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Early Developments in the Theory of the Saturated Adiabatic Process

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ABSTRACT

A study of the history of the early developments in the theory of the saturated adiabatic process reveals that the first quantitative formulation of both the dry- and the saturated-adiabatic cooling rates were given by the British physicist, William Thomson (later Lord Kelvin), in a paper read in 1862, although the first *published* analysis was presented in 1864 by a Swiss meteorologist, Reye. Reye's paper presents both the differential form and the exact integrated form of the reversible saturated adiabatic relationship, which thus attained its modern form at the moment of its first appearance. Reye also gave a very clear, though brief, discussion of the concepts of atmospheric stability of both dry and saturated layers. The papers of Hann (1874) and Hertz (1884), frequently misquoted as the original treatments of the saturated adiabatic theory, are thus shown to be secondary papers in the actual history of this important theory.

1. Introduction

One of the fundamental principles of meteorology that every student of the subject must grasp early in his career is the principle that the only process capable of bringing about condensation rapidly enough and on a sufficiently large scale to result in significant amounts of precipitation is the adiabatic cooling accompanying expansion of rising currents of saturated air. This principle only came to be clearly understood and generally accepted by meteorologists in the last quarter of the nineteenth century, the period during which almost all of what we now regard as the fundamentals of meteorological thermodynamics were being established.

Thus, in 1874, when the eminent Austrian meteorologist, Hann, was writing to introduce his countrymen to recent exciting theoretical developments in other countries (Hann, 1874), he had to

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begin his analysis of adiabatic cooling with the observation that "only in the last ten years have we really begun to fully credit and establish physically the great importance of the ascending movement of air for a whole series of atmospheric phenomena." Hann then goes on to note that although the cooling of air could long since have been computed using "an equation deduced by Poisson," the long-standing theory that precipitation forms chiefly as a result of cooling of air currents that impinge on cold mountaintops was still to be found in a then-standard reference written by Schmidt as recently as 1860. This confusion can be more vividly visualized if one recalls the analogous confusion, still quite prevalent in meteorology less than twenty years ago, as to the physical explanation of the very common occurrence of undercooling of water drops in clouds, or if one reflects on the present confusion as to the physical explanation of the mechanisms of separation of charge within thunderclouds. To the student of meteorology a hundred years ago, the explanation of the causes of precipitation was similarly confused.

Almost equally popular as a theory of the origin of precipitation in that era was the notion (due to the English geologist, James Hutton) that it resulted from the mixing of two saturated, or very nearly saturated, masses of air of different temperatures. Even twenty years after Hann, the position taken by writers remained somewhat equivocal, and we find Waldo (1896, p. 135), in a standard American textbook, *Elementary Meteorology,* admitting that adiabatic cooling probably accounted for most precipitation, but stressing that mixing of air masses of different temperatures was of basic importance in the formation of the clouds themselves.

What were the steps by which this question of the origin of clouds and precipitation (viewed in just the classical, thermodynamic sense, not the more modern cloud physical sense implying questions of nucleation and large-particle growth) came to be clarified and based on sound thermodynamic reasoning? Who were the meteorologists and physicists who first carried over into meteorology the then-new findings of the "mechanical theory of heat" which Clausius, Kelvin and contemporaries were founding upon the experimental efforts of preceding decades? How did this transfer of knowledge happen to occur?

2. Outline of main developments

Present misconceptions. That the true history of the development of the saturated adiabatic theory (which will hereinafter be referred to, for brevity, as the S-theory, or as the theory of the S-process) is not well understood can be stressed by noting that I have been unable to find a single publication after the turn of the century that gives proper credit to those whom the present study indicates to be the real originators of this theory. A check of references made in texts published in the past twenty-five years revealed that most of them state that the S-theory was due to Hertz (1884), with a smaller number going back a bit further in time (but really moving somewhat farther from the truth) in citing the above-mentioned paper by Hann (1874). Even in that earliest of twentieth-century texts treating meteorology in something more than a descriptive manner, the text of Exner (1917), one finds Hertz cited as the originator, with a further reference to Neuhoff (1900). Also Shaw, who gives us so much meteorological history intercalated among his profusion of topics, writes (1926, vol. 3, p. 266) of "the original treatment by Hertz," without further clarification.

