.

The next speaker was **Brian Golding**, Head of Forecasting R&D in the Met Office, whose topic was "Numerical weather prediction since the 1960s: a triumph of numerical analysis or meteorological science?". His abstract sums up succinctly the content of his talk.

"Since the initial introduction of NWP [in the Met Office] in the 1960s, a combination of improvements in observing, increased computer power, and developments in the science of modelling and data assimilation, have combined to produce a spectacular improvement in forecasting capability, such that a four-day UK surface pressure forecast is now as accurate, on average, as a one-day forecast in 1975. NWP is based on the application of laboratory physics, represented as a set of mathematical equations and solved by numerical methods, while meteorology deals with the characteristics of the emergent weather phenomena: clouds, fronts and storms. In order to produce credible predictions, NWP must satisfy the physical laws, conserve the right quantities and solve the equations with a stable and accurate numerical scheme. However, the skill of the forecasts is judged by their ability to reproduce the emergent weather phenomena. The success of NWP has come from a steady improvement to the mathematical and numerical treatment of the physics, guided by careful comparison of model simulations and observed weather systems, especially in the context of major international field programmes. One area in which this has been particularly evident has been in the evolution of our understanding of the structure of mid-latitude depressions."

In the course of his talk, Brian pointed out that improvements in understanding of atmospheric processes had greatly assisted NWP. Examples he quoted included:

- the work of John Green, Frank Ludlam and Robin McIlveen on isentropic relative-flow analysis (published in the *Quarterly Journal of the Royal Meteorological Society* in 1966);
- several papers on airflow through mid-latitude depressions published by Toby Carlson around 1980;
- work Brian himself had published in 1984 in the *Quarterly Journal* on relative-flow analysis in an idealised non-linear baroclinic wave developing in an idealised NWP model;
- research carried out during FASTEX (The Fronts and Atlantic Storm Track Experiment) which shed light on the role of microphysics (mostly published in the period 1999-2003);
- meticulous mesoscale analyses carried out by Keith Browning over many years.

Brian pointed out that the building of a completely new numerical model was rarely undertaken as it involved so many person years of effort. Instead, existing models were modified gradually to make them increasingly more realistic. Output from models was monitored routinely; and improvements to the fundamental mathematics and to the numerical treatments of physics were incorporated in models from time to time. Grid scales were now much finer and models a great deal more sophisticated than in the 1950s and 1960s, when Fred Bushby was pioneering techniques of NWP, but, Brian wondered, would Fred think the basic model had changed much since the early days?

The last speaker was **Andy White**, also of the Met Office (Dynamics Research), who spoke on "The role of meteorological dynamics in numerical model construction and appraisal in 2009". His abstract was as follows:

"An important tension in meteorological science is between the desire of theoreticians to offer simple models of the real atmosphere and the desire of simulators to provide accurate predictions. If weather forecasting and climate modelling had turned out to be straightforward problems, the two dispositions would have converged, and a unified view would have emerged. But the complexity of the atmosphere means that neither theories nor simulation models are simple. This puts great demands on both theoreticians and simulators. Increased pressure for results tends to exacerbate the potential problems."

Andy argued that dynamical theory plays a key role in the design, testing and improvement of numerical models, also that the theoretician/ simulator relationship is very much better than might be expected. In his view, any tension that might exist between dynamicists and modellers should be considered productive and creative, not inhibitive.

All of the speakers provided historical perspectives and addressed the basic question of the meeting: was there a growing divide between dynamicists and numerical modellers? The stage was set for the panel discussion which followed, chaired by **Sir Brian Hoskins** (Imperial College and University of Reading).

Brian first asked each speaker in turn for their views on the basic question and then opened up the discussion to the audience. There was no shortage of contributions.

Most speakers thought there was a divide, but the majority view was that it had always been there and was probably greater in the early days than now. Certainly, one speaker said, there was a great divide in the 1940s and 1950s between theoreticians and forecasters.

The importance of understanding models was stressed; and here, it was suggested, a divide is growing, with some, if not many, modellers not understanding the dynamics. Indeed, models are becoming so complex that the likelihood of anyone fully understanding them is diminishing year on year. One speaker asked if dynamicists are a dying breed in universities and suggested that a new breed of dynamicist is needed.

There was agreement that better understanding of physical processes is needed; and the importance of intelligent diagnostics was stressed. One speaker was concerned that forecasting will be taken over by computer scientists.

Have we strayed from the pioneering principles of Rossby, Charney and others? Few addressed this question, but one who did so mentioned that the importance of group velocity appeared to have been forgotten.

All in all, this was a lively and stimulating meeting. It will have provided considerable food for thought for the many young scientists who attended.

MEETING AT THE UNIVERSITY OF READING ON 18 NOVEMBER 2009:

ESSENTIAL REFERENCES

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Which old papers have been cited most in meteorological and oceanographical papers in recent times? This question was posed on page 1 of Newsletter 3, 2009. Would Hadley's classic of 1735 come top of the list? And where in the list would the papers of the Bergen School of Meteorology on the polar front theory of depressions come? Here's a project for someone. Would anyone care to undertake it? If so, please get in touch with me and I shall send you a copy of the article that featured in my View from the Chair in the aforementioned newsletter.

Malcolm Walker

DON'T TRY THIS AT HOME!
by Howard Oliver

The Royal Society has recently included a range of historic original papers on its public web site, (http://trailblazing.royalsociety.org). One of these is the letter from Benjamin Franklin describing his famous experiment with a kite in a thunderstorm.

So should you be foolish enough, here is a shortened version of how to do it!

The letter is headed "Philadelphia, Oct 1 1752" and was read on Dec 21 1752:-

"......To the top of the upright stick of the cross of the kite is to be fixed a very sharp-pointed wire, rising a foot or more above the wood.

To the end of the twine, next the hand, is to be tied a silk riband; and where the twine and silk join, a key may be fasten'd.

The kite is to be raised, when a thunder gust appears to be coming on, (which is very frequent in this country) and the person, who holds the string, must stand within a door or window, or under some cover, so that the silk riband may not be wet.

As soon as any of the thunder-clouds come over the kite, the pointed wire will draw the electric fire from them; and the kite, with all the twine, will be electrified; and the loose filaments of the twine will stand out every way, and be attracted by an approaching finger.

When the rain has wet the kite and twine, so that it can conduct electric fire freely, you will find it stream out plentifully from the key on the approach of your knuckle

At this key a phial may be charged; and from electric fire thus obtained spirits may be kindled, and all other electrical experiments be performed.......".

NEWSLETTER 2, 2010

Please send us snippets, longer articles and book reviews for the next newsletter. Photographs are welcome, too.

The deadline for sending in your contributions is 31 May 2010. Please send them to Malcolm Walker, 2 Eastwick Barton, Nomansland, Tiverton, Devon, EX16 8PP.

