Hadley's Principle: Understanding and Misunderstanding the Trade Winds

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> Old knowledge will often be rediscovered and presented under new labels, causing much confusion and impeding progress—Tor Bergeron.[1]

Introduction

In May 1735 a fairly unknown Englishman, George Hadley, published a groundbreaking paper, "On the Cause of the General Trade Winds," in the *Philosophical Transactions of the Royal Society*. His path to fame was long and it took 100 years to have his ideas accepted by the scientific community. But today there is a "Hadley Crater" on the moon, the convectively overturning in the tropics is called "The Hadley Cell," and the climatological centre of the UK Meteorological Office "The Hadley Centre."

By profession a lawyer, born in London, George Hadley (1685-1768) had in 1735 just became a member of the Royal Society. He was in charge of the Society's meteorological work which consisted of providing instruments to foreign correspondents and of supervising, collecting and scrutinizing the continental network of meteorological observations[2]. This made him think about the variations in time and geographical location of the surface pressure and its relation to the winds[3]. Already in a paper, possibly written before 1735, Hadley carried out an interesting and far-sighted discussion on the winds, which he found "of so uncertain and variable nature":

> …concerning the Cause of the Trade-Winds, that for the same Cause the Motion of the Air will not be naturally in a great Circle, for any great Space upon the surface of the Earth anywhere, unless in the Equator itself, but in some other Line, and, in general, all Winds, as they come nearer the Equator will become more easterly, and as they recede from it, more and more westerly, unless some other Cause intervene[4].

The opening sentence of Hadley's 1735 paper has become a classic: "I Think the Causes of the General Trade Winds have not been fully explained by any of those who have written on that Subject…" What Hadley had in mind was that all the theories so far did not explain why the trades blow from the north-east and south-east rather than from due east. This might seem to be a minor detail, but was the main point of argument for almost a century.

Views on atmospheric circulation around 1700

The need to map and understand the general circulation of the atmosphere and oceans became an important issue in the 16th century with the increased shipping and the exploration and the marine routes to Asia and the New World.

Early ideas on the trade winds

By 1600 it was known that around 30º latitude the climate was rather dry with weak winds. South of this "torrid zone" were regular north-easterly winds, the Trade Winds (fr. Alizées, germ. Passatwinde) and to the north irregular winds from a westerly direction. This pattern appeared to mirror itself south of the equator with steadily southeast trade wind. When scientists tried to understand the general circulation their discussion centred early on the trade winds. Thanks to their steadiness they were assumed to be the easiest one to explain[5].

Galileo Galilee (1564-1642) saw the trade winds as a consequence of the failure of the earth's gaseous envelop to "keep up" with the speed of the earth's rotation. The earth is rotating fastest at the lowest latitudes, the air and the water are lagging behind and an earthbound observer experiences a westward-directed flow. The eastward directed flow at the mid-latitudes was caused by the opposite mechanism: the earth was rotating slower and the air and water "went ahead." A similar argument was used by Johannes Kepler (1571-1630) to explain the westward motion of the tropical oceans[6]. To both Galileo and Kepler the rotation of the earth not only explained the trade winds, the trade winds were themselves a manifestation of the rotation of the earth.

The British debate about the trade winds

In 1685 a debate about the general circulation of the atmosphere started in the Royal Society in England. Because of its sea faring occupation there was no other country where the wind and weather was more discussed than in England. A Dr Garden enthusiastically promoted Galileo's idea that trade winds "lagged behind" because the air due to the sun's heating became lighter, rose and lost touch with the earth's surface[7]. At about the same, on the other side of the Channel, the French scientist Edme Mariotte (1620-84) also repeated Galileo's hypothesis, but explained the tendency of the wind to approach the equator from the north-east and south-east as a consequence of the sun's varying path between the equinoxes[8].

The renowned astronomer Edmond Halley (1656-1742) now entered the debate and suggested as the main mechanism the diurnal displacement from east to west of the sun's heating in the tropical belt. The deviation of the trade winds from straight east was due to the meridional flow of dense air toward the latitude of maximal radiative heating[9]. In subsequent discussions Robert Hooke (1635-1703) invoked the centrifugal force of the earth's rotation to explain the equatorward component of the trade winds. By this the debate ended, Halley and Hooke became occupied with other matters and no new ideas came forward for almost half a century.

The "Hadley Principle" is born

The novelty of George Hadley's explanation was to take into consideration the diurnal motion of the earth around its axis, rather than the sun's apparent motion due to the earth's rotation:

> For let us suppose the Air in every Part to keep an equal Pace with the Earth in its diurnal Motion; in which case there will be no relative Motion of the Surface of the Earth and Air, and consequently no Wind; then by the Action of the Sun on the parts about the Equator, and the Rarefaction of the Air proceeding there from, let the Air be drawn thither from the N. and S. parts.

