James Espy and the Beginnings of Cloud Thermodynamics

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ABSTRACT

The work of the nineteenth-century American meteorologist, James F. Espy, is discussed in relationship to the early development of understanding of the thermodynamics of clouds and the dynamics of convection. Espy was the first to recognize the important role of latent heat of condensation in sustaining cloud and storm circulations.

1. Introduction

In a recent discussion of the history of the theory of the saturated adiabatic process (McDonald, 1963), I pointed out that we apparently owe the first quantitative formulation of that theory to William Thomson (later Lord Kelvin), and that Thomson was, in turn, directly indebted to James Joule for the key idea that latent heat release in ascending saturated air must be a significant meteorological process. Thomson's work was done in 1862, and was published in 1865, one year after an apparently independent analysis had been published by Rey in Switzerland.

The present discussion will be devoted to the related contributions of a still earlier worker, the American meteorologist, James Pollard Espy. Although Espy did not present a quantitative theoretical formulation of cloud thermodynamical processes as Kelvin and Rey did some twenty-five years later, we shall see that Espy had by 1840, deduced from both observation and experiment a fundamental principle of cloud thermodynamics and cloud dynamics, namely that latent heat is the basic motive agent in cloud circulations. Particularly impressive from the modern point of view is the fact that Espy was able to employ experimental results, along with the thermodynamic data then extant, to make fairly accurate numerical estimates of these latent heat effects. He derived some of his principal information from an instrument which he called the "nephoscope" (cloud watcher), the forerunner of all modern expansion cloud chambers and certainly one of the first pieces of laboratory equipment used in the history of cloud physics. Indeed, with his nephoscope, he arrived at tolerably correct estimates of the dry and the saturated adiabatic cooling rates over twenty years before Thomson's theoretical treatment of these rates.

Espy's work in meteorological thermodynamics, though widely known for a time (Espy lectured in England and on the continent), was rather quickly lost from sight because it was embedded in a larger thesis doomed to be shown erroneous. I shall outline Espy's principal insights and findings, beginning with a brief summary of his ideas on the kinematics of storms. The latter ideas dominated Espy's writings and represented, in his own estimate, his main contribution to meteorology, an estimate which time has shown to be wrong.

2. Espy's Philosophy of Storms

To bring into proper perspective his pioneering work on cloud thermodynamics, we must first briefly examine Espy's principal publication, his Philosohy of Storms (1841), which was the culmination of a dozen years of work and study aimed at 1) understanding the nature of the wind pattern around storms and 2) explaining how the low pressure center of a storm is maintained despite steady convergence of air. Espy advanced the view that the winds blow radially inward on all sides of a cyclone, and the bulk of his long book is devoted to case studies of individual storms for which the none-too-adequate wind reports were believed by Espy to fit this "centripetal hypothesis.

His contemporary, William Redfield, was currently championing the view that the winds blew tangentially around northern hemisphere cyclones and hurricanes in counterclockwise sense (Redfield, 1831). One or two other contemporaries still held that the circulation around northern hemisphere cyclones was clockwise. There were practical as well as theoretical reasons why these workers sought to settle the direction of storm winds: once the relation was established, mariners could pick a safe tack to avoid a nearby storm center. This was the period when earth-rotation effects were first being clarified observationally as well as theoretically. G. Coriolis' 1835 paper on motion in a rotating coordinate system, though already published by the time Espy wrote, had not yet influenced meteorologists' thinking, being destined to enter the mainstream of meteorological work only about a quarter of a century later.

Espy was not the first to defend the centripetal hypothesis (the German meteorologist and mathematician, H. W. Brandes, having published on it in 1820 and 1826), but he was the first to recognize that continued radial inflow could be made possible by convergence maintained by latent heat release. Therein he offered an evident improvement over Brandes' suggestion that "a cyclonic storm arises from air breaking through into a vacuum, or rushing towards regions occupied by denser air" (ibid., p. 300). The fact that Espy spent seven years (1833-1840) assembling wind observations to test the centripetal hypothesis is a tribute to his thoroughness, whereas the fact that he was able to see in those observations confirmation of his erroneous hypothesis is a tribute to the poor quality and probable lack of isochronism of the observations of that day. His study as well as that of most of his contemporaries can, in retrospect, be seen to suffer heavily from confusion as to the usage of the term "storm." Workers at that time were quite indiscriminately lumping, under that one heading, phenomena of extratropical and tropical cyclones, thunderstorms, tornadoes, waterspouts, and even dust devils. At that event, Espy's vigorous defense of the centripetal hypothesis has obscured the merits of his thermodynamic work; and though his nineteenth-century successors did not wholly lose sight of his contributions [see Hildebrandson and de Bort's remarks quoted by Shaw (1926, p. 301)], much of his work appears subsequently to have been forgotten.