We look forward to hearing from you.

MORE REMINISCENCES BY RICHARD GREGORY

My flying career spanned 58 years, over which period I progressed from the Tiger Moth to the Hawker Hunter in terms of powered aircraft, while in gliders and sailplanes, matters advanced even more. I began on – yes, quite definitely on and not in – a most rudimentary affair, which even the Wright brothers would have regarded as a step backward. The pilot sat on a curved piece of plywood at the front of an openwork wooden lattice frame, his feet on the rudder pedals and the control column between his knees – totally out in the open! The last sailplane I flew had retractable undercarriage, flaps and turned-up wing tips, as worn later by advanced commercial jets, all embodied in a sleek, fibreglass body in which the pilot lay almost supine to reduce drag.

The powerless aircraft, like the powerless water-borne vessel, can only go as far as the pilot's skill can take it and, like the cloth-engined vessel, is totally subject to air movement – vertical for the aviator, but horizontal for the sailor. While, in general, the speed of the sailplane is less than that of the powered aircraft, this means that, when flying through turbulent air, these slower sailplanes will be affected for a longer period in either up or down current.

When flying Tiger Moths from Spitalgate, near Grantham in 1947, without radio, if trainee pilots were airborne solo and the weather worsened seriously, the recall relied upon a maroon fired from air traffic control. This became necessary on a day when about a dozen of us were flying solo and the cloud base lowered. Those of us to the east of base crept back, but this left a chum way out to the west, who was practising aerobatics over a four-way rail junction until the cloud base lowered, then he followed the wrong line to get back to Grantham. By the time he had turned round and found the right way back, there was little room below cloud over the airfield, so he followed the road up the hill and, when he saw the hedge marking the airfield boundary, he just lifted his Tiger over and closed the throttle, to land on the grass. One evening a little later, when the advanced course flying the Harvard had reached night flying stage, produced treacherous conditions – very high humidity and a clear sky, bringing the temperature down with fog possible and forecast. However, a gung-ho flight commander decided to give it a go, very foolishly as it happened since, when he took off, the fog initially formed in his aircraft wake and rapidly spread out to cover the airfield. Fortunately, nearby RAF Scampton had radar talk-down Ground Control Approach, so our hero got back to base – by car and with a red face.

Unfortunately, our happy band of brothers at Spitalgate was soon split up with half, including myself, being dispatched to Feltwell in the fens where, after a few grumbles, we settled in and got on with our flying. To illustrate the slow pace of life in general, I recall two rather unusual weather ‘happenings’, both during long spells of settled high pressure. In the first incident, we had been subjected to a gentle northerly drift from the north Norfolk coast. One morning on our way to Met briefing, there was a distinct smell of wood smoke in the air, the smoke rendering visibility to less than 1,000 yards, which rather put the kybosh on flying for a couple of days. It was not until three or four days later that we read in the national press of forest fires in Finland. On the second occasion, with fairly strong winds from the south west, we found the air full of soil, which had been thoroughly dried out – to be lifted and carried away, perhaps to Finland. This too reduced visibility to less than 1,000 yards for a couple of days, though it is probable that the duty forecaster had never seen the like before and, with no other nearby reporting station upwind, he can have had no indication what was on its way, poor fellow.

Only 14 months later, having graduated and been awarded my wings, I found myself at a Spitfire training unit at Chivenor in north Devon, flying over totally different country from the fens, with each day bringing new challenges. One day found me being briefed by an instructor with two rows of campaign medals below his wings, who was to lead me in a tail chase exercise. This would have been exciting enough, but the sky was gin clear, bright blue and sunny. This meant that, when I had been signalled into the line astern position to follow my leader – at all costs – I just had time to lower my seat and raise my feet to the top rudder pedals some 6 inches above the usual pair (both adjustments raising my G threshold) when we were off! Banking hard in very tight turns, rolling and diving, to see the green of Devon and the blue of the sea, and then pulling up, with the horizon dropping rapidly below, the sun coming over my shoulder and disappearing again, wheeling and soaring, using full power – that lovely Merlin with its 1,650 hp on tap – to catch up in the tightest turns. I sweated, clenching my teeth, while still the sun, sea, sand and sky whirled around and I clung on in grim determination not to be shaken off, until at last the wings in front levelled off and I was called back into echelon starboard, breathing heavily but fairly pleased with my performance. Walking away from our aircraft later, my instructor turned and said "That wasn't bad" which, at the time, was better than winning the football pools, and this remains one of my happiest flying memories.

After three idyllic months spent flying this wonderful machine on all sorts of exercises, their Airships put me on a different set of tracks, to Bentwaters, for my introduction to jet-propelled aircraft. The Vampire Mark 1 was not a practicable weapon of war, but it sufficed. The pilot's seat was raised and lowered by means of a handle on the right-hand side, with a button on top to disengage a plunger from whatever hole it might have been occupying for the moment, to be engaged in another hole. However, returning to Bentwaters one very bumpy day, and with the fuel running quite low, I reached down to press the button and raise my seat just as my aircraft flew into an updraft, causing the seat to bottom hard momentarily before the next bump downward came along, and so I continued in a series of spine-jarring ups and downs, while frantically trying to avoid transmitting these movements to the control column – and the aircraft itself! Mercifully, before we ran out of fuel over London, the button clicked into a hole – though not the desired aperture – and I made the best of it to thump tread on the tarmac a little later.

Posted to No.16 Squadron in north Germany, and very much the new boy, I was sent off first to fly a high level cross-country navigation exercise. Only mildly put out to find that my first turning point, Cologne, was marked as Koln (with an umlaut), I turned north and headed for the island of Sylt. I soon found myself seriously affronted, when a glance over the port side of the cockpit showed that we were being pushed bodily sideways! All very well in a Tiger Moth, trundling along at 75 knots with a 25 knot beam wind, but at 32,000 feet and .78 Mach number? However, there it was, and it seemed that those who were to follow me later should be told – but by the new boy? With no trusty Dalton computer on which to plot my triangle of velocities, I had recourse only to what I could remember of the cosine table, the basic 1-in-60 rule, and a few grey cells. Finally, after checking and re-checking my workings, I took a deep breath, pressed the button to call base, and asked air traffic control to pass my message to the Squadron. In the crew room sometime later I was handed the telephone with the cryptic words "It's Met". To my considerable surprise, and with a sense of gratification, I received, first, an apology from the Met office for not briefing us on the existence of this, my first jet stream, and the news that my estimated wind direction was only 10° out, but the speed was spot on.