The circumference of latitude circles at the equinoxes have an absolute difference of 2083 miles compared to the equatorial circle, to which they relate as 917 to 1000, which indicated the difference in absolute velocity:

> From which it follows, that the Air, as it moves from the Tropics towards the Equator, having a less Velocity than the Parts of the Earth it arrived at, will have a relative Motion contrary to that of the diurnal Motion of the Earth in those Parts, which being combined with the Motion towards the Equator, a N.E. wind be produced on this Side of the Equator, and S.E. on the other…

This is what the German meteorologist Adolph Sprung 1880 would name "Hadley's Principle." By then, 125 years after its publication, it only just started to become widely accepted by the meteorological community.

The disregard of Hadley's work

The main reason for the slow appraisal of "Hadley's Principle" was that frictionless, inertial motion in a rotating system was then (as it is now) difficult to comprehend intuitively. Attempts in the 18th and 19th century to use the atmosphere as a test-bed laboratory was complicated because a lack of observations, but also because of too simplistic applications of mechanical principles.

Another reason why it took so long for Hadley to get personal credit was that he was constantly confused with two other scientists. One was Edmond Halley, the other George's own elder brother, John Hadley (1682-1744), who was famous in his own right for astronomical contributions, among them the "Hadley sextant." The fact that the former had also provided a similar, but less elaborated explanation of the trade-winds contributed further to the confusion.

Unfortunately for George Hadley, seven years before his paper was published, Halley's 1686 paper found its way into *Chamber's Cyclopaedia* where the section "Physical Cause of Winds" was copied straight from the last five pages of Halley's text.[10]

Even if "Hadley's Principle" initially was largely ignored by the scientific community, gradually during the next 100 years it would, as we will see, appear once and again, discovered or re-discovered by other scientists. If they had been influenced by Hadley directly or indirectly, or came to think about the mechanism themselves remains unknown. The first time the "Hadley Principle" was brought forward was only a few years after the publication the Transactions and the author, who was Scottish, was no doubt very familiar with the publications of the Royal Society.

MacLaurin and d'Alembert

Colin MacLaurin (1698-1746) argued, without mathematics, in his 1740 work "De Causa physica fluxus et refluxus maris" (On the cause of tides) that the sea currents were affected by "the uneven velocity of a body carried by the earth in its daily motion around its axis":

> If water be carried from the south toward the north, either by the general motion of the tide or by any other cause whatever, the course of the water will thereby be deflected little by little toward the east, because the water at a prior time was carried by the diurnal motion toward this sea with a greater velocity than pertains to the more northerly place. Conversely, if the water be carried from the north toward the south, the course of the water, on account of a similar cause, will be deflected toward the west. From this source I suspect various phenomena of the motion of the sea to arise.

MacLaurin was aware that this explanation could be extended to other motions in the atmosphere and the sea. "But it is not possible to go into this in any detail."[11]

MacLaurin's paper was one of the prize winning contributions to a competition launched by the French Royal Academy of Science. This might have inspired the Berlin Academy of Sciences in 1746 to announce a prize for anybody who could determine "the nature and the law" which the wind ought to obey in case the earth was covered by an ocean. The solution had to be presented in a form that allowed predictions. The winning contribution, by Jean le Rond d'Alembert (1717-83), was published in 1746 under the title "Reflexions sur la cause generale des vents."[12]

D'Alembert made two a priori assumptions which for a modern reader seem completely off the mark: he disregarded the effects of solar heating and the earth's rotation. The winds were supposed to be solely the result only of the attractive forces of the sun and the moon. Assuming that these forces were perpendicular to the earth's axis of rotation, d'Alembert developed equations expressing the resulting oscillations. In a final part he considered the effect of landmasses, in particular mountains.[13]

D'Alembert's approach must be understood from the background of the success of the Newtonian concepts, which had been able to explain celestial mechanics, the shape of the earth, and above all, the dynamics of tides without invoking effects of thermal heating. Although d'Alembert stated correctly that the rotation of the earth has no effect on the velocity of the wind, he never seemed to have realized the modifying effect on already moving objects.

This was the first attempt to express the motions of the atmosphere in mathematical terms; a new fruitful trend in meteorology which would see its major "milestones" in W. Ferrel's equations of motion in 1860, L.F. Richardson's numerical hand-calculations in 1922 and N.A. Phillips computer based general circulation experiment in 1956.

