Hadley's Principle:
Part 3 – Hadley and the British

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> *Students of meteorology should rebel against a theory that is so incapable of bearing a reasonable test.*
> M. W. Davis (1899).

Résumé

It was thanks to the influential German meteorologist Heinrich W. Dove (1804–1879) that the English meteorologist George Hadley was credited with the discovery that the Earth's rotation has a fundamental effect on the atmospheric flow. In the second half of the nineteenth century, however, theoretical meteorologists, in particular the American William Ferrel, the Norwegian Henrik Mohn and the German Adolph Sprung, began to question the validity of Hadley's explanation. But, as it would turn out, the most devastating criticism would come from Hadley's own countrymen.

Meteorology in nineteenth-century Britain

Throughout most of the nineteenth century there was no country the British admired more than Germany. This was also true for meteorological science, and the leading German meteorologist Heinrich W. Dove´s ideas about the clockwise veering of the wind were particularly well received. The renowned English scientist Francis Galton (cousin of Charles Darwin) expressed admiration for Dove's 'well known theory', which he considered 'so fertile in result' (Galton, 1863). Robert H. Scott (1833–1916) translated Dove's *Das Gesetz der Stürme* (Dove, 1862). Scott had worked with Dove in Germany and would later be the longest-serving Director of the UK Meteorological Office from 1867 to1900 (Burton, 1994). It was thanks to Dove that Hadley's Principle finally became known in Hadley's own country.

At this time, there was no meteorologist in Britain with any interest or qualification in theoretical or dynamical meteorology. The leading authority regarding the motions of the atmosphere was the astronomer John Herschel, son of the famous astronomer William Herschel (Good, 2006). Amongst his theoretical articles and books about astronomy and physical geography was a long article on meteorology for the *Encyclopaedia Britannica*. According to Herschel, Hadley's model afforded 'an easy and satisfactory explanation' of the Trade Winds (Herschel, 1853).

Excessive winds

Herschel had to admit that there was one problem with the Hadley Principle. Although it fairly well explained the direction of the Trade Winds, it predicted quite unrealistic values of their velocities. Air moving at rest from the Tropic of Cancer was predicted to acquire a westward velocity of 37m s^{-1} at the Equator, a fact already noted by Hadley (Figure 1). In the mid-latitudes a wind moving meridionally would increase its velocity by on average 5m s^{-1} for every latitude degree passed. A wind from Paris blowing to Newcastle would increase to a westerly hurricane of 35m s^{-1}.[1]

The problem of unrealistic wind speeds had been there already with Galileo's seventeenth-century explanation of the Trade Winds. Why did the air, lagging behind the Earth's rotation, do so by only 5–10m s^{-1} at latitudes where the rotational velocity was more than 400m s^{-1}? Whatever one might think of Halley's and d'Alembert's models and their disregard of the Earth's rotation, it salvaged them at least from the problem of excessive winds!

Hadley explained the absence of such extreme winds at the Earth's surface as due to the effects of friction and so did Herschel. No hurricane winds, Herschel assured the readers of *Encyclopaedia Britannica*, would come into being thanks to friction which would make the easterly tendency diminish, to the point that the Trade Wind 'lost its easterly character altogether'. (Herschel, 1853, 1878).[2]

[1] A German meteorologist wanted to test the hypothesis that the 30m s^{-1} warm föhn winds in the Alps had tropical origin and calculated a backward trajectory from Central Europe according to Hadley's Principle. The result showed not only that the föhn air seemed to originate over Indonesia, the wind would also have had an initial velocity of 121m s^{-1} (von Baeyer, 1858; Mousson, 1866).

[2] Herschel thought that at least half, more probably two-thirds, of the energy in the westerlies derived from the energy of the rotation of the earth. He did not realize that if friction between the air and the Earth was that effective the rotation of the Earth would probably have decreased much more rapidly than actually observed.