On another high level cross-country exercise which took me along the north coast of Germany during a long spell of high pressure with a gentle southerly drift, I encountered dense smoke haze at 35,000 feet as I headed westward. Initially this was coloured grey, but then became a bilious orangey-yellow, turning a dirty blue, and finally back to grey, as I passed through the smoke plumes from the Ruhr, some hundreds of miles to the south. Evidently "Bomber" had not quite flattened it all. On another occasion, we were returning to base in formation below cloud, with heavy showers about, when our gallant leader (sic) took us into the blackest of these at about 320 knots. We managed a fairly rapid turnabout, headed back to base and landed. Leaving the cockpit and walking past the leading edge of the wing, I saw bare metal - totally stripped of paint and filler, (which had been generously applied) going back 18 inches, and the metal bullet-shaped fairing between tail boom and fin looked just like a ball of putty into which some playful child had put his pudgy thumb – frequently! Later still, to celebrate Bastille Day in 1952, it had been decided that six NATO countries should each contribute 36 aircraft to a massive demonstration fly-past over Paris. The practices for this event gave us all some of the most unpleasant, sweaty, and unnecessarily hazardous flying ever, but on a day off, No.16 Squadron took off and headed south east, climbing steadily, in a fairly wide open formation, which gave us the chance to look around. This time the resident high-pressure system gave us the most glorious view of the snow-covered, sunlit Alps some way ahead and a little lower, for the first – but happily not the last – time for me, as I was to enjoy two weeks of skiing there, in the company of a bunch of other single seat pilots, under the pretext of carrying out a Winter Survival exercise.

Ground Controlled Approach radar talk-down to the landing was normally practised in clear weather, concentrating hard on the instruments with one's seat at the bottom of its travel.

Cheating by looking out was strictly not on, being the equivalent of shooting oneself in both feet. One was guided by a continuous stream of gentle instruction and correction, with the talk-down controller saying, for instance, "You're on the glide path – come two degrees left, left – steady – you are a little below the glide path – check your descent slightly – steady" and so on until "You are approaching 400 feet (the break-off height with our equipment). "Look ahead and land visually", which usually meant that all that was needed was to close the throttle and hold off for a smooth landing on the centre line. However, one day the weather was worse than forecast – as I discovered when, at 400 feet I was still in cloud. Luckily, the talk-down controller knew this quite well from looking out of his window before going on watch, so he kept the calm, reassuring, patter going as we descended still further – 350 feet, 300 feet and still in cloud – 250 feet with "You are off the centre line come 2° left, steady" – 200 feet and "Come a further 2° left" and finally, just below 150 feet and less than half a mile from the end of the runway while still travelling at 125 knots, I picked up the runway lights apparently miles to the left, but a quick jink, with the throttle closing and the wings levelling once more, we touched down left wheel with first. Not my neatest landing ever, but there was plenty of runway ahead as we slowed down and finally turned off. We could not see the air traffic control tower, nor the line of parked aircraft only 800 yards away, but the lights on the taxiway finally brought us on to the hard standing and the nose wheel chock!

While having always been inclined to do those things which had not already been expressly forbidden, I once took off at night from Worksop in the Dukeries, and climbed steadily northward. At about 40,000 feet, and probably over Northumberland, I beheld, for the first and only time, the shimmering glory of the Northern Lights – a diaphanous, curtain-like, pale rainbow coloured light, quite inexpressibly and breathlessly awe-inspiring. This was an echo of Jacques Cousteau's enchantment of the depths, without a doubt. In the reverse sense, descending late one autumn afternoon toward Swinderby, between Newark and Lincoln, and with the Vale of Trent covered in mist, 15 miles away the towers of Lincoln Cathedral were clearly silhouetted against the mist filled vale beyond. There remain many more memories, but these must wait.

STORM WARNINGS FOR SEAFARERS PART 2

by Malcolm Walker

The story of how a violent storm in October 1859 changed the course of meteorological history was told in Newsletter 3, 2009. The outcome in question was the introduction of a storm-warning service for seafarers by the Meteorological Department of the Board of Trade (now Met Office). Warnings were first issued by the Department on 6 February 1861. However, the service was suspended on 7 December 1866. The story of why, and what happened then, is told in this article.

Admiral FitzRoy believed he had permission to issue storm warnings – which he called ‘cautionary signals’ – but he certainly exceeded his authority when, on 1 August 1861, he began to publish weather forecasts for the general public on a routine daily basis.

Board of Trade officials were not entirely happy over FitzRoy issuing storm warnings and considered them experimental. They were also not happy that he had begun to publish forecasts for the general public. This did not in itself bring official reprimand, but criticism of his forecasting techniques soon came. His storm warnings and weather forecasts, though helpful on the whole, were not always accurate. This came as no surprise to him, for he was aware that his methods were imperfect, as shown by his insistence on the word ‘forecast’, rather than ‘prophecy’ or ‘prediction’. Unfortunately, he assumed that the views of those who considered forecasting unscientific were directed against him personally. In the early 1860s, meteorology was an emerging science that was not yet firmly based on absolute laws of physics and was, furthermore, believed by many to be incapable of mathematical expression. This was a time of debate over the nature and methodology of science.

Some championed ‘practical science’, from which there were tangible benefits for society and in which, in the case of meteorology, weather wisdom and amateur observers played important rôles. Others believed worthwhile progress could be made only through advances in ‘abstract science’ or, as some called it, ‘philosophic science’. FitzRoy was essentially a man of ‘practical science’ and never claimed to be “a truly scientific man”, as he put it in his *Weather Book*. He was “only”, he wrote in *The Athenæum* (24 November 1860, Part 2, p.710), “a superficial follower, however devoted an admirer, of *real* philosophers". Perhaps he was being modest. Perhaps, on the other hand, he was a little unsure of himself.

Whatever the truth of the matter, FitzRoy had no need to react as he did to the views of those who considered forecasting unscientific. He was a pioneer of meteorology with intuitive insight into the ways of the atmosphere, a man who introduced well-founded empirical methods that were significantly more scientific than those based entirely on weather lore. In retrospect, it is easy to say that he worried unnecessarily, but FitzRoy was a sensitive man and, moreover, did not take too kindly to criticism. The eventual outcome of his reaction to the doubts over the scientific respectability of his work was that he all but isolated himself from the scientific community; and his health was to suffer as a consequence of this and other problems which began to mount for him in the early 1860s.

The *Royal Charter* disaster was not the only event in 1859 that affected FitzRoy profoundly. So, too, did the publication of Darwin's book *On the origin of species*, which appeared in November of that year. To FitzRoy, a devout Christian with conservative views, Darwin's atheistic theory of organic evolution by natural selection was unacceptable. It was completely at odds with his fundamentalist beliefs. Darwin's theory contradicted Biblical 'truth'.

Though Darwin's conclusions did not come as a total surprise to FitzRoy, for the two men had discussed Darwin's observations and their implications many times during the *Beagle* voyage, publication of the book was, nevertheless, a great disappointment and frustration to FitzRoy. Whenever an opportunity arose, he attempted to refute Darwin's theory, sometimes in writing (usually under a pseudonym), sometimes in public debates. He became obsessed with the matter and it preyed on his mind. His health would surely suffer.