Together with Denis Diderot (1713-1784), d'Alembert was instrumental in the creation of the legendary *Grande Encyclopedie*. Originally only a project to translate Chamber's *Cyclopaedia* into French, it soon took off in its own direction. It is generally regarded as a progressive enterprise supporting the enlightenment, common sense and new knowledge. However, in the case of the understanding of the atmosphere's general circulation the *Grande Encyclopedie* succeeded less well. The section on "Winds" was a straight translation of the Chamber's version. This is the reason why Halley's 1686 explanation of the trade winds remained the most widely known internationally almost to the beginning of the 19th century. However, in the middle of the section there is an extensive insertion, most likely by d'Alembert, outlining his own explanation. It found its way into several books on natural sciences in the 18th century[14] and as late as 1859 d'Alembert's outdated 1746 explanation provided the basis for a French paper on the general circulation.[15]

Unknown to the readers of all these encyclopaedias was that Halley himself did not quite believe his own theory. When one of his colleagues, the mathematician John Wallis (1616-1703) admitted that he could not understand why the sun's heat should cause a westerly oriented rather than easterly oriented Halley, unable to find new convincing arguments, started to doubt his own hypothesis: "Your questioning my hypothesis for solving the trade winds makes me less confident of the truth thereof, and I should be glad to see some other notion where by more of the appearances would be naturally solved."[16] But these doubts would remain private to the loss to later generations through the 18th century.

In the mid-18th century "Hadley's Principle" slowly started to become appreciated, obviously independently of Hadley. It first happened in the other end of continental Europe, in Königsberg (today's Kaliningrad) and by their most renowned son, the philosopher Immanuel Kant.

Immanuel Kant (1724-1804)

It is easy to forget that Kant during most of his professional life worked as a scientist or academic in physics, mathematics and earth sciences[17]. His philosophical works, in particular *Critique of Pure Reason*, on which his fame rests, came about quite late in his life, when he was in his 50's. In the period 1747-56, when Kant was 24 to 32 years old he published several works on kinetic energy, the possible changes of the earth's rotation, the age of the earth and the mechanisms of earthquakes. In 1755 he outlined a theory of the formation of the universe, in particular the solar system. This would later be further developed by Laplace in his 1799 celestial mechanics into the "Kant-Laplace nebular hypothesis."

Kant's texts on meteorological problems, in particular on winds, are found mainly in three sources: (A) "Neue Anmerkungen zur Erläuterung der Theorie der Winde" (New Comments to clarify the Theory of Winds), a pamphlet of about 15-20 pages published in Königsberg in April 1756, consisting of five "Comments" with the following headings: (1) Differential heating drives the wind as long as the heating persists; (2) The heated air replaces the cold air; (3) The wind from the equator to the pole becomes increasingly westerly due to the rotation of the earth; (4) The easterly trade winds for the same reason; (5) The monsoon is also explained by the 3rd cause. Kant reached the conclusion that there existed an upper current directed towards the pole. Weather is caused when this upper wind comes into conflict with the surface wind. He noted, as others before him (and after him would do), that the wind locally tended to veer from E to S to W[18].

The second source, *Physikalische Geographie* (Physical Geography) is a texbook comprising 158 pages out of which 17 deal with the atmosphere general circulation[19]. It deals with the atmosphere circulation in five chapters with the following headings: On the Trade Winds; Sea- and land breezes; Monsoons and other periodic winds; Cause of the monsoons; Yet some rules of the variations of wind. The text was published at the end of Kant's life, in 1802 but was probably written twenty years earlier. Kant had by then established himself as a renowned philosopher and a new book by him about the earth, oceans and atmosphere was likely to attract attention.

The third source, Kant's handwritten lecture notes from 1756 to 1796, have been published in several editions 1838-39, 1868, 1898, 1911 and 1925[20]. The part dealing with meteorology occupies about ten pages and contains the following chapters: (On the winds; Law of the Trade winds from the rotation of the earth; A law on the monsoons due to the same reason; Some scattered comments on the law of winds. Meteorological discussions are also found in Kant's so called "Vorkritischen Schriften."[21]

Kant starts by refuting the old Galilean notion about the tropical air "lagging behind" the earth's rotation. Like Wallis he found Halley's explanation of the motion of the maximum solar heating from east to west "badly chosen" since it would rather cause a diurnal change of wind between west in the morning and east in the evening, with calm conditions in between at midday and midnight. Instead he was designing an explanation of his own and he confessed in his notes: "I am right here busy to renew, the old theory, though with one added condition only to make it mechanical possible…This rule, which as far as I know, not anyone has considered, may be seen as a key to a general theory of the winds."[22]

From Kant's handwritten notes we are able to reconstruct, also graphically, how he, on the blackboard at the university in Königsberg, outlined the deflective effect of the earth's rotation, his "key to the general theory of the winds" (see figure 1):