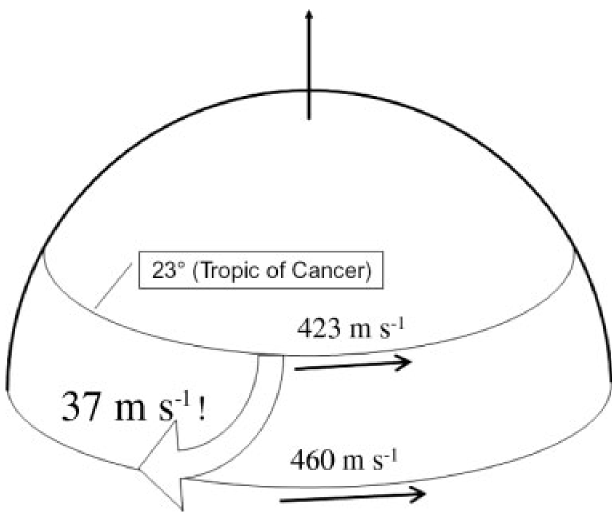

Figure 1. George Hadley was already aware that his explanation of the Trade Winds predicted unrealistically high wind speeds in the tropics. He explained the discrepancy with reality by invoking frictional effects.

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Objections to Hadley's Principle

The frictional explanation did not satisfy everybody. In 1840, a French book about the wind systems on the world's oceans, expressed doubts that the Earth's rotation around its axis really was important for the Trade Winds. In the next edition of the book the author omitted this chapter because *a presentation of facts is sufficient instead of an insufficient explanation*. (Lartigue, 1840, 1855.)

M. F. Maury (1806–1873) was a leading American authority on marine and oceanographic problems and had, in 1853, been instrumental in calling an international meeting in Brussels to coordinate marine traffic. His book *Physical Geography of the Sea* became a bestseller with six editions (Maury, 1855).[3] From initially having endorsed Hadley's Principle, in later editions Maury became more critical and finally reached the verdict that diurnal rotation should not be regarded as the sole cause of the easterly direction of the Trade Winds.

Nor did the frictional explanation satisfy the man in Britain who would most forcefully formulate the criticism against Hadley's Principle. John K. Laughton (1830–1915) had an influential post in the Royal Navy as instructor, teacher and textbook author in astronomy, meteorology and oceanography (Lambert, 1999). In his scientific publications he comes over as a strong empiricist with a deep-rooted scepticism about theories. According to him scientists were often too quick to generalize:

> *There is no more dangerous source of error in physical science than a strong … conviction that certain phenomena must exist and a determination to find them. …Difficult as it is to banish from the mind all preconceived ideas, and to inquire into things as they really are, not as they have been imagined to be, it is only by such a beginning that we can hope to arrive at knowledge and an understanding of the truth.*

In his powerfully argued book, *Physical Geography in its relation to the prevailing winds and currents,* Laughton (1873) dismissed the commonly received theory of the Trade Winds. The standard explanation of the circulation of the global atmosphere was open to *very grave objections*. Rather than a *storm of unheard-of severity, whose fury nothing could withstand* he found the Trade Winds frequently dying away on the Equator. The friction between the air and Earth must be so great, Laughton thought, that *the influence of the rotation of the Earth is not sufficient to produce the effects observed*:

> *It would be contrary to all direct evidence to admit that the rotation of the Earth produces any sensible effect on the motion of the air which we call wind.*

[3] If there ever was a 'Da Vinci Code of Oceanography' it was M. F. Maury's book. Profound observations, like Ekman transports in the Gulf Stream, were mixed with pure fantasies and religious speculations.

Laughton's book was well received by the British meteorological community and led to his lifelong connection with the Royal Meteorological Society. In 1878 he, together with five other prominent British meteorologists, published a collection of articles under the title *Modern Meteorology* where there is no mention of Hadley or the rotation of the Earth (Mann *et al*., 1879).

The anti-Trade Wind proves Hadley right?

At about this time, new observations seemed to give some credence to the strong winds predicted by Hadley's Principle. Observations of cirrus clouds in the subtropics revealed the existence of rapid southwesterly high-level winds of 20–40m s^{-1}, sometimes 60m s^{-1} around 30°. Was this the upper-air return flow from the tropics, the 'Anti-Trade Wind' envisaged by Edmond Halley in 1686? In his paper in 1735, George Hadley had already extended his model to account for air moving towards higher latitudes. Equatorial air at rest at the Equator would at 30° latitude acquire a speed of 62m s^{-1} (Figure 2). This was now confirmed by observations – or so it seemed.

