James Espy and the Beginnings of Cloud Thermodynamics

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ABSTRACT

The work of the nineteenth-century American meteorologist, James P. 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 release 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 have 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 Reye in Switzerland.

The present discussion will be devoted to the related contributions of a still earlier worker, the American meteorologist, James Pollard Espy.[1] Although Espy did not present a quantitative theoretical formulation of cloud thermodynamical processes as Kelvin and Reye did some twenty-five years later, we shall see that Espy had, prior to 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 "nephelescope" (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 nephelescope, 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 *Philosophy 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

[1] Shaw (1926, p. 136) presents the only biographical sketch of Espy that I have found. Born in Pennsylvania in 1785, Espy became a student at Transylvania University, in Lexington, Ky., at age 18, and after taking his degree taught school and studied law. Although he completed the law studies, his love of teaching led him in 1817 to take a position as a teacher in the classical department of the Franklin Institute in Philadelphia. Impressed by the meteorological writings of Dalton and Daniell, he began to observe humidity phenomena and to experiment on related matters. His enthusiasm became so strong that he gave up teaching in order to carry on these researches. Espy died in Cincinnati in 1860.

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 high-level divergence 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 rarefied air" (Shaw, 1926, 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, phenomenology of extratropical and tropical cyclones, thunderstorms, tornadoes, waterspouts, and even dust devils. At any 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 workers. Espy makes clear in his preface 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 dew-point apparatus (tin tumbler containing chilled water) is glowingly referred to by Espy at the very outset of *Philosophy of Storms* (p. iii) as "the lever with which the meteorologist was to move the world." Espy first read Dalton in 1828 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 latent 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 the 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 expansional cooling brings about condensation. Espy stresses that he laid no claim to any originality on that particular ground, adding that expansional cooling "is a principle long familiar to the scientific world . . . which I used in my early writings as belonging to the great storehouse of science." The latter assertion is interesting because we find Hann in 1874 (McDonald, 1963) still having to take pains to stress the view that clouds form only as a result of expansional cooling (rather than by blowing over cold mountain peaks, 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 had 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 Delaroche gave the specific heat of air at constant pressure as about 0.25, referred to water, too high by only about 5 per cent. The data available to Espy on latent heats was tolerably accurate: at 32F, he used, for the latent heat of condensation, L, a value of about 1200 BTU (in his notation, "1200° Fahrenheit," because the unit of heat, or rather of *caloric,* was then expressed as the number of degrees one pound of water would tend to rise due to addition of any given quantity of caloric); and for the latent heat of fusion he had a value of about 140 BTU.[2] (Corresponding modern values are 1075 and 144 BTU, respectively.) The temperature dependence of L was already 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 Apjohn, although the long-famous 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 Espy's customary units was 1F per 100 yards, derived chiefly from mountain observations but already roughly checked by early balloonists.

What Espy and contemporaries most lacked was not rough estimates of basic thermodynamic data, but rather the conceptual framework on which to build sound analyses. The caloric theory of heat was still accepted, despite Rumford's cannon-boring experiments forty years before; even William Thomson preferred that theory until about 1850 (Mendoza, 1961). Joule's systematic experiments laying the foundation for the first law of thermodynamics were barely begun at the time Espy published his *Philosophy of Storms.* Carnot's 1824 publication was still almost unknown for the significance it was ultimately recognized to hold. Even such a relevant theoretical formulation as that which we now term Poisson's law (which Espy could have used to very real advantage in his theorizing and in interpreting his nephelescope observations) was not yet part of the thinking of meteorologists. We must note that the principle of adiabatic cooling was only given quantitative formulation in about 1820 by Laplace (in correcting the error in Newton's formula for the velocity of sound) and, although rederived on slightly different grounds by Poisson in 1823 (Mendoza, 1961), was not widely published until appearance of a treatise by Poisson in 1834. That Espy was not alone in his ignorance of Poisson's relation is clearly revealed by the fact that when he presented a memoir on his work to the French Academy of Science, three eminent French physicists and meteorologists, Babinet, Arago, and Pouillet, prepared a detailed and laudatory official commentary on Espy's memoir, yet not one of those three realized the crucial bearing that their countryman's work had for the problem to which Espy addressed himself. Clearly, this was a time when conceptual analysis of heat phenomena was still in a state of great confusion, not to be cleared until another ten years elapsed and Clausius and Thomson brought order into the chaos of the study of heat and thermodynamics about 1850.