> **N** and **S** design the two poles, **W** to **O** the equatorial circle. Two latitude circles are marked as **mn** and **hi**, and the remaining are meridians. If there is no wind in **a** so it has no other motion than the one which is appropriate for the earth's surface in this point **a**, that is the half of what the latitude circle **hi** covers in 12 hours from west to east.
>
> From now on let us assume that the air in **a** moves to **b** along a meridian, and let us imagine that this increasing north wind in the same time could follow the curve **ea** from west to east due to the rotation of the earth. Then follows, if we disregard all obstacles that could meet the air during its course, is on a moving earth would not be at **b**, but at **c** at the end of this time, so that **dc**=**ea** and **cb** the difference of similar latitude circles, because the air with its intrinsic westerly velocity of the place, from whence it came, can cover in the same time the curve **dc**=**ea** from west to east, since the earth meanwhile at this latitude has described the curve **db**. Since it does not matter if the air moves with respect to the earth, or the earth moves with respect to the air, a combined movement will follow along a certain diagonal curve **ac**, of which the sides **ab** and **bc** represent those northerly

> wind velocities, and the difference of the motion at both latitude circles, respectively.

Kant then proceeded to apply the same reasoning for winds moving poleward into the westerlies:

> Every south wind has on our hemisphere a tendency as it progresses to turn into a southwest wind, and does indeed so when the conditions are present, as is shown in the previous case. If the velocity is the same as before and it starts in point **b** with the velocity **ba**, so will the westerly velocity, which it due to the rotation of the earth around its axis carries with it, cause that it will in the same time cover the curve **ag=db** and at the end itself be at **g**…

The more the air moves away from the equator the more it is deflected until it becomes straight from west.

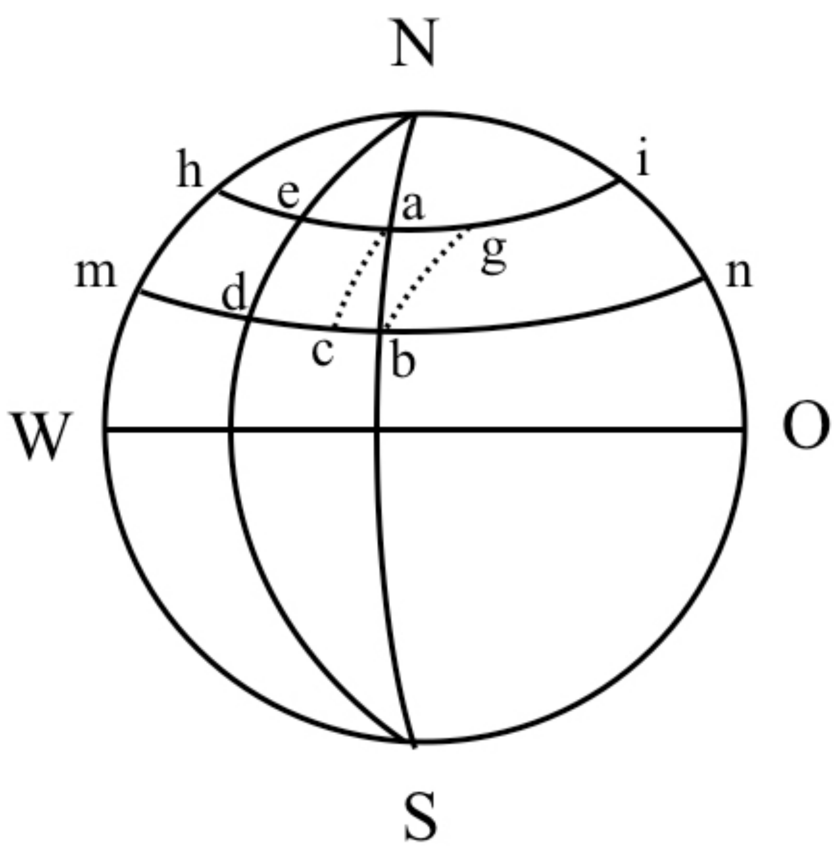

Fig. 1. Graphical reconstruction from Kant's handwritten notes of his explanation of the deflective effect of the earth's rotation.

One has to enter the vast field of Kantian research to find out how much impact Kant's ideas on the general circulation of the atmosphere had on his contemporaries, for example how widely his 1756 pamphlet "Neue Anmerkungen der Winde" was read, and how his lectures in natural philosophy at the Königsberg University were received by the students. Throughout the 19th century there are, however, occasional references to Kant in the meteorological literature,[23] mostly related to the publication of his 1802 book on physical geography.

Late 18th century scientists

Laplace

In the late 18th century "Hadley's Principle" became more and more appreciated, sometimes with references to Hadley. One main promoter of "Hadley's Principle" in the second half of the 18th century was no other than Pierre Simon de Laplace (1749-1827). Laplace had in 1775 set out to develop what would become known as "Laplace Tidal Equations," a rigorous mathematical description of the motions of the atmosphere and ocean, taking the rotation of the earth into account. The latter was not least important because Laplace had reached the conclusion in contrast to d'Alembert that the rotation of the earth had an importance of its own, not only for changing the diurnal position of the sun and moon.