At this time it was also realized that for frictionless inertial motion over the Earth's surface it is the absolute *angular momentum*[4] that is conserved rather than the absolute *velocity* (absolute *linear momentum*). But when this correct principle was applied, the consequences became even more absurd. Parcels of air (or more correctly, hemispheric rings of air) displaced meridionally by an impulse from the Equator would, at 30° latitude, appear to produce a wind of 124m s^{-1}

[4] From about 1800 it was realized that Kepler's angular momentum conserving 'Second Law' was applicable also for motions on the Earth. Meteorological texts, discussing atmospheric dynamics, referred to the 'Laws of Areas' ('Flächensatz' in German, 'Loi des aires' in French) well into the twentieth century.

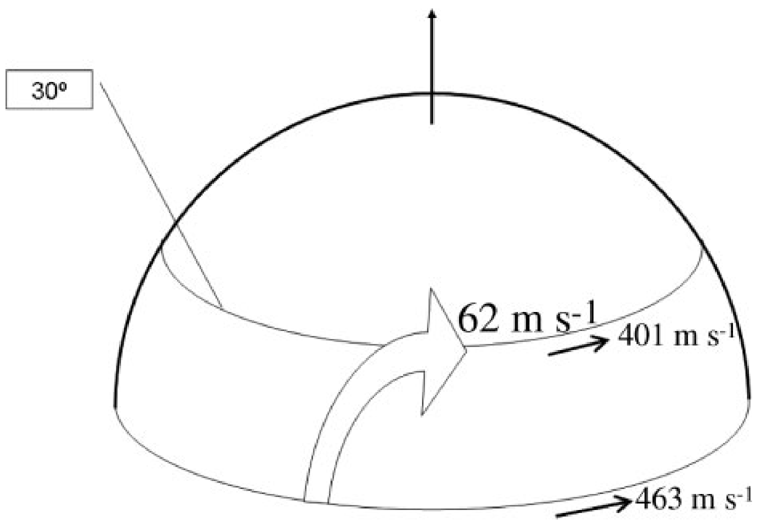

Figure 2. When Hadley's model was applied to upper air motion where friction plays a minor role, the results seemed to be closer to observations of rapidly moving cirrus clouds.

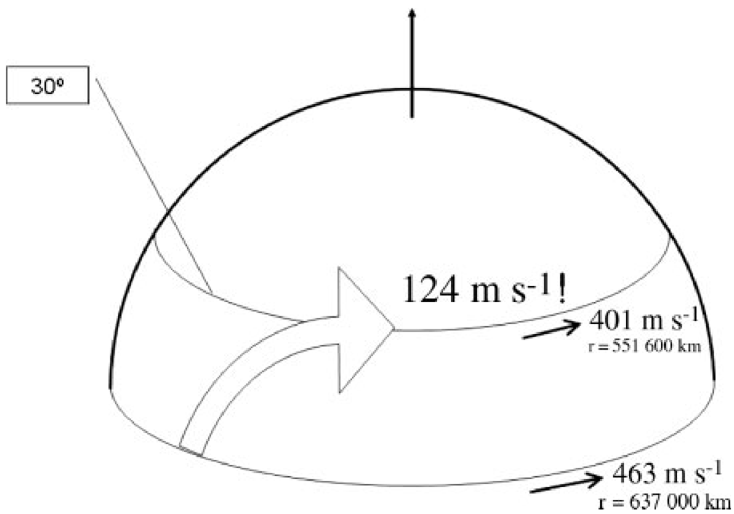

Figure 3. When the correct principle of conservation of angular momentum was applied to Hadley's mechanical model, the results again became highly unrealistic with excessive winds.

in an easterly direction, twice as much as in Hadley's model and what was observed (Figure 3). Winds from Paris would appear as westerly 70m s^{-1} in Newcastle! *So what was wrong?*

The fundamental flaw of Hadley's model

To understand why hurricane winds did not occur we must realize that the most misleading part of Hadley's Principle is *the mechanical model itself*. The assumption about an impulsive force pushing the air in a north–south direction is just not applicable to the atmosphere (or ocean). This was realized by German meteorologists already in the 1870s. Others who understood this were Americans such as Davis (1893), Marvin (1920) and Clough (1920), and in the 1930s this idea reached the influential textbook by David Brunt, British professor in meteorology at Imperial College:

> *It is frequently stated in meteorological treatises that if air moves from one latitude to another, retaining its original angular momentum (in space) about the earth's axis, then in its new latitude it will have enormous velocities along the … circle of latitude. This statement is highly misleading.* (Brunt, 1934, 1944.)