The misconceptions corrected. In contrast to these versions of the history of the S-theory, it appears that *the first paper containing a quantitative and thermodynamically accurate description of the S-process was given in 1862 by William Thomson (later Lord Kelvin)*, although not actually published, for some reason, until three years later (Thomson, 1865). This publication delay provides a small bone of historical contention inasmuch as another and more elegant presentation of the S-theory was actually published in 1864 one year prior to publication of Thomson's paper, by Reye (1864), a Swiss meteorologist. Then, well before Hann enters our story, a French engineer, Peslin (1868), gave a treatment much like Thomson's, though in more finished form. The reader is given no hint by either Reye or Peslin as to whether either was aware of Thomson's work. My guess is that it is unlikely that Reye in Zurich could have known of the unpublished paper Thomson read in Manchester just two years prior to the date of January 7, 1864, when Reye submitted his own paper. On the other hand, it seems rather likely that Peslin must have been aware of either Thomson's or Reye's work by the time he was writing in 1868. I shall return below to these questions of priority.

The next step in the development of the S-theory appears to have been the publication of Hann's paper (1874), which was presented only as a summary of the previous work of Thomson, Reye and Peslin, and hence could not be misconstrued as an original effort by anyone who had read Hann's paper since Hann himself is quite explicit on this point. Then, passing over the 1878 work of Guldberg and Mohn, which was chiefly concerned with dynamical matters but did contain a resume of the S-theory as already developed to that date, we come to the work of Hertz (1884). Here again, I would stress that anyone who had studied the original paper could not have drawn the conclusion that Hertz should be credited with originating the modern theory of the S-process. Hertz' own introductory comments are as explicit on this score as are Hann's, since he notes that, "This contains nothing theoretically new except insofar as that it also completely considers the peculiar behavior of moist air at 0C, which, so far as I know, has hitherto not been treated of." From a practical point of view, however, there *was* something new in Hertz' paper, namely his adiabatic diagram, the prototype of all our subsequent thermodynamic diagrams.

Four years later, von Bezold (1888) published the first of a series of several long papers on meteorological thermodynamics, which are perhaps most noteworthy for their extremely detailed discussions of many points of theory, and for the clear-cut introduction of the concept of the "pseudoadiabatic" process and formulation of the first exact equation for that latter process. Earlier writers had, in effect, talked about the pseudoadiabatic process, noting that one might neglect the thermal effects of the condensate with little numerical error, as did Hertz, for instance. But this point was made quite explicit for the first time by von Bezold, and it is for this and for his pseudoadiabatic equation that we should remember his work.

This brief summary of early developments in the S-theory can be concluded by noting that Neuhoff (1900), a student of von Bezold's, published a rather lengthy paper which, though often referenced, added nothing essentially new to the S-theory. Neuhoff's paper apparently came to be frequently cited in the literature chiefly because he published a much-used thermodynamic diagram, slightly different in its mode of construction from that of Hertz, with linear rather than logarithmic temperature scale, having no "hail-stage" discontinuities at the 0C isotherm, and with pseudo-adiabats based on a better approximation than that used by Hertz.

Indeed, if the concepts of entrainment and turbulent exchange between cloud and environment be excluded from present consideration on the ground that those concepts concern intrinsically nonadiabatic processes, we can say that the S-theory was brought to fully-completed form by 1888, the date of von Bezold's first paper. Throughout the ensuing three-quarters of a century, it seems to me that only a single additional concept of fundamental significance has been added, though it has gone essentially unnoticed. I refer to a principle discussed by Brunt (1934) from which it follows that one cannot take it quite for granted that clouds form as a result of saturated adiabatic expansion. Brunt showed that this latter familiar pattern results only from the circumstance that water vapor has a high latent heat of condensation, and he noted that there exists the possibility of having planetary atmospheres whose condensable vapors have sufficiently low latent heats that clouds are formed by saturated adiabatic compression in downdrafts. Since I hope to discuss that point in more detail elsewhere, I shall only here stress the view that Brunt's paper seems not to have received due recognition for its fundamental significance in the theory of cloud formation by the saturated adiabatic process.

Having outlined, in general, the principal steps leading to our modern view of the S-theory, I shall now examine each step in more detail.

3. First formulation: Thomson, 1862

The year 1962 was the centennial year for both the first formulation of the dry adiabatic lapse rate and first formulation of the saturated adiabatic lapse rate, since the paper which William Thomson read on January 21, 1862, to the Literary and Philosophical Society of Manchester contained quantitative formulations of each of these two meteorologically important quantities. This was in the middle of the period during which Thomson was devoting a major portion of his considerable energies to both fundamental scientific problems and engineering and administrative problems associated with the laying of the Atlantic submarine cable and its perfection as a communication device. In 1857, Thomas had analysed the problem of the adiabatic temperature gradient in the oceans, and that analysis doubtless played some role in turning his thoughts to the analogous problem in the atmosphere. Biographers offer us no hint of how Thomson happened to be thinking about atmospheric lapse rates at this time, but the range of subjects entering into Thomson's roughly 600 published papers is so great that one need not be too surprised that the topic caught his attention in the midst of his cable activities and while he was preparing the manuscript, with Tait, for his lengthy *Treatise on Natural Philosophy*.