> Considering that the planet has a rotation like a liquid, the velocity of a molecule is supposed to be the same in the direction of a latitude, its angular velocity increasing or decreasing if it moves away or approaches the equator, so that it changes the meridian of this motion when it changes latitude.[24]

The extension to the atmosphere came in 1796 with Laplace's publication of his semi-popular presentation on celestial mechanics:

> …the attraction of the sun and the moon does not produce, neither or the sea, nor in the atmosphere any constant motion from east to west, like the one, called Trade-Winds, that is observed in the atmosphere between the tropical circles. They are caused by something else, here is the most probable…

Laplace envisaged two opposite currents of air, one in the lower part of the atmosphere and the other one in the upper part of the atmosphere.

> However, the real velocity of the air, due to the rotation of the earth, becomes lower when it is much closer to the pole. Thus it should when advancing towards the equator, rotate more slowly than the corresponding parts of the earth, and the bodies placed on the earth's surface should hit it with the excess of their velocity and feel as a reaction, a resistance counter to their motion of rotation. Thus, for an observer who believes himself to be immobile, the air appears to blow in the opposite direction to the one of the rotation of the earth, that is to say from east to west, this is indeed the direction of the Trade-Winds.[25]

In his *Traité de Mécanique Celeste* (1799) Laplace would repeat, with renewed emphasis and in indirect polemic with d'Alembert', that the trade winds are not caused by the gravitational attraction from the sun and the moon.

Immanuel Kant and Simon de Laplace overlap historically and scientifically (The Kant-Laplace Nebular Theory), but they never met or exchanged letters, and there are no indications that Laplace ever read Kant's works, in particular those dealing with meteorological problems. Laplace mentioned in his preface[26] that he was inspired by Newton, Euler and Bernoulli , but most by d'Alembert and MacLaurin, He might of course have come to think about the trade wind explanation completely on his own, just as Kant did and another contemporary scientist, Jean André de Luc.

De Luc and Dalton

In his "Idées sur la météorologie" from 1787[27] the Swiss scientist, Jean-André de Luc de Luc (1727-1817) explained the prevalence of southwesterly and northeasterly winds as a consequence of the earth's rotation on north-south air displacements caused by the sun's differential heating:

> If the air leaving the equator was calm there, i.e. if its movement is the same as the movement of the surface of the earth, when it arrives at our climate, and if it still has conserved a part of its movement in this sense; then it should go quicker than the surface of the earth in the same meaning being from west to east, and become south-west. The inverse cause change, for us to north-east the winds from north."

It is possible that de Luc, who was a frequent visitor to England where he held influential contacts, had read or heard about Hadley's paper. However, it is more likely that he had the idea from de Laplace's 1775 articles or might have come to think of it himself. That was at least the opinion of another prominent scientist, John Dalton.

But the first time George Hadley gets explicitly associated with the explanation of the trade-winds, without being confused with Edmond Halley or John Hadley, seems to be by the English chemist and natural philosopher John Dalton (1766-1844). Although Dalton's fame today rests almost entirely on his atomic theory, he carried out a wide range of research. In 1787 he began keeping a meteorological journal which he continued all his life. In his 1793 book *Meteorological Observations and Essays* he outlined an explanation of how, "The effect of the earth's rotation to produce, or rather to accelerate the relative velocity of winds, being as the difference, or more strictly, to the [sine] of the latitude…increases in approaching the poles."

Dalton credited de Luc as "the only person, as far as I know, who have suggested the idea of the earth's rotation altering the direction of the wind." Only when Dalton's book was in its final stages did he find out that the trade-winds "had been explained on the very same principles and in the same manner" by his country-man George Hadley. Dalton expressed his surprise and perhaps irritation that Halley's 1686 theory, in spite of being "inadequate and immechanical," had become "almost universally adapted" and could be found in "several modern productions of great repute… On the other hand, G. Hadley Esq, published in a subsequent volume of said Transactions a rational and satisfactory explanation of the trade-winds; but where else shall we find it?" [28] Dalton re-issued his book in 1834 with few added notes, none of which dealt with atmospheric circulation.[29]

Lampadius and Brandes

Another expression of the growing interest in "natural philosophy" was the slowly increasing number of meteorological textbooks. One such was *Systematische Grundriss der Atmosphärologie* by W. A. E. Lampadius (1772-1842) at the University of Freiberg in Saxony. Lampadius refuted the Galilean-Mariotte model and acknowledged Kant and de Luc who had quite correctly "and as far as I know" been the first to formulate the basic law about the influence of the rotation of the earth. Lampadius also made the important point that "the earth's rotation can only change the wind not create it."[30]

Heinrich W. Brandes (1777-1834) was professor in Breslau (Wroclaw) and the father of synoptic meteorology. He was aware of the role of the earth's rotation and in his 1820 textbook, *Beiträge zur Witterungskunde*, correctly credited Hadley for his 1735 trade wind explanation.