Brunt's explanation was along the lines already made by Ferrel:

> *The effect of the Earth's rotation is to make it difficult for any parcel of air or water to move any considerable distance over its surface.*

A frictionless moving body at the Equator given an impulse to the north of 20m s^{-1} would travel to the north no more than 1000km, equivalent to about 9° of latitude, before it is turned back by the Coriolis force in an inertial circle motion (Figure 4). A parcel of air at latitude 60° given the same 20m s^{-1} impulse northward would only travel 160km (1½ latitude degree) before it was turned back:

> *In practice the motion of a mass of air through a large range of latitude, while retaining its original angular momentum about the axis of the Earth, can never arise.* (Brunt, 1934, 1944)

And the problem with the excessive westerly winds such as 70m s^{-1} (or 124m s^{-1})? In fact there is no increase in speed. The air in Paris (or at the Equator) must be given an *initial* velocity of 70 (or 124m s^{-1}) to be able to even reach Newcastle (or 30° latitude). There it for a short while appears as an excessive westerly wind, before it is turned back in its inertia circle motion.

The angular momentum conserving model is therefore not applicable for *parts* of the atmosphere, only for the *total* atmosphere-earth system (as shown by Hide *et al.,* 1980).

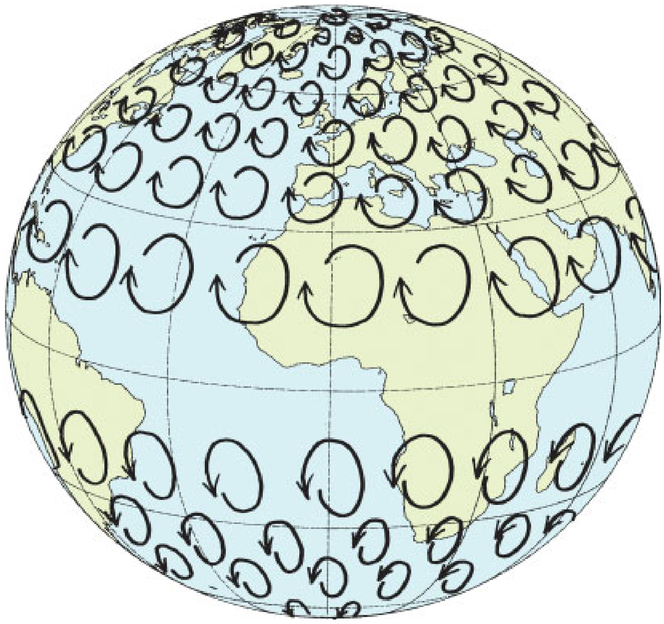

Figure 4. Any particle, moving frictionless and under inertia over the Earth's surface will, while conserving its angular momentum, follow almost circular motions due to the Coriolis Effect. To reach any considerable distance the parcel must have substantial velocity. The 'inertia circles' in the figure correspond to velocities of 50m s^{-1}. Due to the latitudinal variation of Coriolis Effect the trajectories are more curved towards the poles which induces a slight westward drift. The rotation of the Earth therefore has, quite counter-intuitively, not only the effect of constraining the motion of the atmosphere (and oceans), but also of pushing it slightly to the west, against the rotation (the 'Beta Effect').

Continued confusion about the Coriolis Effect

But now we have gone ahead in time. Let us therefore return to the late nineteenth century when British meteorology was left in a bewildered state with respect to the Coriolis Effect. Some physical scientists, mainly *mathematicians* mastered its derivations,[5] but were not particularly interested in its geophysical applications. *Meteorologists* tended to stick to observed empirical facts, because the mathematical derivations were complex and Hadley's Principle anyway made wrong predictions. *Physicists* with no direct involvement in meteorology found Hadley's Principle a handy and intuitive explanatory model, more convenient to refer to than the complicated mathematical derivations.

Among the latter we find James Thomson (1822–1892), elder brother of Lord Kelvin (William Thomson). In his 1892 Bakerian Lecture to the Royal Society on the general circulation of the atmosphere, he mentioned Hadley's name almost 40 times, twice as much as that of William Ferrel:

> *Hadley's theory in its main features […] must be substantially true, and must … form the basis of any tenable theory [of the general circulation of the atmosphere] that could be devised.* (Thomson, 1892.)