The paper itself plus knowledge of what events were occurring in the world of thermodynamics at that time, provide at least interesting clues. Whether steam condenses on expanding or on compressing was a question that had been of interest since the days of Watt; and, in the years just prior to 1862, evidence was accumulating that showed fairly conclusively that expansion was, in fact, required. That is, the evidence indicated that the so-called "specific heat of saturated water vapor" was actually negative. The definitive proof thereof is usually taken as the work of Hirn; and since that work was done in 1862, we can imagine the great theoretical interest then centering around any and all questions concerned with the thermodynamics of vapor condensation. From the paper itself, we find Thomson crediting his colleague in research, James Joule, for the *qualitative* suggestion that one might account for observed atmospheric lapse rates by making some allowance for latent heat release during cloud-condensation. Joule was no mathematician, and in more than this one instance (Crowther, 1936) provided the physical insight that permitted more mathematically inclined associates to derive important new relationships. It is very interesting to realize that we must divide credit between those two eminent nineteenth-century British physicists, Thomson and Joule, for the conception and mathematical formulation of the relationship for the saturated adiabatic cooling rate.

A final clue as to why Joule and Thomson might have been discussing the lapse rate problem is found in the recency of the first systematic balloon ascents in which upper-air temperature distributions were accurately observed. In 1852, John Welsh, superintendent of a physical observatory set up by the British Association, made four balloon ascents measuring pressure, temperature, and humidity, attaining 22,930 ft on one occasion (Shaw, 1926, vol. 1, pp. 207, 223). It was lapse rate information from Welsh's ascents that Thomson used to check his theoretical computations in the 1862 paper. In all, we may be close to the truth to surmise that the Thomson paper had a history somewhat as follows: Preliminary computations of the magnitude of the dry-adiabatic lapse rate by Thomson (which comprise the first short section of the paper) failed to fit Welsh's data closely, giving too steep a lapse rate. With the recent findings on the expansional condensation of steam in mind, Joule suggested that Thomson's idea of an atmosphere in "convective equilibrium" (a further important concept advanced in the paper) would be altered if proper allowance could be made for latent heat release during cloudy ascent of air in such a well-stirred atmosphere. Thomson worked out a theory for the latter process and found that it agreed very closely with Welsh's lapse rates of about 6C/km, and presented the results in his Manchester paper. At least this speculative history would fit all the known facts quite well.

The actual methods by which Thomson derived the dry- and the saturated-adiabatic lapse rates seem very awkward from a modern point of view. But there is nothing unsure about the physical argument underlying the mathematical steps, even though the latter, too, seem very strange today, largely because in deriving his equation for the S-process, Thomson views the problem in terms of volumes rather than masses of air and vapor. His specification of the amount of condensate is made by subtracting the actual volume-increment for the moist air from the volume-increment that the saturated vapor would exhibit if no condensation occurred. The difference, he notes, must represent the volume of vapor condensed in an infinitesimal expansion step. In this frame of reference, his latent heat is a latent heat per unit volume of vapor, a most unhandy conception. His approach to the evaluation of this latent heat also seems very odd, today: Whereas we would merely consult the *Smithsonian Tables,* Thomson felt it best to evaluate the latent heat indirectly from the Clausius-Clapeyron equation, and also evaluated the specific volume of saturated vapor by the same equation, relying on Regnault's pressure data for water vapor as the basic input information. Curiously, Thomson does not credit either his German contemporary, Clausius, nor his French predecessor in thermodynamics, Clapeyron, for this equation, but cites an earlier paper by Joule and himself in which that equation was given. Crowther (1936) attributes this failure to credit properly earlier work primarily to Thomson's lack of historical sense and not to any desire on Thomson's part to take undue credit.