> When there is a flow of cold air towards the south, it must appear to use, not as a north wind, but as a north-east wind, because the rotation of the earth gives our region a faster velocity to the east than the polar regions….I have no doubt that this explanation is the right one."[31]

While discussing the wind and pressure differences between La Rochelle and Bordeaux, Brandes was close to discovering the geostrophic wind law[32]. However, the meteorologist who really put George Hadley's name on the meteorological map was a student of Brandes, a scientist who would dominate meteorology not only in Germany, but also in Europe for more than four decades.

Germany's dominating meteorologist

Heinrich W. Dove (1804-1879) published more than 300 papers, not only in meteorology but also in experimental physics, especially optics and electromagnetism. By his contemporaries he was hailed as the "greatest meteorologist of our time" and "the Father of present day meteorology." Professor at the university in Berlin, lecturer at several civilian and military schools, a member of the Prussian Academy of Science, and Director of the Prussian Meteorological Institute, he exerted a strong, sometimes dictatorial, influence on the meteorological debate.

Dove's early career

When he was only 18 years of age, Dove entered H.W. Brandes' institution at the University of Breslau in 1822. After a couple of years at the Berlin University, Dove moved in 1826 to University of Königsberg to work as a "Privatdozent." It is here, in a contribution to the leading German scientific journal *Annalen der Physik,* he presented what would be known alternatively as "Dove's Law of Turning," "Dove's Wind Law," or "The Law of Gyration."

Based on a series of observations in the course of twelve days in Königsberg, from 25 September to 6 October 1826, Dove inferred the existence of a law-bound, clockwise variation of the wind. But "Dove's Law" was just a reflection of the typical wind changes in the extratropical westerlies for locations south of the main storm track. Since most of continental Europe was south of this track it was easy to find confirmations of the "law" from seamen, weather amateurs, renowned philosophers and scientists[33].

Dove invokes the rotation of the earth

When Dove's attempt to establish mathematical-statistical equations for his "wind law" failed he turned to dynamical arguments. According to Dove all wind systems (the trade winds, the monsoons and the westerlies) were "necessary and simple consequences of the same fundamental causes," the effect of the rotation of the earth. In 1831, in a paper in *Annalen,* that would mark the real start of his scientific career, he explained the monsoon wind systems as a consequence of the earth rotation[34]. In 1835 he used the same mechanism to underpin the "wind law "[35] in an equally influential article on the effects of the earth's rotation on the atmospheric flow.

Dove imagined air parcels lined up in north-south direction. By some impetus they were brought into meridional motion. Those air parcels closest to the observer would arrive first and have had least time to be affected by the earth rotation, those arriving from further away would have had time to be more deflected. Air parcels arriving from N would gradually arrive from a more NE directed those from the S from an increasingly SW direction. Air parcels further away would have become even more deflected and arrive as E respectively W winds. This led Dove to postulate two major air flows, one warm south-westerly, one cold and north-easterly[36].

Did Kant influence Dove?

Dove spent 1825-27 at the university in Königsberg, where Kant once had lectured. At least two of Kant's works containing meteorological discussions, *Anmerkungen* and *Physikalische Geographie*, were available in Dove's time. They contained all the ideas that Dove promoted so forcefully: the typical veering of the wind, the idea about two contesting air masses and the effect of the earth's rotation.

On the other hand Kant's *Anmerkungen* did not seem to have had any impact on the scientific community until it was published in connection with Kant's collected works later in the 19th century *Physikalische Geographie* was published in 1802, but it is not known how widely it had found readers. Brandes, Dove's professor in Breslau, as late as 1820 did not seem to have been aware of Kant's contributions. In 1846 Dove claimed that he only recently had became aware of Kant's meteorological texts: "This theory [The Law of the Turning] is partly, as I have seen later, briefly hinted at, although in a place where I least had looked for it, namely by Kant in his physical geography."[37] There is nothing to suggest that he was not telling the truth. That cannot quite be said about the priority controversy he was involved in nearly ten years earlier.

Dove's polemic with Dalton raises Hadley to fame

In the first paragraph of his influential 1835 paper Dove had exaggerated the importance of his finding by neglecting any contributions from anybody else: "…it must seem strange that since 1685, in which year Halley published his theory of the trade-winds, consequently for 150 years, *not a step* [emphasis added] has been made towards a general solution of the question." When Dove's article two years later was translated into English and published in the September 1837 issue of *Philosophical Magazine* under the title "The Influence of the Rotation of the Earth on the Currents of its Atmosphere; being Outlines of a General Theory of the Winds," it caught the eyes of the ailing John Dalton. After seeing Dove's bombastic priority claim, he sent a letter to the editor-in-chief[38]:

Notice Relative to the Theory of Winds
By John Dalton, D.C.L., F.R.S.
To Richard Taylor, Esq

Dear Friend Manchester, Sept 5th 1837

I published a theory of the Trade Winds, &c, as Mr Dove has published, - it was forty-four years ago, as may be seen in my Meteorology, 1793 and 1834. It was first published by G. Hadley, Esq, in 1735, as I afterwards learnt. It is astonishing to find how the true theory should have stood out so long.