There was criticism of FitzRoy's forecasting techniques from astro-meteorologists, too, the early 1860s being an especially active time for Victorian astro-meteorology. As yet, the boundaries between astronomy, astrology and meteorology had not been clearly drawn. Criticism came too from ship-owners, who were concerned less with the safety of their crews than with the loss of revenue caused by captains keeping their vessels in port when storm warnings were in force.

FitzRoy's health did indeed suffer, and on 30 April 1865 he took his own life.

After his death, there was an inquiry into the work of the Meteorological Department of the Board of Trade. The outcome was the Galton Report, so named because Francis Galton chaired and dominated the committee of inquiry. The findings of the committee were laid before both Houses of Parliament on 13 April 1866.

FitzRoy had said in the 1863 *Report of the Meteorological Department of the Board of Trade* that storm warnings and daily weather forecasts both rested on the same footing and therefore stood or fell together as part of one system. Galton's committee disputed this view, believing that it probably did an injustice to storm warnings, which they considered to have been "to a certain degree successful" and "highly prized". Weather forecasting was a different matter. It was not based on "precise rules" or on "a sufficient induction from facts" and was "not in a satisfactory state".

The committee reported that weather forecasts had proved "popular and interesting" and had caused no additional expense. However, there was, as yet, they said, no scientific basis for them. Furthermore, the forecasts were not "generally correct in point of fact" and there was "no evidence of their utility". There appeared to be no good reason why a government department should continue to undertake the responsibility of issuing them.

The committee's disapproval of the Meteorological Department's involvement in weather forecasting was obvious from the very beginning of their report. Galton's dislike of FitzRoy's approach to forecasting had been clear for a number of years. The recommendation of the committee that the publication of daily forecasts for the general public should cease immediately therefore came as no surprise and was accepted without much delay. The last forecast to be published appeared in *The Times* on 28 May 1866.

Months passed, but still the storm-warning service continued. In fact, Galton's committee had not recommended withdrawal of that service, but there was disagreement between Board of Trade officials, Galton's committee and a number of Fellows of the Royal Society over the best ways to produce and present storm warnings. Galton's committee had proposed changes and a consultation process followed. Then, on 29 November 1866, a circular was issued by the Board of Trade. With effect from 7 December 1866, the service would be suspended, but not necessarily permanently.

This decision was resented by many, which should not have been unexpected, for the surveys carried out by FitzRoy's Department, the Board of Trade's Wreck Department and Galton's committee had all shown that most seafarers considered storm warnings beneficial. There was widespread agreement that the system of cautionary signals had helped save the lives of many seafarers, especially fishermen. Given that so many involved in maritime activities approved of the storm-warning service, suspension of it in the middle of winter, the stormiest time of year, surely indicated a lack of judgement on the part of those responsible for the decision.

Strong and vociferous complaints came from seafarers, harbour authorities and many others. Letters were written to *The Times* and other journals. Letters were written to the Board of Trade. Questions were asked in the House of Commons. Even the astro-meteorologists who had so needled FitzRoy spoke out in his defence. And some people wondered why storm-warning services very like that developed by FitzRoy had been established in France and other countries if cautionary signals were as unreliable as critics claimed.

If any thought the storm of protest over the suspension of the storm-warning service would soon blow itself out, they were much mistaken. The stream of complaints which began to flow as soon as the suspension was announced continued unabated and a campaign to restore the service soon developed, with a formidable champion in the person of Colonel W H Sykes, FRS, MP. His offensive began in the House of Commons on 15 February 1867, when he asked the President of the Board of Trade whether the Storm Signals, as hitherto practised by the late Admiral FitzRoy, were to be continued. If so, in what manner would they be continued, and by whom? If they were to be discontinued, would it not be prudent to invite the Chamber of Commerce of the United Kingdom to express an opinion on the subject?

Complaints and enquiries about the suspension of the service came from far and wide. In early 1867, most of the foreign correspondence received by the Meteorological Office related to this matter;[1] and memorials pressing for a resumption of the warnings were received from the Leith Chamber of Commerce and from the underwriters and shipowners of Glasgow and Greenock. Many of the complaints and enquiries were ignored or brusquely brushed aside, including a letter from the Director of the Paris Observatory in February 1867, when he sought information about the suspension of the service.

However, a letter from the Board of Trade to the Director of the Meteorological Office at the end of May 1867 could not be disregarded. It read:

"Sir, I am directed by the Lords of the Committee of Privy Council for Trade to state that a large deputation has waited on the Duke of Richmond to urge that some warning should be given of apprehended danger from storms, and I am to ask whether it might not be possible for the Committee appointed by the Royal Society, upon such conditions and under such limitations as they might think necessary, to give effect to a desire which is strongly expressed by many competent and influential bodies and persons."

Notice had to be taken of this letter. The Duke of Richmond was the President of the Board of Trade and the deputation included members of both Houses of Parliament.

The Royal Society Committee which controlled the Meteorological Office discussed the letter at their meetings on 3 and 7 June 1867 and the Office's Director (Robert Scott) replied on the 8th, saying that the Committee were not willing to "prognosticate weather, or to transmit what have been called 'storm warnings'". However, he informed the Board, they were collecting information which they confidently anticipated would enable them, sooner or later, to frame rules by which such prognostications could be made, one of the main objects being "the advancement of meteorological science in this important practical direction". The observatories, he said, were not yet in practical operation and, indeed, could not be until the necessary funds had been voted by Parliament.

Scott reminded the Board that the Meteorological Office had issued a circular in March 1867 in which (as it was put in the circular) they had offered to "forward each day, by post, free of charge, to any port which may require it, a copy of the daily weather report, the same as that furnished to the second edition of the London morning papers". Moreover, he further reminded the Board, the Meteorological Office were prepared, on application, to "furnish, without unnecessary delay, any telegraphic information which it may have received", half the cost of the transmission to be borne by the local

[1] The Meteorological Department of the Board of Trade became the Meteorological Office on 25 February 1867.

authorities that wished to receive the information. The Committee were "willing to communicate information to any accessible place upon the terms laid down in their circular, and to an extent limited only by the sum placed at their disposal for the purpose".

Sykes was impatient to know the outcome of the deputation's appeal to the Duke of Richmond. In the House of Commons on 7 June, he asked the Vice-President of the Board of Trade, Mr Stephen Cave, "whether any and what action had been taken to restore 'storm warnings', consequent upon the Deputation of Members of Parliament to the President of the Board of Trade". In reply, Cave told the House the Meteorological Committee were willing to provide warnings of storms "as far as practicable". Sykes raised the matter in the House again on 24 June, 8 July and 19 July 1867, pointing out on the latter occasion that 28 petitions, bearing a total of 1,744 signatures, had been presented to the House of Commons in favour of the resumption of storm warnings, five of the petitions from incorporated bodies, others from different ports.