[5] Normally trigonometrically formalism was used, although already O'Brien (1851) had made a vector derivation (Persson, 2005).

Thomson seems to have made the British start to appreciate Hadley's Principle out of patriotic reasons, unaware of the fundamental criticism which foreign scientists, first and foremost William Ferrel, had launched against it. It is therefore no coincidence that is was a friend of Ferrel, who would deliver the most fatal criticism of the Hadley Principle, who gave it what should have been its final *coup de grace*.

Call for a rebellion against Hadley's Principle?

William M. Davis (1850–1934), professor in physical geography at Harvard University, was a leading force in the creation of the short-lived *Journal of Meteorology* 1884–1896. In the 1880s, he published several articles on the deflective force which was then summarized in his textbook *Basic Meteorology* (Davis, 1893). In 1899 he was invited to make a speech at the Royal Meteorological Society on the general circulation of the atmosphere. Most of the talk developed into a condemnation of the contemporary scientists who still regarded Hadley's explanation *so sufficient that it is still widely quoted*, although it has been repeatedly shown to be *seriously incomplete*. Davis pointed out, and he was one of the first to do so, that the violent storms didn't need any tremendous friction to abate; the resistance of the pressure field would accomplish that. See also Lorenz (1967).

> *Much more serious is the omission from Hadley's statement ... of all consideration*

of the effect produced on the distribution of pressure by the deflection of the winds…. (Davis, 1899, p 162)

In the same way as horizontal pressure gradients accelerate the wind, they can also decelerate them. The most effective brake on the winds is not friction but the pressure field through the geostrophic adjustment process. If the winds become super-geostrophic the Coriolis force, having an upper-hand relative to the pressure gradient force, would drive the air toward higher pressure and retard it (Persson, 2001, figure 3).

As long as the effect of the winds in modifying the distribution of pressure is left out of consideration, no broad understanding of atmospheric processes can be reached. (Davis, 1899, p 169)

Davis found it *curious* that at a time when the global pressure distribution was known, students who were familiar only with Hadley's explanation of the effect of the Earth's rotation should continue to believe in the conventional theory of the winds. Since this explanation of the circulation of the atmosphere was *seriously incompetent* Davis called for a *rebellion* against unscientific teaching:

If he [the serious student] makes no objection, it must be that he is too accustomed to basing his opinions on authority instead of on evidence. It is utterly unscientific to believe in a theory whose deduced consequences are not borne out by facts; yet what is more common than to find among students of meteorology an acceptance of the conventional origin of the general circulation of the atmosphere…they should rebel against a theory that is so incapable of bearing a reasonable test. If assured that the theory is correct, they should rebel against the insufficiency of the evidence that is presented to them in its favour. (Davis, 1899, p 162)

Davis' speech did not seem to have any great impact except perhaps in the works by Marvin (1920), Clough (1920), and Brunt (1934, 1944). But there never was a 'rebellion' and the consequences can still be seen today, 110 years later, in many meteorological textbooks, both popular and academic. It is not uncommon to see a correct mathematical derivation beside a Hadley inspired explanation that is in conflict both with reality and the mathematics it is supposed to enlighten.

Conclusion

To be a scientist you do not have to be right, the important thing is that you have reached your conclusions by sound reasoning with the concepts and observations available at the time. In that sense, Aristotle and Ptolemy were scientists, but not those who echoed them in the seventeenth century when new facts showed that the Earth was not at the centre of the Universe. On the other hand, others we regard as scientists made fundamental errors: Copernicus kept numerous epicycles in his heliocentric system, Kepler believed in number mystique and Galileo was convinced that the planetary orbits were perfect circles.

Hadley, MacLaurin, Kant, Laplace, de Luc and others have their place in the history of meteorology as scientists who were on the right track by pointing to the rotation of the Earth as a crucial mechanism for the motions of the atmosphere. But what was scientific in the eighteenth century is not scientific today when new observations and theories have shown that they applied too simplistic mechanical principles.

What makes Hadley stand out scientifically in comparison with MacLaurin, Kant, Laplace, de Luc and others, is that his discussion was the most quantitative. He also addressed the problem of excessive wind velocity, whereas the others had only discussed the wind's direction. Any suggestion to rename the Exeter-based centre for climate change research would therefore not have my support!