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

[2] Espy uses the terms then current, "latent caloric of elasticity" and "latent caloric of fluidity," for these two quantities.

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 result ran as follows: "The bases of all clouds forming by the cold of diminished pressure from upmoving 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 about 1.25F per 100 yd, in contrast to the correct value of about 1.65F per 100 yd. His estimate was based on his nephelescope 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 1.5 to 2 degrees per hundred yards, nicely bracketing the correct value.

Another illustration of the type and surprising accuracy of computations Espy made 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 at 80F at sea level, he predicted a temperature 50F warmer than that which would be attained if no vapor were condensed. The correct value is about 44F, which is fairly impressive agreement (chiefly limited by inadequacies in Espy's high-temperature vapor pressure 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 there, too. Since he was unaware of entrainment effects, his 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 50F 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." From this, he then subtracted his estimate of 1300 cubic feet occupied by the vapor which condensed to form the cubic foot of liquid water, leaving a figure of 6700 cubic feet of net expansion per cubic foot of condensate produced. (Note here that earlier conceptions made allowance only for the volume-reduction, thus predicting an *increase* of cloud density upon condensation because of omission of any latent heat effect.) Where Espy got 8000 cubic feet above, the modern estimate is about 7200, which is close; but Espy's allowance for the volume diminution due directly to vapor condensation can be seen to be faulty. At any event, from such figures, plus others making allowance for possible release of the latent heat of fusion if ice and snow were formed, Espy sought (successfully) to convince his contemporaries that cloudy convection produced its own self-renewing energy source. In Fig. 1, a reproduction of one of the woodcuts in Espy's book, we have an indication of the flow pattern which he inferred, his unique kinematical contribution being the idea of the divergent flow aloft and his unique dynamical contribution being the idea that the latter resulted from the thermal expansion accompanying latent heat release. Although the cut shows this idea applied only to individual convective clouds, Espy believed that extensive storm systems must derive energy in the same way and that this was how they could exist for several days in the face of the kind of steady influx of air at low altitudes which he envisaged. Altogether, one sees that Espy was very much on the right track, despite error on what he put forth as his main thesis, the radial wind flow at the surface.

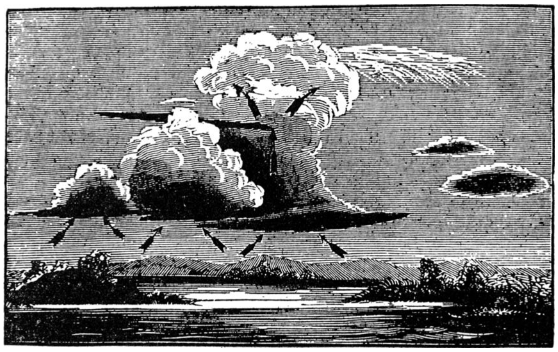

FIG. 1. Woodcut from Espy's *Philosophy of Storms* (1841), showing high-level divergence driven by latent heat release.

5. The nephelescope experiments

Air pumps were commonplace by Espy's time, and the fact that a cloud often forms in the receiver of an air pump on extracting moist air was expressly cited by Espy as a long-familiar point. When Espy set about trying to measure the dry and the moist adiabatic cooling rates (to use terms quite foreign to the vocabulary of that day), he might have contented himself with observations made in that fashion. However, he devised a rather more convenient method making clever use of certain elementary ideas. The device he constructed to carry out that method is the forerunner of all present-day pump-up-and-blow-down expansion chambers (*e.g.,* Wilson cloud chambers). This device for observing clouds, the "nephelescope," is shown in Fig. 2 in a reproduction of the original woodcut from Espy's book. In the cut, *a* is a hand-pump for forcing air into the glass chamber, *b,* a hand-valve being provided to close the chamber after pumping. A mercury 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 nephelescope 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_0 and room pressure p_0 was pumped into the nephelescope. If done infinitely slowly, the compression path involved would be the T_0-isotherm. In practice, the pumping was done quickly, adiabatically heating the air above T_0, but by waiting sufficient time after pumping, the nephelescope and contained air sample cooled back to T_0, attaining some pressure p_1, 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_1 back to the room value p_0, the expansion ideally cooling the enclosed air along the dry adiabat γ_d through A, terminating in 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 slowly warmed isosterically from T_B to room temperature T_0, taking the air to state B', and raising the manometer reading to