Thomson gave no neat, closed formula for the saturated adiabatic lapse rate in his paper. Rather, his results are embodied in a table giving the height in meters through which one must ascend in a saturated atmosphere in convective (adiabatically mixed) equilibrium in order to experience a temperature decrease of 1C. For a temperature

of 0C he finds 152 m, for 35C he finds 284 m, both at sea level. Corresponding modern values read from the *Smithsonian Tables* (List, 1951) are 155 m and 310 m, at 1000 mb, so Thomson's values agree very well with current values at lower temperatures, but his basic vapor pressure data were evidently not too accurate at higher temperatures. Since the lower-temperature case matched Welsh's ascent temperatures and lapse rates very encouragingly, Thomson drew the conclusion that observed lapse rates are explainable in terms of convective equilibrium arguments, with proper allowance being made for latent heat effects. Today, we would say that this was a somewhat oversimplified view of the mean tropospheric lapse rate. As in many other instances in the history of science, a new development was here brought to light because its author found agreement with observations which later insights show to be partly fortuitous.

The 1862 paper is roughly written. No one would praise its smoothness nor praise it for elegance. It contains a number of both notational and numerical errors. Crowther (1936) points out that Thomson's "indiscipline penetrated down to his working habits. He used to write papers in pencil, often on odd pieces of paper, and send them in this condition to the printers." The units employed in the paper are strange from our present-day viewpoint, but probably Thomson would therein only agree.[1] But, notwithstanding these defects of mere polish, the paper introduced for the first time, and introduced correctly, the basic quantitative relations governing two very important meteorological processes, dry- and saturated-adiabatic ascent and descent.

Why did Thomson not publish this short paper immediately? Was it his practice to let work lie as long as three years between completion and publication? To check the latter, I tallied the bibliographical data appearing with 154 of his papers, comprising the first four volumes of his collected works. In only 17 of these was the paper read in a year different from the year of its publication, and in 16 of those 17, the lag was only one year. The 1862 paper's three-year lag in publication is thus unique. Only a student of the history of science intimately familiar with Thomson's working methods and working habits would be able to shed real light on the implication of this lag. Is it possible that Thomson originally delivered the paper with the thought that it was of only secondary scientific interest and that two or three years later, hearing of Reye's (1864) paper, he decided to submit a rough summary for 1865 publication in the Manchester Society *Memoirs*?

4. Reye's 1864 formulation of the reversible saturated adiabatic relationship

As has been noted above, Reye does not mention Thomson's work. The latter, still over a year from formal publication on the January 7, 1864 date on which Reye submitted his paper from Zurich to a Leipzig journal, could have been known to Reye only by some accidental chain of communication. That communication of meteorological results was, in general, still very imperfect in that era is documented for us by Hann's (1874) paper, the entire point of which was to acquaint Hann's Austrian colleagues with the work of Thomson, Reye, and Peslin, of which the first two efforts were then a decade old. A further good reason for concluding that Reye's work is to be viewed as a wholly independent effort is seen in the very different mode of approach used by Reye.

Reye's paper is chiefly concerned with the question of how whirlwinds and severe storms develop. Reye tilts with Mohr, a contemporary who urged the view that sudden condensation of vapor within a storm cloud created a partial vacuum into which air rushed from below and from the sides, creating tornadic circulations. The paper is fairly long, and the theoretical work appears only near its close. However, though Reye's cloud dynamics is badly dated by his concern with Mohr's odd thesis, his thermodynamics is above reproach. In about one page, Reye proceeds from first principles to a formulation of the differential equation for the strictly reversible saturated adiabatic equation and then to its exact integration. Therefore, we can

[1] In searching his works, I found a delightful footnote on units in which Thomson condemns continued use of English units in place of metric units. Because that issue is just as rankling today, it seems worth quoting. A paper entitled "Note On the Possible Density of the Luminiferous Medium and on the Mechanical Value of a Cubic Mile of Sunlight," written in 1854, received this salty 1882 footnote by Thomson in the course of editorial preparation of the collected works: "The brain-wasting perversity of the insular inertia which still condemns British engineers to reckonings of miles and yards and feet and inches and grains and pounds and ounces and acres is curiously illustrated by the title and numerical results of this Article." Could it be said better?

assert unequivocally that the theory of the reversible S-process came full-blown [2] into existence in final form in the 1864 paper of Reye, leaving essentially nothing new for any later worker to add!