References

von Baeyer JFW. 1858. Über die Bahnlinien der Winde auf der sphräoïdischen Erdoberfläche. *Ann. Phys.* **104**: 377–404.

Brunt D. 1934 and 1944. *Physical and dynamical meteorology*. Cambridge University Press: Cambridge, pp 392–393 and pp 404–405 respectively.

Burton J. 1994. Pen Portraits of Presidents: Robert Henry Scott, MA, DSc, FRS. *Weather* **49**: 323–324.

Clough HW. 1920. The principle of the conservation of angular momentum as applied to atmospheric motions. *Mon. Wea. Rev*. **48**: 462–465.

Davis WM. 1893. *Elementary Meteorology*. Ginn and Company: Boston.

Davis WM. 1899. The circulation of the atmosphere. *Q. J. R. Meteorol. Soc*. **25**: 160–169. Discussion pp 169–172.

Dove HW. 1862 and 1866. *Law of Storms*. London, Berlin. (Translation of 2nd ed. Dove's *Das Getsetz der Stürme*, Verlag Dietrich Reimer: Berlin.)

Galton F. 1863. A development of the theory of cyclones. *Proc. Roy. Soc.* **12**: 385–386.

Gold E. *Barometric gradient and the wind force, Report to the Director of the Meteorological Office on the Calculation of Wind Velocity from Pressure Distribution*. HM Stationery Office London: London.

Good GA. 2005. A shift of view: meteorology in John Herschel's terrestrial physics in *Intimate universality: local and global themes in the history of weather and climate*, Fleming JR, Jankovic V, Coen DR (eds). Science History Publications: Sagamore Beach, MA, USA.

Herschel J. 1853 and1878. *Outlines of Astronomy*. Longman, Brown, Green, Longmans, and Roberts: London.

Hide R, Birch NT, Morrison LV, Shea DJ, White AA. 1980. Atmospheric angular momentum fluctuations and changes in the length of day. *Nature* **286**: 114–117.

Lambert DA. 1999. Pen Portraits of Presidents, Sir John Knox Laughton, RN. *Weather* **54**: 27–30.

Lartigue J. 1840 and 1855. *Exposition du système des vents*. Impr. Royale: Paris.

Laughton JK. 1873. *Physical geography in its relation to the prevailing winds and currents*. JD Potter: London.

Lorenz E. 1967. *The nature and theory of the general circulation of the atmosphere*. WMO: Geneva pp 69–70.

Mann RJ, Laughton JK, Strachan R, Ley WC, Simons GJ, Scott RH. 1879. *Modern Meteorology, Six lectures under the auspieces of the Meteorological Society*. Edward Stanford: London.

Marvin CF. 1920. The law of the Geoidal Slope and fallacies in Dynamic Meteorology. *Mon. Wea. Rev.* **48**: 565–582.

Maury MF. 1855, 1861 and 1963. *The physical geography of the sea and its meteorology*. Leighly J (ed.) Cambridge University Press: Cambridge.

Mousson A. 1866. Über die Bahnbewegung eines freien Theilchens auf einer sich drehenden Kugel. *Ann. Phys.* **129**: 652–658.

Mühry A. 1869. *Über die Theorie und das allgemeine geographische System der Winde*. Göttingen: Germany.

O'Brien M. 1851. On symbolic forms derived from the conception of the translation of a directed magnitude. *Proc. Roy. Soc*. pp 161–206.

Persson A. 2000. Back to basics: Coriolis: Part 2 – The Coriolis force according to Coriolis. *Weather* **55**: 87.

Persson A. 2001. The Coriolis force and the geostrophic wind (Coriolis part 5). *Weather* **56**: 267–272.

Persson A. 2005. The Coriolis Effect: 400 years of conflict between common sense and mathematics. *Hist. Met.* **2**: 1–24. http://www.meteohistory.org/2005historyofmeteorology2/01persson.pdf

Sprung A. 1879. Über das Hadley'sche Prinzip, Anhang zu Studien über den Wind und seine Beziehungen zum Luftdruck. *Archiv des Deutschen Seewarte*. II Jahrgang 1, p 33.

Thomson J. 1892. On the grand currents of atmospheric circulation. *Phil. Trans. Roy. Soc*. **183A**: 653–684.

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