Moreover, he said, a report submitted to the Manchester Literary and Philosophical Society in April 1867 contained not only the accusation that the Meteorological Committee of the Royal Society were utterly regardless of public opinion and feeling on the subject of storm warnings but also an expression of hope that the Board of Trade would again take on the management of the Meteorological Department. There were, he said, 50,000 fishermen along the eastern coast, "and it was a perfect mockery to require them to pay half the expense of telegraphing". "Such an arrangement", he commented, "would be of no more use than the daily meteorological report in the papers, telling what had occurred yesterday". He wondered what the Committee would do prospectively. The answer to that question was, he suggested, that "with six new stations they would be able to make their records, and ten years hence they might do something"! It is recorded in *Hansard* that "he appealed to the common sense of the House whether they would tolerate such a mockery".

Sykes hoped the Board of Trade would insist on the restoration of storm signals and saw no reason why this demand should not be satisfied, "except the fear on the part of the Committee of the Royal Society that their scientific dignity would be compromised". "Suppose it were compromised", he said; "what then, if the public gained in the end? The objection was nothing more than a piece of scientific coxcombry and pedantry."

Still the protests continued in newspapers, and still the Board of Trade rejected efforts to restore a storm-warning service. And then Sykes stepped up his campaign. At the annual meeting of the British Association in 1867, he was scathing. In his paper on *Storm signals – their importance and practicability*, which he delivered on 9 September before a large audience, he claimed that 305 out of 405 storm warnings "given under the system lately in use" had proved correct; and he asked if such results did not "sufficiently justify the continuance of these warnings". It was evident, he complained, that those in authority did not think so. The warnings had been stopped, and he wanted to know the reason why. To laughter and applause, he suggested that the argument employed by the Meteorological Committee of the Royal Society was "a pedantic affectation of science – literally, the coxcombry of science"! The Committee considered the reliability of the warnings questionable, he said, "on the ground that Admiral FitzRoy had obtained his conclusions mainly on empirical data". To remedy this, the Committee proposed to establish eight additional observatories and "at the end of fifteen years expected to be able to predict storms on philosophical data, not on empirical data". He was sceptical, doubting they would obtain these results in the next fifteen years if they had not done so in the last fifteen. "Here were we", he went on, to further applause, "the most maritime nation in the world – having set the example to other countries in this matter of storm warnings – and yet we were now dropping them". "We were", he said, to further laughter, "too scientific for the work".

Support for Sykes came from far and wide, and the pressure mounted on the Board of Trade and the Meteorological Committee. Eventually, in November 1867, the campaign was won. The Board of Trade announced on 13 November 1867 that storm warnings for shipping would be issued again, and the first storm signals were issued on 10 January 1868. From that day to this, a storm-warning service for seafarers has continued uninterrupted, but the decision to cease publishing daily weather forecasts for the general public was not reversed at this time. Not until 1879 was that step taken.

LATE SWEDISH SURGE FOR STORM WARNINGS INITIATED MODERN WEATHER FORECASTING
by Anders Persson

While regular storm warnings were established around Europe during the second half of the 19th century there was one exception, Sweden. It would last until September 1905 that the first (and successful) warning was issued. The genesis was to affect to whole of meteorology.

The year 1903 saw Vilhelm Bjerknes (1862-1951) depressed, frustrated and with insomnia. Since 1897 he had been professor in physics at Stockholm Högskola (later to be named Stockholm University). His beloved father had just died, as had his father-in-law. His oldest son was seriously ill and the two younger, Jakob and Kristian, had problems with their nerves. As if all this were not enough, his wife Honoria had miscarried. Bjerknes himself, who came from humble conditions in Norway, felt a social outcast among his wealthier Swedish colleagues. As the Swedish-Norwegian political crisis escalated[2] he felt an increasing personal and professional antagonism towards him from his colleagues in the department.

Scientifically, Bjerknes was in a crisis. Before coming to Stockholm in 1897 he had worked with the renowned German physicist Heinrich Hertz and established himself as a leading expert on the dynamics of the 'ether'. Ether was the extremely thin substance, the fluid or medium through which electromagnetic waves were supposed to be carried or propagated. Bjerknes had in 1898 found a circulation theorem which described in mathematical terms how density variations in the ether would generate observable currents. But by 1903 doubts about the existence of ether had grown and Bjerknes' star was in decline. Adding to the ordeal, summer 1903, which the Bjerknes family spent in their summer house in the Stockholm archipelago, was cold, rainy and stormy.

As his influence among physicists decreased, it grew among geophysicists. Ether or no ether, Bjerknes' circulation theorem and his other results fitted very well into the description, understanding and perhaps prognostication of currents in the atmosphere and the world oceans. Encouraged, almost pushed, by his Swedish geophysical colleagues, Bjerknes slowly started a change of direction in his research. It would give birth to a new carrier and make him one of the world's leading meteorologist and oceanographers. The stormy summer of 1903 provided the decisive pretext.

One of Bjerknes' meteorological friends, Nils Ekholm, had for some years pursued a campaign for a special storm-warning system for the Swedish coasts. The havoc after the storms made this even more urgent. Sweden was at this time the only country in northwestern Europe without such a service. The forecast office was governed by the Royal Swedish Academy of Science, and its leading body felt that there were no established scientific rules or laws comparable to the forecasting of the tides, the moon and the planets. Ekholm claimed that upper-air measurements, obtained by kite or balloon could provide observational indications of approaching storms. Bjerknes supported Ekholm's idea but wanted to broaden the debate to show that it was not enough with more observations; methods ought to be found how to deal with these observations.

At the Stockholm Physics Association meeting on Saturday October 24 1903 Bjerknes presented a lecture on "A rational method for weather forecasting". The speech attracted the attention of the contemporary newspapers and it is from them we can reconstruct an outline of Bjerknes' speech:

A rational method for weather forecasting. Lecture by Professor Vilhelm Bjerknes at the Physics Association meeting in Stockholm, Saturday, October 24 1903.

It is natural that meteorology's highest mission, like every other science, is to predict the future or at least lift a corner of the veil. But for that we require a complete knowledge, and the only means to test if we have that knowledge consists of making forecasts and seeing if they verify. Meteorology has also the privilege that its forecasts are of the greatest practical meaning. Weather forecasts can either concern a definite day, normally tomorrow, or mean conditions for a longer time, a month, a season, or a year.