3. Background of Espy's thermodynamic work

In the early development of any field, quite simple principles may captivate the imagination of a worker. Espy makes clear in his prefatory note that much of his endeavor stemmed from strong conviction that Dalton's work on the laws of vapors was destined to unlock doors to many meteorological mysteries. Dalton's six-point apparatus (in tumbling container containing chaff) later is gloriously referred to by Espy at the very outset of Philoophy of Storms (p. iii) as "the lever with which the meteorologist was to move the world." Espy first read Dalton in 1826 and thereafter began collecting dew point data and working on problems involving atmospheric vapor.

At that time the known fact that water vapor density is less than that of air had given rise to the interesting (and not implausible) notion that after condensation (removal of a lighter component) air became heavier, an hypothesis that rendered cloud-growth rather mysterious. A new level of understanding came when it occurred to Espy to "calculate the effect which the evolution of the latent caloric produces during the formation of a cloud." In Espy's words, "The result was an instantaneous transition from darkness to light." His initial estimates, based on such thermodynamic data as then existed, convinced him of what has since become a basic principle of meteorology, namely that heat released in condensation must exert a strong tendency to accelerate cloud-growth by thermally reducing cloud density. The step from this recognition to the realization that here was a new basis for understanding the way in which large-scale storms maintained themselves despite radial inflow was a further step that Espy quickly took and that sent him off on many years of zealous defense of the inaccurate centripetal theory of storm circulation.

The conceptions of lasting importance we owe to Espy are thus: 1) that condensation has a net effect of lowering, not raising, cloud density, and 2) that the resulting increase of buoyancy will enhance convection and hence lead to still more condensation and convection.

That these were truly original conceptions with Espy seems incontrovertibly shown by the fact that he discussed his ideas before many American, British, and French scientific audiences, yet was never challenged on any significant grounds of priority. Rather, his claims and methods seem
to have been generally quite well received, with the chief exception that what Espy came to regard as central, his wind-flow conception, was widely disputed. Espy does record a claim made by H. Meikle of Edinburgh to the effect that the latter had advanced similar ideas in 1839, but Espy's quotation from Meikle makes clear that Meikle had really noted only that exponential cooling brings about condensation. Espy stresses that he laid no claim to any originality on that particular ground, adding that exponential cooling is "a principle long familiar to the scientific world... which I used in my earlier writings as belonging to the great storehouse of science."

The latter admission is interesting because we find Hunt in 1874 (McCormick, 1963) still having to blame his basic error to the view that clouds form only as a result of exponential cooling (rather than by blowing over cold mountains, for example), a measure of how long it takes to correct well entrenched misconceptions. From the absence of any other priority claim than the irrelevant one of Meikle's, we may safely conclude here that no one before Espy correctly recognized the role of latent heat in cloud thermodynamics and in storm dynamics.

In the 1830's, Espy had available enough thermodynamic data to make rough but quantitative estimates of the consequences of latent heat release. The 1812 experiments of Berard and Delacroix gave the specific heat of air at constant pressure as about 0.25, referred to water, too high by about 5 per cent. The data available to Espy on latent heats was tolerably accurate at 32°F, he used, for the latent heat of condensation, L, a value of about 1200 Btu (in his notation, "1200 Fahrenheits," because the unit of heat, or rather of calories, was then expressed as the number of degrees one pound of water would tend to raise due to addition of any given quantity of calorics); and for the latent heat of fusion he had a value of about 140 Btu.1 (Corresponding modern values are 1055 and 144 Btu, respectively.) The average of a series of measurements of latent heat of fusion is rather accurately known, as several of Espy's computations reveal. The vapor pressure data of Dalton and Gay-Lussac were at Espy's disposal, as were also the dew-point data of Daniell and of Agassiz, although the long-footed Regnault vapor pressure data were still four years in the future when Espy wrote in 1841. There was also available by 1841 a quite accurate estimate of the average mid-latitude lapse rate, which in 1841 was 0.97°F per 100 yd, derived chiefly from mountain observations but already roughly checked by early balloonists.