Reye's approach does not explicitly bring in the "specific heat of saturated vapor," but his reference to Zeuner's text on thermodynamics leads to introduction of a relation which stems directly from that concept. He discusses the problem of numerical solution of the transcendental adiabatic equation, and gives several quantitative implications of his equations. A specific comparison with the Poisson equation for dry air parallels, conceptually, Thomson's so very closely as to tempt one to conclude that Reye did know something of Thomson's Manchester paper; but this is unsafe speculation. He concludes his analyses with a brief but an astonishingly modern treatment of stability considerations, showing that saturated layers would be in neutral equilibrium if their lapse rates are about 6C/km. I believe one can say that Reye's is a rather more correct application of S-theory than Thomson's, for Thomson concluded that the atmosphere really is in full convective equilibrium in the saturated adiabatic sense, whereas Reye uses his theory of the S-process only to discuss the very real phenomenon of lapse-rate instability. Reye carefully distinguishes stability criteria for dry and for saturated layers, obtaining the correct numerical values of lapse rates for each, and doing so in a mathematically smoother manner than did Thomson.

Clearly, whatever further historical search might reveal as to the origins of Reye's work, his 1864 paper deserves to be recognized as one of the classical papers of meteorological thermodynamics. That this Zurich Privatdocent's work is, instead, not even mentioned in a single account written after Neuhoff's 1900 paper, shows how little we have known of the real history of the S-theory. It would be of interest to know more of Reye; perhaps someone closer to original sources bearing on his activities will find it possible at some time in the future to illuminate the history of his work and of his important 1864 paper, in particular.

[2] Note added in press: The writer has, since the above writing, come upon a much earlier discussion of the *physical* aspects of the S-process in the work of the early American meteorologist, James Espy. Espy's ideas thereon are so interesting and so far ahead of his time as to warrant separate treatment which the present writer hopes to give in a future paper.

5. Peslin's contribution

A paper by M. Peslin (1868) discussing precipitation and storms, begins with a concise discussion of dry and saturated adiabatic processes. Peslin's sole reference is to a French thermodynamics text by Combes, from which he draws the Clausius-Clapeyron equation (attributed in an historically inaccurate way to Zeuner's use of Carnot theory); we are left uninformed as to whether he was aware of the papers of Thomson and of Reye published several years earlier. It would be remarkable if in the short span of four years, three different workers independently derived the dry- and the saturated-adiabatic relations, although the time was clearly ripe for this development, and such nearly simultaneous innovations have occurred at other times in the history of science. Unlike Reye, who must have been something of a meteorologist by profession, Peslin was a mining engineer at Tarbes, so it is barely conceivable that he may not have been in touch with contemporary meteorological work in other countries and worked out his theory from first principles independent of Reye or Thomson.

It is interesting to note that Peslin's manner of deriving his equations is very modern in tone. He does not give a derivation of the reversible saturated adiabatic equation, as did Reye, but approaches the problem more in the manner of Thomson, seeking a direct expression for the saturated adiabatic cooling rate. His method does not strike one as awkward in the way Thomson's does; indeed, Peslin's derivation is almost indistinguishable from a derivation often used in current texts (e.g., Brunt, 1944, p. 65; Haurwitz, 1941, p. 55).

6. The work of Hann, Hertz, and von Bezold

As has been stressed earlier, Hann's paper, which is often misquoted as presenting original contributions to the S-theory was only offered as a summary paper. Its role was one very much like that of numerous papers to be found in volumes of the *Monthly Weather Review* about forty years ago, or in the *AMS Bulletin* of that same era, papers conveying results of new and important work recently done abroad. Hann recognized in the work of such foreign scientists as Thomson, Reye, and Peslin, the first meteorological fruits of the new science of thermodynamics, and, for the benefit of Austrian meteorologists, Hann brought together their main findings in his 1874 paper. He gives Thomson's derivation of the *dry* adiabatic relation, but wisely prefers Peslin's to

Thomson's derivation of the *saturated* adiabatic cooling rate. Hann does mention Reye, but seems not to recognize that Reye's derivation of the reversible equation in integrated form was, at least formally, a more handsome show of thermodynamic virtuosity than that to be found in either Thomson's or Peslin's paper. Hann used the Peslin equation to prepare a table, that came to be known as "Hann's table," giving values of the saturated adiabatic cooling rate for a series of discrete pressures and temperatures that covered the principal range of meteorological interest, though not at a very dense network of points. Hann compared the cooling rates of his table with balloonists' lapse-rate observations, having by that date not only Welsh's observations but also those made by Glaisher (Manley, 1962) and by Kaemtz. The precise nature of the interrelationship between an adiabatic cooling rate, on the one hand, and a lapse rate, on the other hand, was not as clear to Hann as it had been to Thomson. The latter, at least, explicitly recognized that the two would only be equal if the atmosphere were in "convective equilibrium" under control of the given adiabatic cooling law.