—John Dalton

Dalton's letter was published in the next (October) issue of Philosophical Magazine. It soon reached the editor of *Annalen der Physik*, where it was published together with a long reply by Dove.

At about this time Dove was busy editing a collection of his most important papers with an added chapter on the general circulation of the atmosphere[39] contained numerous references to Hadley[40]. Those parts where Hadley was mentioned half a dozen times were reprinted in *Annalen* together with a long-rambling justification (where Hadley is mentioned another seven times): "My theory has with Hadley's that in common, or rather borrowed from it, that the most important moment is the different rotation velocities at different latitudes…" The reason why he in his previous work never mentioned Hadley was, Dove explained, because he was so well-known: "It is unnecessary in a scientific journal to mention what everybody already knows and no other theory than his can have been alluded to." He then reminded the readers that neither Hadley's, Dalton's, nor anybody else's work contained a "turning law." The article ends:

> As I am convinced that I have never deliberately kept silent about what others have already published with respect to subjects I have investigated, so I believed I could avert the suspicion that I was seeking to appropriate the intellectual property of a man of such statue that was beyond the reach of my praise or criticism.

From now on Dove never failed to mention Hadley's name in connection with his own "Law of the turning." In 1857 he mentioned Hadley several times in a talk to the Berlin Academy of Science and in a paper he submitted to the French Academy of Science.[41] In his 1861 book *Gesetz der Stürme* (The Law of the Storms), Dove duly credited Hadley. Indeed, Dove championed Hadley's Principle so persistently that it gradually became known as the "Hadley-Dove Principle." However, when he was confronted with William Ferrel's (correct) analysis of the deflective effect, he tactically but quite erroneously interpreted it as an "extension of the principle of Hadley's theory." This caused some consternation among Dove's many followers who were convinced that Ferrel was outright wrong.

The heydays of Hadley's Principle

Dove made Hadley known in Britain

Until his death in 1879 Dove remained a dominating personality in European meteorology; his ideas were particularly well received in the United Kingdom. Throughout most of the 19th century there was no meteorologist in Britain with any interest or qualification in theoretical or dynamical meteorology. The leading authority in questions regarding the motions of the atmosphere was the astronomer John Herschel (son of the famous astronomer William Herschel who discovered Uranus). John Herschel published during almost half a century, from the 1830s to the 1870s, theoretical articles and books about astronomy and physical geography. He also contributed a long article on "Meteorology" for the *Encyclopaedia Britannica*.[42]

But it was thanks to Dove's strong influence in British meteorology that Hadley and his "Principle" finally became well-known in his own country, although the British tended rather to credit Dove than Hadley. The renowned English Francis Galton (cousin of Charles Darwin) expressed in 1862 admiration for Dove's "well known theory" about the winds, which he considered "so fertile in result "[43]. The translation of Dove's *Das Gesetz der Stürme* in 1862[44] was made by a prominent British meteorologists Robert H. Scott (1833-1916) who had worked some years in Germany with Dove and would later be the longest serving Director of the UK Meteorological Office, 1867-1900[45].

With "Hadley's Principle" firmly established in the meteorological community it became possible to explore in a rudimentary way to follow up what Brandes had touched upon in 1820, the link between the wind and the pressure distribution. In particular why did the winds not blow straight into low pressure systems and straight out of high pressure systems?

Buys Ballot's Law

In the late 1850's C.H.D. Buys Ballot (1817-90), a Dutch physicist, published papers to illuminate the coupling between the horizontal pressure distribution and the wind, in particular the direction of the wind. Soon "Buys Ballots Rule" had won acceptance around Europe: "…if one turns in the direction of the wind, with the back to the place where it comes from, one will have the lowest pressure on the left hand side…"

Buys Ballot had started as a devoted follower of Dove's concepts and in 1853 published a paper confirming "Dove's Wind Law "[46]. When Buys Ballot in 1857 published his new theory it was highly controversial since it challenged Dove's authority[47]. His formulation that "one can better judge the wind from the barometer than from a wind vane" did not please Dove. Later Buys Ballot admitted: "This Rule has cost me the favour of my beneficiary Dove. He had called me the best defender of his Law of Turning…and now I had to prove just the opposite. Not with the sun, in our hemisphere, but against the sun should the air move."