Two things are required for forecasts: knowledge of the current weather situation and the laws according to which it will change. The most difficult and most fundamental requirement is solution of the dynamics, because mechanics introduces time into the problem. A solution is theoretically possible, in that weather is determined by the wind or air motion together

[2] In 1814, after the Napoleonic wars, Sweden annexed Norway as a compensation for the loss of Finland in 1809. The union was never a happy one and in 1905 Norway declared its independence.

with the density, pressure, temperature, and humidity of the air, which corresponds mathematically to seven variables. With the help of hydrodynamics and thermodynamics, together with knowledge of the heat radiation from the sun and outgoing radiation from the earth, seven equations can be formulated for determination of the seven variables.

But a pure analytic solution would exceed our capability, and even if it could be brought about, would be so detailed as to be practically worthless. One cannot take into consideration every little eddy, for example, on a street corner in Stockholm. The formulation should be stated in this way: On the basis of today's synoptic weather chart, construct that for tomorrow. However for this we require weather charts not only for the surface of the earth, but also for different heights in the atmosphere where aeronautical observations with kites and balloons are essential.

The lecturer described the path which should be followed in general strokes. The problem is divided into two, one dynamic and one physical, in which one first considers that temperature and humidity are unchanged for a short time and determines changes in the wind, density, and pressure through graphical construction. With help of that result and the heat that is exchanged, changes in temperature and humidity are then determined and continue so with the values for these.

Topographic charts must be used so that attention can be paid to the elevation of the land, and not, as is usual, 'reduced to sea-level'. The speaker finally remarked that the method usually used for weather forecasting does not differ essentially from the method he suggested. The difference is only that one must use all knowledge in a systematic way, including the knowledge of mechanics and physics that have been gathered for us by investigators since the time of Galileo and Newton. (Translation from Swedish by Norman A.Phillips)

Bjerknes' lecture was, according to the newspapers, followed by a "lively discussion" where the problem of the earth's topography became the centerpiece of the discussion. Two meteorologists, Professor H.E.Hamberg and Dr Westman suspected that friction at the earth's surface and the effect of a deformed continent might be impossible to account for in the calculations. Bjerknes replied that this would certainly be the case with an analytic treatment of the problem. But in the graphical method, irregularities introduce no difficulty, because one can account for them with topographic maps and consider friction through pure empirical investigations. Ekholm pointed out that Bjerknes' method, if it could be practically applied, should without doubt lead to a very significant step forward. He showed with examples how the Scandinavian Peninsula topography has a strong effect on the formation and movement of cyclones. From this followed, said Ekholm, the need followed to pay more attention to unevenness in the earth's surface than at that time generally took place.

Over the Christmas and New Year holidays 1903-04, Bjerknes seem to have pondered the problems about mathematical weather forecasting, because on 7-8 and 10 January 1904 he published a long text in Norway's leading newspaper *Aftenposten* with the title "Weather forecasts and the possibilities to improve them". Shortly afterwards the leading meteorological journal *Meteorologische Zeitung* carried a scholarly article by him with the title "The problem of weather forecasting considered from the points of mechanics and physics".

This German-speaking version of Bjerknes' article would over the coming years before the outbreak of the war in 1914 inspire meteorologists around the world. For reasons still not understood, the impact seems to have been most profound among British meteorologists and mathematicians. On 27 May 1910, Bjerknes was invited by the Royal Meteorological Society to deliver a lecture at the University College in London titled "Synoptical representation of atmospheric motion". In the speech he suggested that observations from the free atmosphere were made more frequently, involving the broader meteorological community.

Just a week earlier, a broad, international undertaking had been made. On 20 May 1910, a large amount of upper-air observations had been made over Europe. This was not to support numerical weather prediction but to investigate the possible effects on the atmospheric flow when the earth on that day passed through the tail of Halley's famous comet. Nothing was found, but the material was in a few years time published by Bjerknes and his assistance in upper air maps covering much of Europe. This was later used by Lewis Fry Richardson in his famous integration of the basic equations Bjerknes had mentioned. And the rest is history as they say…

RIKITEA METEOROLOGICAL STATION
23°07'49"S 134°57'54"W
on Mangareva, Îles Gambier, French Polynesia, South Pacific
About 09:00 hours Local Time, 24 September 2009
After an earlier sharp shower.
Mangareva is a volcanic peak and attracts rain clouds.

Pictures by Anita McConnell

MORE ON THE D-DAY FORECAST
by Anders Persson

If you wish to comment on this article, as we hope you will, please send comments to Malcolm Walker.

As a follow up to the notice in the last newsletter (Newsletter 3, 2009), here are two German weather maps as they were published at the time in their *Tägliche Wetterbericht* (Daily Weather Report).

The first one (Fig.1a) is from 00 GMT on 4 June 1944 and presents an analysis you would expect when only observations from German-controlled territories were available, plus from neutral Spain and Portugal (including the Azores). The analysis over the Atlantic is quite crude, either being based on speculations or isolated reports from German submarines either around 00 GMT or earlier. In either case, the observation would not have been made public (in spite of the "Geheim" status of the weather report).

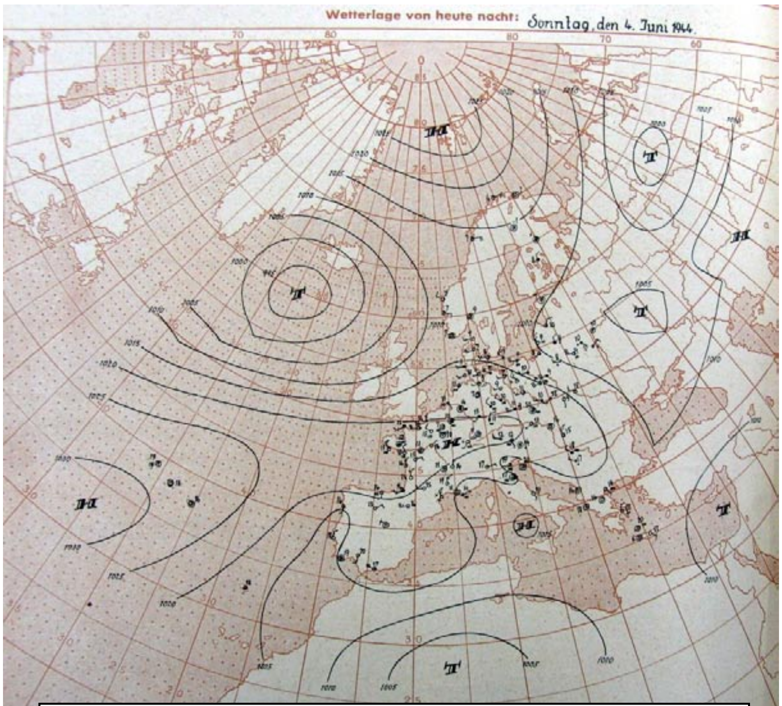

Fig.1a: 4 June 1944, 00 GMT, surface analysis in the German "Tägliche Wetterbericht".