When we come to Heinrich Hertz's (1884) contribution, a contribution so often cited as the original paper in the S-theory, we are, it should now be clear, coming to the end rather than the beginning of the story. Hertz gave us not the S-theory, but the prototype of all thermodynamic diagrams. Why Hertz might have been busying himself with such matters is much more difficult to guess than it was to guess how Thomson might have been led to the topic of his 1862 paper in Manchester. Hertz, a student of Helmholtz at the University of Berlin, received his doctorate there in 1880, and after several years' work as Helmholtz's assistant became a Privatdocent in Kiel in 1883. It was in Kiel that Hertz began laying the ground work, both theoretical and experimental, for his celebrated electromagnetic work. I have sought unsuccessfully, in such sources as are at my disposal, for clues as to what might have aroused Hertz's interest in the seemingly remote subject of S-theory. In 1882, Hertz had written a short paper on a hygrometer, based on a hygroscopic absorption technique, which he had devised. This is his only other publication even indirectly related to meteorology, as far as I can learn. Philipp Lenard's preface to Hertz's *Miscellaneous Papers* (Hertz, 1896) gives us our only glimmer; speaking very briefly of the adiabatic diagram, Lenard remarks that, "the drawing of this, as a recreation after other work, seems to have given Hertz a great pleasure." It was called to my attention by D. O. Staley that Hertz and one of the Bjerknes had once collaborated, which seemed to offer promise of a clue as to what might have led Hertz to the subject. But Petterssen (1962) indicates that this was a collaboration with V. Bjerknes that only began in 1890, six years after the publication of his diagram, so that is not how Hertz happened to be interested in the S-theory.

Hertz offered his adiabatic diagram as exactly the useful graphical aid that such diagrams remain to this day. He claimed nothing new theoretically, as had been noted above, but urged that his chart would permit rapid analysis of a number of thermodynamic processes which were difficult to work out solely with the aid of Hann's table. Except for a rotation of axes, Hertz laid out his diagram (Fig. 1) in a form that has become standard for pseudoadiabatic charts. Both temperature and pressure axes were on a logarathmic scale. The chart had dry adiabats, saturated adiabats, and lines of constant saturation mixing ratio. Hertz computed his saturated adiabats using one of a variety of approximate forms of the pseudoadiabatic equation, and not one of the best. However, its slight numerical defects were unlikely to throw off any worker of that era, or even ours. The one novel idea (which proved to be unrealistic) was Hertz's introduction of the idea of the "hail stage." Although his saturated adiabats were computed as pseudoadiabats, Hertz recognized clearly that some, possibly all, of the condensate might be carried up with the rising parcel. When such a parcel with its liquid water reached a temperature of 0C, Hertz visualized that the water must begin immediately to freeze, whence the released latent heat of fusion would preclude further cooling until all the liquid had frozen. Since the expansion work would be done solely at the expense of the heat of fusion, each saturated adiabat in the Hertz diagram had an isothermal jog toward lower pressure where it met the 0C isotherm (see Fig. 1). An auxiliary portion of the chart served as a sort of nomogram for estimating fractional portions of this jog when air parcels contained less than the maximum liquid water content possible with saturation at sea-level pressure. Today we know that undercooling of the condensate is the norm, whereas immediate freezing on reaching 0C is the rare exception. When Neuhoff prepared his revision of the Hertz diagram in 1900 he omitted these hail-stage jogs in his pseudoadiabats on the grounds that the pseudoadiabatic process did not admit of