4. Theoretical calculations

Because of the lack of a systematic basis for his several thermodynamic calculations, Espy's computational results are somewhat difficult to describe in brief. A few examples should suffice to indicate the basic soundness of much of his approach. He was first interested in establishing a basis for predicting the height of cloud bases. From existing dew-point data he deduced the proper way to relate this height to what we would now term the dew-point depression. In his words, his "space for all of the heat being distributed forming the cold of diminished pressure from upwaving columns of air, will be about as many hundred yards high as the dew point in degrees is below the temperature of the air at that time."

Where he predicted 100 yards, we now have, as the correct value, 75 yards. Analysis of his calculations shows that his error lay almost wholly in his inadequate basis for anticipating the correct value of the dry adiabatic cooling rate, which he took to be 2°F per 100 yd, in contrast to Storer's value in the correct value of about 1.6°F per 100 yd. His estimate was based on his nebescopic measurements, the inaccuracies of which will be noted below. He was aware that this experimental measure might be too low and at one place he entertains the possibility that the figure might be 1 to 2 degrees per hundred yards, nicely bracketing the correct value.

Another illustration of the type and surprising accuracy of the computations of 1828 is the following: He computed the difference in final temperature attained in given ascent by dry and saturated ascending air, and compared each with the environmental air temperature. For air initially saturated until 50°F and then exposed to a temperature 50°F warmer than that which would be attained if no vapor were condensed. The correct value is about 44°F, which is fairly impressive agreement (chiefly limited by inadequacies in Espy's French pressure and temperature data). When he essayed to estimate cloud-to-environment temperature differences, he again had to use his too-low-dry-adiabatic rate as a reference, and hence underestimated too, since he was unaware of environmental effects. The estimates of excess of cloud-top temperatures over environmental temperatures came out as very gross overestimates, in some examples implying cloud temperatures as much as 50°F above ambient. This was the only instance in which Espy's cloud thermodynamics was grossly in error, and we see that it was basically ignorance of cloud dynamics that permitted him to accept such large temperature excesses.

From these and other calculations Espy went on to predict that latent heat release should "expand the air about 8000 times the bulk of the water generated; that is, 8000 cubic feet for every cubic foot of water formed out of the condensed vapor."

5. The nebescopic experiments

Air pumps were commonplace by Espy's time, and the fact that a cloud often forms in the re-
u-tube manometer at c was used to observe pressure differentials between chamber and environment. For dry-air experiments Espy employed calcium chloride as a desiccant. For his experiments on in-cloud cooling rates, he placed water in the bottom of the chamber.

The technique of using the nephoscope is best described with the aid of the diagram of Fig. 3 where isotherms of temperature T and adiabats are plotted on coordinates of pressure p and specific volume v. (Needless to say, no such clear-cut graphical interpretation was extant in 1841.) Air at room temperature T<sub>0</sub> and room pressure p<sub>0</sub> was pumped into the nephoscope. If done infinitely slowly, the compression path involved would be the T<sub>0</sub>-isotherm. In practice, the pumping was done quickly, adiabatically heating the air above T<sub>0</sub>, but by waiting sufficient time after pumping, the nephoscope and contained air sample cooled back to T<sub>0</sub>, attaining some pressure p<sub>i</sub>, that state being depicted by point A in the figure, the corresponding chamber pressure being read from the manometer.

Let us suppose the chamber to have dry air, with no pool of water in the bottom, i.e., consider an experiment to study effects accompanying the dry adiabatic cooling rate. On opening the hand-valve, chamber pressure rapidly fell from p<sub>i</sub> back to the room value p<sub>0</sub>, the expansion ideally cooling the enclosed air along the dry adiabat γ<sub>D</sub> through state B. By closing the hand-valve again just as soon as the manometer indicated equilibrium with room pressure, the system was momentarily trapped in state B. However, the experiment was not concluded until the chamber and enclosed air had been warmed isothermally from T<sub>0</sub> to room temperature T<sub>0</sub>, taking the air to state B', and raising the manometer reading to p<sub>B'</sub>. In a saturated adiabatic run, expansion took the system from state A (following pump-up and thermal equilibrium) down (again ideally) along the saturated adiabatic path γ<sub>E</sub> to state C. Closure of the valve and thermal equilibrium then brought the system to state A'.