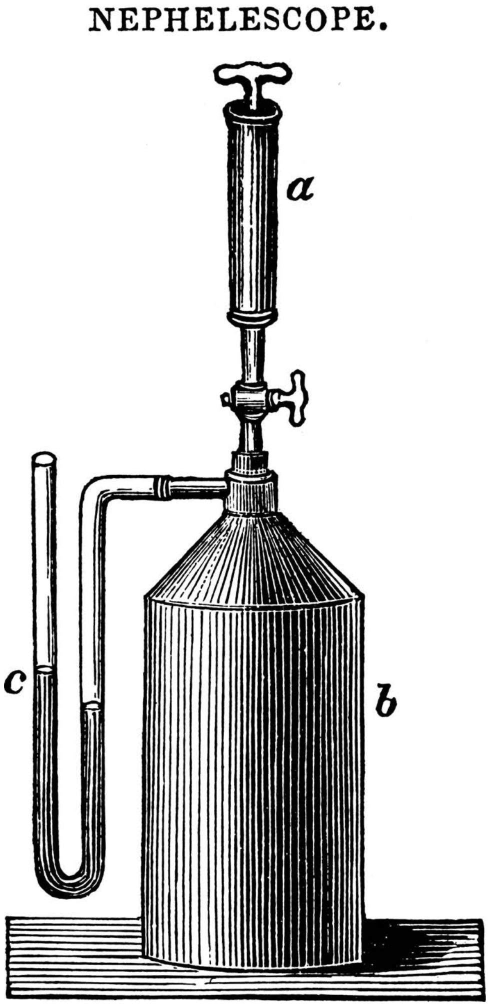

FIG. 2. Woodcut of Espy's nephelescope.

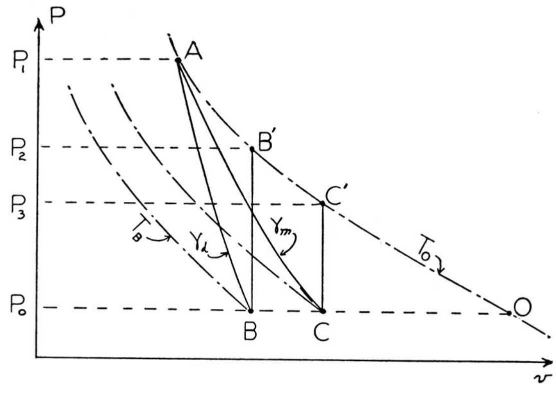

FIG. 3. Schematic diagram of operating process of nephelescope.

p_2. 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_m to state C. Closure of the valve and thermal equilibration then brought the system to state C' and pressure p_3.

Espy took as his indirect measure of the magnitude of the dry and the saturated cooling rates the two pressure differences, $p_2 - p_0$ and $p_3 - p_0$. One sees immediately that this is a) not the proper measure, but b) 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 did not seem to know the precise question to ask in his experiments; but his intuition at least led him to a usable answer. In addition to using the nephelescope, 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 well above room temperature and, after allowing it to come to full thermal equilibrium, opening the stopcock to allow an expansional pressure drop to room pressure as in the nephelescope, followed by the same isosteric return to room temperature.

Unfortunately for his subsequent computations, in neither device did Espy seem aware of what we would today term a "non-adiabaticity 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 estimate the non-adiabaticity error of the nephelescope. 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_d path, ending somewhat to the right of B, thus yielding a pressure increment $p_2 - p_0$ 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 undetected 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, Espy 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 1.25F per 100 yd by calculations combining the nephelescope results with the barometric formula and with gas-law estimates of T_B, a temperature he was evidently unable to 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 great as the dry adiabatic rate—(*versus* a correct value at 1000 mb, 20C of 0.43 γ_d). 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 Espy's work it has become clear that the historical picture must be revised to the following: Espy made the first quantitative estimates of γ_d and γ_m, based on *experimental* data; Thomson gave the first quantitative estimates of γ_d and γ_m, based on *theoretical* arguments. My earlier study of Thomson's work leads me to believe that Thomson was unaware of Espy's work, even though Espy's qualitative ideas on latent heat effects were accepted by many meteorologists by that time. Joule might have been aware of Espy'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 on the basis of suggestions from Joule.