The other weather map (Fig.1b) is from 6 June 00 GMT, two days later. There are still no observations plotted over the Atlantic, but the analysis, which is quite detailed and accurate (compared with later re-analyses) stretches all the way to eastern Canada. About 50% of the 00 GMT weather maps in the Tägliche Wetterbericht, at least in spring and summer 1944 (the period of study), has this coverage. The rest have a coverage as depicted on the 4 June map above.

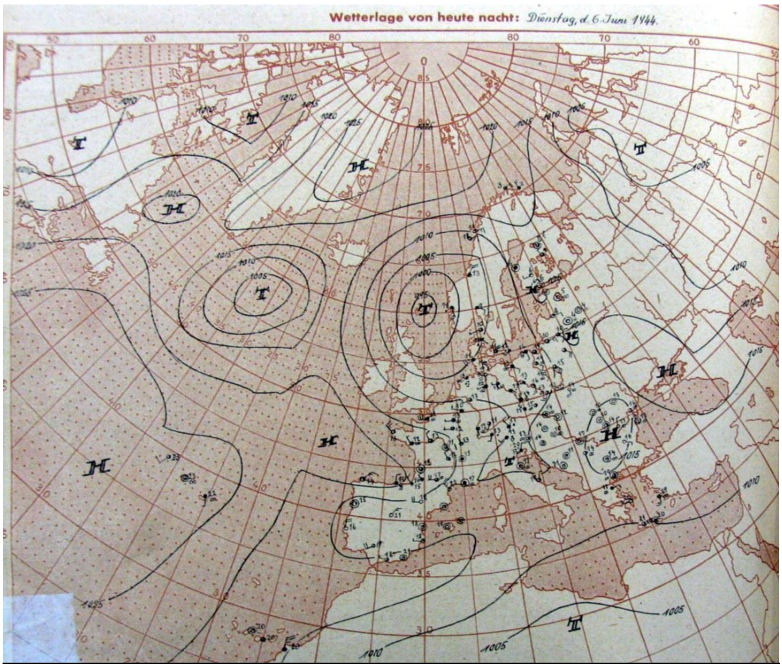

Fig.1b: Same as Fig.1a but for 6 June, 00 GMT. The analysis is quite detailed. Note the 'kinks' in the isobars where probably fronts were analysed. This map, and the one above, agree well with later re-analyses and also with contemporary Allied analyses. This can be taken as an indication that they were based on decoded analysis, rather than observations.

On what information did the German forecasters base their North Atlantic analyses? Their own submarines? Decoded observations from Allied ships? Decoded Allied analyses and/or forecasts?

One should keep in mind that the British and Americans used different codes. Even if the Germans did not manage to decode British information, they might have been more successful with American. When I studied weather maps from April-May 1945 in the archive of the Swedish meteorological service (SMHI), I noted that the Swedes had not got hold of observations from NW Germany, which was controlled by the British, but had indeed from the parts occupied by the Americans.

Finally: there are no observations plotted over Sweden. There was no agreement between Sweden and Germany about exchange of observations. That is why the Swedes had to, and quite successfully managed to, decode a lot of German or German controlled weather information. To what extent the Germans decoded Swedish weather information is not known, but during the whole war there was an exchange of weather information and observations between Sweden and Finland. This can be understood from a recent investigation I made in the archives of SMHI where the routine maps always have good coverage over Finland. Since the policy of Sweden was to help Finland as much as the neutrality allowed, there must have been a mutual exchange. After all, being downwind, the Finns benefited more from Swedish observations than the Swedes from Finnish.

I wouldn't be surprised, considering that Finland between June 1941 and September 1944 was allied with Germany, that the Swedish observations, via Finland, found their way into the hands of German meteorologists. What is more surprising is the presence of Swedish observations on British weather maps. On the 6 June 1944 12 GMT chart displayed in the Met Office National Meteorological Library, there is a very coverage over Sweden.

Did the British manage to decode Swedish observations? Or did they decode these observations when they were sent by the Finns to Germany? The role of meteorological reconnaissance flights on both sides has been well documented, but what about the decoding of each others' weather information?

RECENT PUBLICATIONS

This list of books and articles concerned with the history of meteorology and physical oceanography has been compiled by Malcolm Walker, Margaret Deacon and Anita McConnell.

ANDUAGA, A., 2009. Sydney Chapman and the layering of the atmosphere: conceptual unity and the modelling of the ionosphere. *Annals of Science*, Vol.66, pp.333-344.

BURT, S., 2010. Obituary of Ken Woodley. *Weather*, Vol.65, p.27.

COLEMAN, T.A. and PENCE, K.J., 2009. The proposed 1883 Holden tornado warning system. *Bulletin of the American Meteorological Society*, Vol.90, pp.1789-1796.

CORNFORD, S., 2009. Obituary of Margaret Bushby. *World Meteorological Organization Bulletin*, Vol.58, pp.210-211.

FRIEDMAN, J.S., 2008. *A history of lightning: science, superstition, and amazing stories of survival*. Delacorte Press, 290 pages. ISBN: 978-0-385-34115-8.

MENZEL, W.P. and PHILLIPS, J.M., 2009. Satellite meteorology: how it all started, 50 years ago. *Bulletin of the American Meteorological Society*, Vol.90, pp.1435-1436.

MILLS, A., 2009. Dr Hooke's 'Weather-Clock' and its self-emptying bucket. *Bulletin of the Scientific Instrument Society*, No.102, pp.29-30.

MILLS, E.L., 2009. *The fluid envelope of our planet. How the study of ocean currents became a science*. University of Toronto Press, 434 pages. ISBN: 978-0-802-09697-5.

OHRING, G. *et al*, 2009. Radiation and ozone: catalysts for advancing international atmospheric science programs for over half a century. *Bulletin of the American Meteorological Society*, Vol.90, pp.1669-1681.

PETTIFER, R.E.W., 2009. From observations to forecasts – Part 2. The development of *in situ* upper air measurements. *Weather*, Vol.64, pp.302-308

POTTER, S., 2009. September 16, 1903: the vagabond hurricane. *Weatherwise*, Vol.62 (No.5 – September/October), pp.12-13.

STANSFIELD, A. (Editor), 2009. *100 years of weather: twentieth century in pictures*. Ammonite Press (Press Association Images), 299 pages.
ISBN: 978-1-906672-24-9.

STRANGE, C., 2010. Griffith Taylor's Antarctica: science, sentiment and politics. *Polar Record*, Vol.46, pp.65-74.

ZILLMAN, J.W., 2009. A history of climate activities. *World Meteorological Organization Bulletin*, Vol.58, pp.141-150.

AND ALSO

FERRARI, G. (Editor), 2009. Letters in the Earth Sciences: their historical value and present-day scientific relevance. Complete issue of *Annals of Geophysics*, Vol.52, No.6.