Espy took as his indirect measure of the magnitude of the dry and the saturated cooling rates the two pressure differences, p<sub>B'</sub> − p<sub>B</sub> and p<sub>B</sub> − p<sub>A</sub>. One sees immediately that this is not the proper measure, but it still a measure whose magnitude would shed at least useful light on the comparative magnitudes of the cooling rates. Briefly, for lack of a well-developed rationale of thermodynamics, Espy left the precise question to ask in his experiments; but his intuition at least led him to a usable answer. In addition to using the nephoscope, Espy did related experiments in which an initial pressure elevation was produced by raising a closed vessel, fitted with a stopcock and manometer, to some temperature above room temperature and, after allowing it to come to full thermal equilibrium, opening the stopcock to allow an exponential pressure decay to room temperature and then reading the manometer, followed by the same isothermic return to room temperature.

Unfortunately for his subsequent computations, in neither device did Espy seem aware of what we would today designate "error," resulting from heat exchange between the air sample and the walls of the vessel. I have checked several representative sets of data from Espy's tables, using Poisson's equation and the equation of state to compute the heat-error of the nephoscope. It appears to have been about 15 per cent in terms of Espy's measured pressure-differences. That is, the actual path from A to B with dry air departed from the γ<sub>D</sub> path, ending somewhat to the right of B, thus yielding a pressure increment p<sub>B</sub> − p<sub>B'</sub> only about 85 per cent of the ideal value. I have also examined the degree to which his wet experiments reproduced ideal saturated adiabatic conditions, and find indications that his charges of air were less than fully saturated at start of the expansion step; perhaps Espy underestimated the time required for diffusion to bring the system to saturation with only a pool of water in the base of the chamber. The untested non-adiabaticity error led Espy to underestimate the dry adiabatic cooling rate, and hence to make other derived errors already mentioned.

From various experiments carried out at widely varying temperatures and initial pressure elevations, Esphy deduced two quantities which he used in his theories of cloud and storm thermodynamics: First, he derived his estimate of a dry adiabatic cooling rate of 1.25°F per 100 yd by calculations combining the nephoscope results with the barometric formula and with gas-laws estimates of T<sub>0</sub>, a temperature he was evidently unable to measure directly, possibly for thermometer-lag reasons. Secondly, he concluded that the saturated adiabatic cooling rate (in our terms) was only about four-tenths to five-tenths as large as the dry adiabatic rate—(very close to the correc value at 1000 mb, 20°C of 0.43 γ<sub>E</sub>). Clearly, both deductions were somewhat in error; yet both were close enough to lead Espy to correct conclusions concerning the importance of latent heat release for cloud dynamics.

In an earlier paper (McDonald, 1963), I have suggested that we must regard Thomson's 1862 work as having provided the earliest quantitative estimates of the dry and the saturated adiabatic cooling rates. From the present study of Esphy's work it has become clear that the historical picture must be revised to the following: Espy made the first quantitative estimates of γ<sub>D</sub> and γ<sub>E</sub>, based on experimental data; Thomson gave the first qualitative estimates of γ<sub>D</sub> and γ<sub>E</sub>, based on theoretical arguments. My earlier study of Thomson's work leads me to believe that Thomson was unaware of Esphy's work, even though Esphy's qualitative ideas on latent heat effects were accepted by many meteorologists by that time. Joule might have been aware of Esphy's 1840 memoir presented to the British Association, but even if he were, this would not significantly alter the originality of the analysis. Thomson gave the basis of suggestions from Joule.