6. Other contributions

Whereas Espy's defense of the centripetal hypothesis of storm circulation was erroneous and was fairly well discounted within one or two decades after publication of *Philosophy 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, Hann (1874) acknowledges Espy as having adequately explained the matter. We find such writers as Reye and Hildebrandsson acknowledging Espy for his recognition of the function of latent heat in cloud and storm dynamics. That Espy's thermodynamic work has subsequently been largely forgotten is understandable inasmuch as the later work of Thomson, Reye, Peslin, Hertz, and von Bezold put the whole matter into so much more analytical form, permitting so much broader conclusions to be drawn, that Espy's only partly quantitative work became lost in the growing literature of meteorological thermodynamics. But viewed in historical perspective, it seems necessary to acknowledge Espy as a pioneer in cloud thermodynamics and one who added significant insights to mid-nineteenth century meteorology.

Espy deserves credit for certain other contributions. He was one of the first to suggest an adequate explanation of the diurnal variation of winds near the earth's surface (Davis, 1894, p. 133). He observed and correctly explained on theoretical grounds the diurnal variation in convective cloud-base elevations, using his dew-point-depression relation as the basis for his discussion, just as one might today. He and his contemporary, the German meteorologist Dove, offered the first satisfactory explanation of foehn winds and their relation to latent heat release and precipitation on the windward side of a mountain. He was an enthusiastic proponent of the notion, recently revived on a more controlled research basis by Dessens in France, that artificial cloud-formation could be induced by burning woodland areas, and in his *Philosophy of Storms* assembles much observational material on fire cumulus and fire storms. The most valuable outcome of this interest lay in the emphasis thereby placed on the dynamics of convection and on its origin in surface-heating. There is no evidence that Espy actually carried out any of his projected burning experiments.

That Espy was a very perceptive critic of current misconceptions in meteorology is well illustrated in his debates with a Professor Olmsted of Yale College, one of Espy's most vocal opponents. The chapter in *Philosophy of Storms* dealing with his several exchanges with Olmsted makes delightful reading, and shows Espy to be by far the more astute of the two. Especially interesting is Espy's refutation of the Huttonian theory of precipitation, towards which Olmsted had leanings, and which was still not fully scotched in Hann's time (McDonald, 1963) despite the fact that anyone who considered Espy's excellent discussion of the defects in that theory should have been completely convinced of its absurdity.

In his role as a meteorologist in the Franklin Institute, Espy devoted much energy towards setting up a network of meteorological observers. 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 Espy for the ultimate establishment of a meteorological observing program in this country.[3] 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 drouth, the atmosphere overhead is usually well-charged with water vapor. Espy 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 Espy 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. We must also recognize him as 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

[3] Espy's 1836 report for the Joint Committee of the American Philosophical Society and the Franklin Institute of the State of Pennsylvania, reads so much like a plea for establishing the Cooperative Observer program of the U. S. Weather Bureau, that it deserves quotation in full. "We would suggest, as the most effectual and perhaps the only means of obtaining this end, an appropriation by the government for the purchase of meteorological instruments, to be presented to those academies, schools, and colleges, that would pledge themselves to keep a journal of the weather, according to a prescribed plan, for five years; and send a monthly statement to a meteorologist, to be appointed by the government. If instruments were thus furnished for one hundred observers, it is altogether probable that two hundred more would volunteer their services, knowing that their labors would be one hundred fold more valuable in combination with others than they had hitherto been. With three hundred observers, properly located, no storm could spring up within, or enter, the United States, without being constantly under the eye of at least two observers. And thus its extent, its progress, and the direction of the wind in its borders, would be fully known." Perhaps a certain measure of naive optimism has always been essential in meteorology.

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 Espy 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 nephelescope). 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 moist 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 drouth 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 observations for the benefit of shipping and agriculture.

Espy 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 contributions of lasting value. Working at a time when it was necessary to break through tangled thickets of ignorance and error to come upon even simple scientific truths, Espy is to be remembered as one of the pioneers of nineteenth-century meteorology and of cloud thermodynamics, in particular.

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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, Drexel 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 introduced 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—the problems with which we must deal effectively over the next few decades if the world is to be a better place in which to live; or, indeed, if 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. Cloud-cover pictures and other data received by this station from the orbiting satellites and sent to the Weather Bureau's National Weather Satellite Center at Suitland, Md., 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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