This issue contains papers which were originally presented at two seminars, the first held in Rome in 2002 at what was then the Ufficio Centrale di Ecologia Agraria (UCEA)[3] – now renamed the centre for applied climatology and meteorology, the second

[3] A note on the collection of old meteorological instruments and apparatus held at the Ufficio (examined by Anita McConnell during this seminar) was reported in our Group's newsletter in 2002.

held in Foria, on the Isle of Ischia, in 2003. The papers principally deal with the correspondence of seismologists but often such men worked in observatories dedicated to the earth sciences in general, including meteorology. This volume therefore offers guidance to the extent and location of some of the surviving meteorological records and correspondence in Italy.

An article by Franca Mangianti and Cesare Mangianti deals with the archive of meteorologist Pietro Tacchini (1838–1905) while he was director of Rome's Central Office of Meteorology in 1879-99, and which is now held at the UCEA. Between 1860 and 1901 Tacchini received more than 4500 letters, from over 800 correspondents, of whom some 300 were foreign. This correspondence has been digitised and is available to scholars.

Tacchini was a competent diplomat and administrator, and as first director of the unified Italian government's meteorological service, strove to mould the former regional organizations into a coherent whole, supplying them with money, instruments and proformas for submitting data. He founded journals for publishing this data, first in *La climatologia d'Italia;* from 1879 the *Bolletino meteorico*; adding observations from certain foreign stations in the *Bolletino meteorico internazionale.* Agrarian news found a place in the *Servizio* (later the *Revista*) *meteorico agraria*.

An article by Ileana Chinnici summarises Tacchini's scientific biography (the subject of Ileana Chinnici's doctoral thesis, University of Palermo 1993) and notes other Tacchini archives in Rome and at Modena which are still being explored.

AND MORE

From the Scientific Yearbook of the German Maritime Museum, Deutsches Schiffahrtsarchiv, Vol.31, 2008.

HAAS, Jochen. Stürme auf See und Dürren an Land. Zur Wetter und Witterungsrekonstruktion in frühmittelalterlichen Nordwest und Westeuropa nach Schriftquellen. (Storms at sea and droughts on land: on reconstructing the weather and atmospheric conditions of early medieval North-Western and Western Europe with the aid of written sources), pp.255-287.

From pages 363 to 448: Historische Meteorologische und Meereskundliche Forschungen in Hohen Breiten [papers given at a meeting of the German, Austrian and Swiss Meteorological Societies, Hamburg, 12 September 2007]:

WEGENER, Gerd. Meteorologisches und Ozeanographisches aus der 'Grönlandfahrt'. (Meteorological and oceanographic observations from the 'Greenland expeditions'), pp.365-377.

KRAUSE, Reinhard A. Meteorologie und Geomagnetik als Auslöser der internationalen Polarforschung. Anmerkungen zur Ideengeschichte der Polarhahre. (Meteorology and geomagnetics as catalysts of international polar exploration. Thoughts on the history of the polar year concept.), pp.378-396.

LÜDECKE, Cornelia. Über die globale Verteilung von Luftdruck und Temperatur am Beispiel des Ersten Internationalen Polarjahres 1882/1883. (On the global distribution of air pressure and temperature as determined by the First International Polar Year, 1882-1883), pp.397-411.

LENZ, Walter. Wilhelm Brennecke, Pionier der südozeanischen Tiefenzirkulation, und seine Rolle beim desaströsen Ende der Zweiten Deutschen Südpolar-Expedition 1911/1912. (Wilhelm Brennecke, pioneer of deep ocean circulation in the southern hemisphere and his role in the disastrous conclusion of the Second German Antarctic Expedition of 1911-12), pp.412-420.

STEINHAGEN, Hans. Verlauf und Ergebnisse der Spitzbergen-Expedition von Kurt Wegener und Max Robitzch, 1912/1913. (The 1912-13 Spitsbergen Expedition of Kurt Wegener and Max Robitzch and its results), pp.421-433.

BERNHARDT, Karl-Heinz. Zur Erforschung der polaren troposphärischen Grundschicht vor dem Zweiten Internationalen Polarjahre 1932/1933. (The study of the Polar Tropospheric Boundary Layer prior to the Second International Polar Year, 1932-33), pp.434-448.

DID YOU KNOW?

Did you know that there are Bjerknes Craters on the Moon and the planet Mars, named after Vilhelm Bjerknes? There is also a Buys-Ballot Crater on the Moon.

FORTHCOMING MEETING

On **SATURDAY 17 APRIL 2010**, jointly with the Observing Systems Special Interest Group of the Royal Meteorological Society, there will be a meeting in **London**, in the Zoological Society of London's Huxley Lecture Theatre, Regent's Park, **to mark the 150th anniversary of the formation of what was later called the British Rainfall Organization**.

Programme:

10:30 Coffee and registration

11:00 Welcome and introduction by Stephen Burt

11:10 David Pedgley
The history of the British Rainfall Organization

11:40 Malcolm Walker
The man behind the British Rainfall Organization – George James Symons

12:10 Ian Strangeways
The history of the rain-gauge

12:40 Lunch and exhibition of relevant historical artefacts

13:40 Stephen Burt
British Rainfall 1860-1993

14:10 Harvey Rodda
Digitising the British Rainfall Heavy Falls archive 1866 to 1968

14:40 Tim Allott
The British rainfall network in 2010

15:10 Tea/coffee break

15:40 Malcolm Kitchen
Precipitation measurement: towards the next 150 years?

16:25 Stephen Burt
The Symons memorial commemorations, July 2010

16:35 Closing discussion, round-table questions and answers, exhibition viewing

17:00 Close of meeting

There is no charge for the meeting. However, pre-registration is required, through the Royal Meteorological Society, not the History Group. A sandwich lunch will be available at a cost of £5 and this too needs to be ordered in advance.

The Wind Force Committee of the Royal Meteorological Society aboard HMS *Worcester*, 17 June 1898
Back row from left to right: William Marriott (Assistant Secretary of the Society), Robert William Munro, Richard Henry Curtis, Richard Inwards, Cuthbert Edgar-Peek, Captain David Wilson-Barker
Front row from left to right: William Ellis, George James Symons, Francis Campbell Bayard (Society President), Edward Mawley, William Henry Dines

This committee was set up after the 1879 Tay Bridge disaster and subsequently devoted much attention to anemometry, including experiments on and near HMS *Worcester* in the late 1890s.

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Please send any comments or contributions to:
Malcolm Walker, 2 Eastwick Barton,
Nomansland, Tiverton, Devon, EX16 8PP.
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The Group's annual subscription is £5 (cheques payable to *Royal Meteorological Society History Group*). A reminder will be sent when your subscription is due.

AND IN THE NEXT NEWSLETTER

This year, the Royal Society celebrates the 350th anniversary of its foundation (on 30 November 1660). In our next newsletter, there will be an article about work in meteorology and oceanography carried out by Fellows of the Royal Society in the 1660s.