6. Other contributions

Wheras Esphy's defense of the centripetal hypothesis of storm circulation was erroneous and was fairly well discounted within one or two decades after publication of "Phyllosophy of Storms," his thermodynamic ideas and his recognition of the dynamical role of latent heat release were essentially correct and were generally accepted within the latter half of the nineteenth century. Although Mohr returned some thirty years later to the older notion that convection was sustained by a "partial vacuum" aloft, created by condensation and radiance, Esphy's having adequately explained the matter. We find such writers as Rye and Hildebrandson acknowledged Esphy for his recognition of the function of latent heat in cloud and storm dynamics. That Esphy's thermodynamic work has subse-
His own efforts to gather storm data to test the centripetal hypothesis made him keenly aware of the need for a synoptic net, and one wonders in reading his pleas, if we do not owe a large debt to Essey for the ultimate establishment of a meteorological society. He was especially eager to encourage observers to carry out humidity observations, and gave detailed instructions as to how to construct dew-point devices and wet-bulb thermometers from simple materials. His own humidity observations were evidently very systematic and led him to recognize a meteorological truism still poorly understood by most laymen, namely that even in the midst of periods of severe drought, the atmosphere overhead is usually well-charged with water vapor. Essey clearly realized that what was lacking was not moisture but vertical motion, and it was in hopes of filling the latter need that he developed an interest in artificial stimulation of convection through controlled burning.

7. Summary

We may attribute to Essey the earliest quantitative estimates of both the dry and the adiabatic cooling rates, based on experimental data obtained from the prototype of all subsequent expansion chambers. He was the first meteorologist to sense the fundamentally important role of latent heat release in cloud dynamics and in the energy budget of storms. He gave the first adequate explanation of how it might be possible that the central barometric depression of a storm could be sustained despite steady convergence at low levels, namely through high-level outflow accompanying the latent heat release within the clouds of the storm. He formulated a cloud-base-height formula correct in principle (and in error only because Essey worked at a time when the nature of adiabatic processes was still too imperfectly understood to permit him to suspect the non-adiabaticity error of his nepheloscope). He accounted for the diurnal variability of winds and the diurnal variability of convective cloud heights, gave an incisive refutation of the Huttonian theory that precipitation forms as a consequence of mixing of warm and cold air masses of widely different temperature, and was one of the first meteorologists to elucidate the role of latent heat processes in foehn warming. His cloud and storm model, plus his extensive dew-point observations carried out in times of drought as well as in moister periods, led him to postulate that artificial rains might be stimulated by controlled burning. Finally, all of his work led him to plead for early establishment of a government-supported network of meteorological instruments for the benefit of shipping and agriculture.

Essey was an enthusiastic observer, a scientist who appreciated the need for both theoretical calculation and experimental and observational studies, and one who sought to push his interests in directions best suited to aid the mariner and the farmer. Although he cannot be counted one of the most eminent of early meteorologists, he made many contributions of lasting value. Working at a time when it was necessary to break through uncharted thickets of ignorance and error to come upon even simple scientific truths, Essey is to be remembered as one of the pioneers of nineteenth-century meteorology and of cloud thermodynamics, in particular.

REFERENCES


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Expanded Program in Environmental Science

Drexel Institute of Technology, Philadelphia, Pa., this summer began a comprehensive educational and research program in Environmental Engineering and Science to deal with environmental hazards in urban industrial centers. Director of the project is Prof. Francis K. Davis, Jr., head of Drexel's Department of Physics.

The program, financed in part by a $42,000 annual grant by the U.S. Public Health Service, involves close cooperation with public agencies at the federal, state, and local levels. The associate director, who is devoting full time to the undertaking, was formerly director of the Division of Environmental Health, Community Health Services, Philadelphia Department of Public Health. In developing the program, he worked closely with the U.S. Department of Health, Education, and Welfare.

Dr. Davis stated that curricula in air resources, water resources, radiological health, and land resources leading to the Master's degree will be offered in successive academic years until a comprehensive instructional program with related research activities is brought under the administrative direction of an Institute of Environmental Engineering and Science. "The developmental activities of the program will require an interdisciplinary approach to the complex engineering and science problems which affect our everyday life. Worried people must deal effectively over the next few decades if the world is to be a better place in which to live; or, indeed, the world is to remain even as good a place to live as it is today," he said.

Third TIROS Ground Station in Operation

A third command and data acquisition (CDA) station began operation in mid-September at Fairbanks, Alaska. Color television pictures and data received by this station from the orbiting satellites and sent to the Weather Bureau's National Weather Satellite Center at Wallops Island, Va., are in addition to those furnished by the CDA stations at Wallops Island, Va., and Point Mugu, Calif.

Besides providing additional reception and back-up capability for the two current satellites, TIROS VI and VII, the Fairbanks station furnishes operational experience at a remote site which will be useful in later programs. For polar-orbiting meteorological satellites the new station will be the primary CDA facility.

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