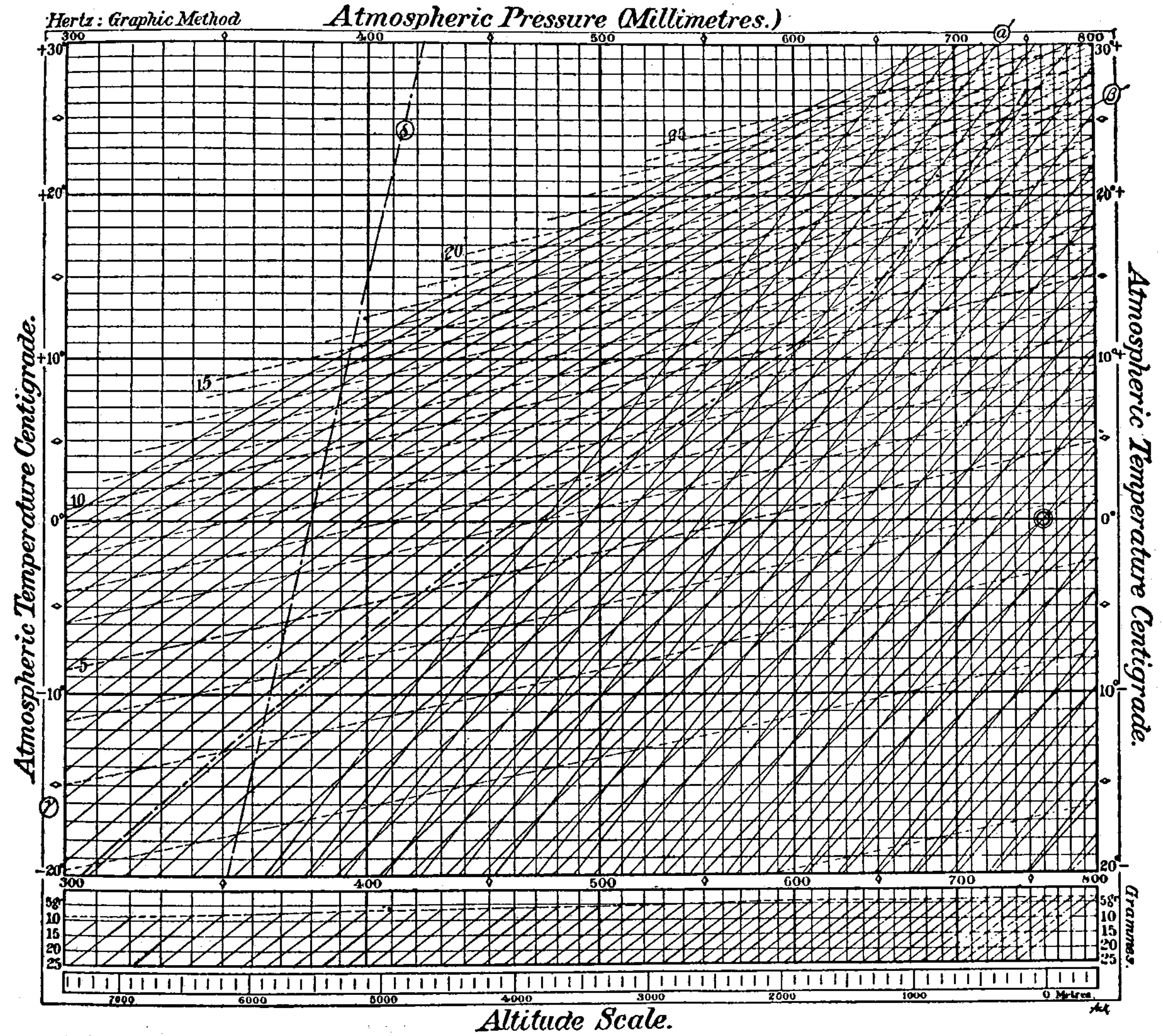

FIG. 1. The original Hertz adiabatic diagram. Rotate ninety degrees clockwise for comparison with currently used pseudoadiabatic diagrams. Note auxiliary panel at bottom for hail-stage determinations.

such a stage, not because at that early date it was recognized that undercooling was so common in clouds. Neuhoff was right for the wrong reasons.

Four years after publication of the Hertz diagram, Wilhelm von Bezold, director of the Bavarian meteorological service, published the first of five communications (1888–1906) on meteorological thermodynamics. Of these, the first (von Bezold, 1888) is of the greatest present interest. In that paper, von Bezold introduced the concept of the "pseudoadiabatic process" in exactly its present sense, and gave the exact (but non-integrable) equation for this process. His paper contains no other original contributions to the S-theory, and seems a bit over-written when read today. However, the latter characteristic stems from the great detail in which von Bezold covered each topic, and it is probably true that his series of papers helpfully brought together in organized form the full S-theory and showed how it could be applied in a number of problems; so contemporaries undoubtedly profited greatly from von Bezold's work. Clearly, in von Bezold's papers we move from the realm of original research in the S-theory to the realm of good textbook presentation, and nothing very exciting was destined to be done on the physiology of clouds for another half-century.

REFERENCES

Brunt, D., 1934: The possibility of condensation by descent of air. *Quart. J. R. meteor. Soc.*, **60**, 279–284.

——, 1944: *Physical and dynamical meteorology*, 2d ed. Cambridge, Cambridge Univ. Press, 428 pp.

Crowther, J. G., 1936: *Men of science.* New York, W. W. Norton Co., 332 pp.

Exner, F. M., 1917: *Dynamische Meteorologie.* Leipzig, B. G. Taubner, 308 pp.

Hann, J., 1874: The laws of the variation of temperature in ascending currents of air, and some of the most important consequences deducible therefrom. Transl. from *Zeit. Oest. Gesell. Meteor.*, **9**, 1874, by C. Abbe (First Misc. Coll.), *Ann. Rept. Bd. of Regents.* Washington, Smithsonian Inst., 1877, pp. 397–418.

Haurwitz, B., 1941: *Dynamic meteorology.* New York, McGraw-Hill, 364 pp.

Hertz, H., 1884: A graphic method of determining the adiabatic changes in the condition of moist air. Transl. from *Meteor. Zeit.*, **1**, 1884, by C. Abbe (Second Misc. Coll.), *Smithsonian Misc. Coll.*, **34**, 1893. Washington, Smithsonian Inst., pp. 198–211.

——, 1896: *Miscellaneous papers.* Transl. by D. E. Jones and G. A. Schott. London, Macmillan, 340 pp.

List, R. J., 1951: *Smithsonian meteorological tables*, 6th rev. ed. Washington, Smithsonian Inst., 527 pp.

Manley, G., 1962: A venturesome Victorian. *Weather*, **17**, 340–341.

Neuhoff, O., 1900: Adiabatic changes of condition of moist air and their determination by numerical and graphical methods. Transl. from *Memoirs R. Pruss. Meteor. Inst.*, **1**, 271–306, by C. Abbe (Third Misc. Coll.), *Smithsonian Misc. Coll.*, **51**, 1910. Washington Inst., pp. 430–493.

Peslin, M., 1868: Sur les mouvements generaux de l'atmosphere. *Bull. Hebd. de l'Assoc. Scientifique de France*, **3**, 299–320.

Petterssen, S., 1962: Vilhelm Bjerknes: an example of the essential ingredient. *Bull. Amer. meteor. Soc.*, **43**, 301–302.

Reye, T., 1864: Ueber vertikale Luftstroeme in der Atmosphäre. *Zeit. f. Math. u. Phys.*, **9**, 250–276.

Shaw, N., 1926: *Manual of meteorology.* Cambridge, Cambridge Univ. Press, in four volumes.

Thomson, W., 1865: On the convective equilibrium of temperature in the atmosphere. *Mem. Lit. and Phil. Soc. Manchester*, **2**, 125–131. See also Kelvin's *Mathematical and Physical Papers*, v. 3, Cambridge, Cambridge Univ. Press, 1890, pp. 255–260.

von Bezold, W., 1888: On the thermodynamics of the atmosphere (First commum.). Transl. from *Sitzb. Koenig. Pruss. Akad. Wiss. Berl.*, 1888, pp. 485–522, by C. Abbe (Second Misc. Coll.), *Smithsonian Misc. Coll.*, **34**, 1893, Washington, Smithsonian Inst., pp. 212–242.

Waldo, F., 1896: *Elementary meteorology.* New York, American Book Co.

NEWS AND NOTES

Satellite Designation System Revised

Beginning 1 January 1963, the international system for designating satellites and space probes for scientific purposes was changed from Greek letters to Arabic numerals.

Prior to 1963, satellites were named in the order of the letters of the Greek alphabet, beginning anew each year: the first satellite (Sputnik I) was 1957 Alpha, the first 1958 satellite (Explorer II) was 1958 Alpha, the second (Vanguard I) was 1958 Beta, and so on. The first satellite or space probe in 1963 will be 1963–1, the second will be 1963-2, etc. The numbering will also begin anew each year; for example, the fifth space vehicle of 1964 will be 1964–5.

Usually the launching of a satellite places more than one object in orbit. The burned-out rocket casing goes into orbit, and sometimes two or more satellites may be carried into space where they are separated and ejected into separate orbits. The new system provides that the suffix A will identify the main satellite or space probe (the one carrying the principal scientific payload), and that B, C, etc. as needed will be used first for any subsidiary scientific payloads in separate orbits, and then for inert components. Thus, under the old system the navigation satellite, Transit II-A, its piggyback companion, Greb, and the spent rocket which ejected them into orbit, were called 1960 Eta 1, 1960 Eta 2, and 1960 Eta 3, respectively. If the new scheme had been in effect, they would have been called 1960–7A, 1960–7B, and 1960–7C, respectively.

The new system was agreed upon by all national members of the Committee on Space Research (COSPAR) at its meeting in Washington in May 1962. In the United States this system has been adopted by the National Aeronautics and Space Administration and the Department of Defense. It will also be used in registering U. S. satellites and space probes with the United Nations.

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