Hadley's Principle could only explain "Buys Ballot's Rule" for winds from a northerly or southerly direction (or winds with north and south components). The respected British meteorologist William Clement Ley (1840-96) tried to accommodate "Hadley's Principle" with the inflow in a cyclone from any direction. In his book *The Laws of the Winds* from 1872 he suggested that winds from straight east or west, according to "Hadley's Principle" not affected by the earth's rotation, were assumed to be directed by some internal forcing from neighbouring portions of the atmosphere the north and south winds.

"Hadley's Principle" questioned

When finally in the mid 19th century, much thanks to Hadley's pioneering paper, it was realised that the rotation of the earth exercised a profound influence on the general circulation of the atmosphere and in particular the trade winds, a deeper understanding of the mechanism and its consequences had developed and "Hadley's Principle" came under increasing attacks from both theoretical and practical meteorologists.

Foucault and Coriolis

In 1851 Jean Bernard Foucault (1819-68) had conducted his pendulum experiment, which unleashed an international debate about the deflective mechanism of the earth's rotation. It now became clear that it was equally affecting motions of *all* directions[48]. In 1859 the French Academy of Science organised a comprehensive debate about the practical consequences of this deflection, primarily on flow in rivers and the balance of railway trains.

It is now that Gaspard Gustave Coriolis (1792-1843) and his 1835 work on relative motion in a rotating system is re-discovered. Coriolis showed that the inertial (centrifugal) force Ω^2R or U^2/R, until then only considered for objects stationary (at distance R from the centre) in the rotating system (Ω), had to be extended by an extra term, $-2\Omega V_r$, to account for the total inertial force on an objects moving (V_r) relative within the system. In 1837-38 Siméon Denis Poisson (1781-1840) applied Coriolis theorem on deflection of artillery gunnery[49].

Ferrel and Maury

The man who in a correct way, scientifically and physically, brought in the effects of the earth's rotation in meteorology was a mathematically gifted school teacher in Tennessee, William Ferrel (1817-91). His positive inspiration came from Foucault's pendulum experiment and Nathaniel Bowditch's English translations of Laplace's *Mécanique Celeste*. The negative inspiration, or challenge, came from awkward explanations of the general circulation in contemporary popular literature, in particular Maury's *Physical Geography of the Sea*.

M.F. Maury (1806-73) was a leading American authority on marine and oceanographic problems. In 1853 he had been instrumental in calling an international meeting in Brussels to coordinate the marine traffic. His 1855 *Physical Geography* became a bestseller with sixth editions. If there ever was a "Da Vinci Code of Oceanography" then it was this book. Profound observations, like Ekman transports in the Gulf Stream, were mixed with pure fantasies and religious peculations. Nothing, however, evoked more general unanimous opposition in the world of science than Maury's scheme of the circulation of the atmosphere. Maury's main hypothesis was that the high level currents towards the poles and the low level currents from the poles crossed around the subtropical high pressure belt. Against all observations to the contrary Maury postulated low pressure in the polar regions with an inflow of southwesterly winds.

Ferrel not only derived a correct expression of the Coriolis effect, he also, after some minor mishaps, reached a correct mathematical and physical understanding of the deflective effect and its consequences for the atmospheric flow. This brought him into opposition to the Hadley-Dove explanation: "The reader is no doubt familiar with Hadley's theory. Although [it] furnishes an explanation of the trade-winds, yet it does not account for many other remarkable phenomena in the motions of the atmosphere."[50] It was not until the mid 1870s that Ferrel's results became known in Europe. When they finally did, they would have a profound impact.

Peslin and Mohn:

Inspired by the 1859 Academy debate the French scientist Peslin (1836- ?) derived the geostrophic wind equation in 1869. When his paper was rejected by the French Academy of Science he published it in a little-read astronomical publication[51].

A half-baked attempt by a Danish engineer L.A. Colding (1815-1888) to derive an expression for the geostrophic flow[52] challenged the Norwegian meteorologist, later professor, Henrik Mohn (1835-1916) to do better. Also Mohn had followed the debate in the French Academy of Science in 1859, and in particular taken impression from Babinet's arguments that the deflective effect worked for all directions of motion.

This view was expressed in Mohn's 1870 *Storm-Atlas* and two years later in his semi-popular book *Om Vind og Vejr, meteorologins hovedresultater* (On Wind and Weather, the main results of meteorology)[53]. Already in the preface Mohn made the readers aware that dynamic meteorology had entered a period when old traditions were challenged: "When there is a disagreement between the old and new opinions, I would advice the reader, to consider the basis upon which both are founded and thereafter chose, rather than to try to find agreement where there cannot be any."[54] Mohn's articles had a profound effect for the development of dynamic meteorology in Germany.

Development of modern dynamic meteorology in Germany and Austria

Well before Dove's death in 1879 a "thaw" had developed in the meteorological science in Germany. The younger generation at last felt free to admit that Dove's quite "unphysical theories" had retarded the progress of meteorology "for a long time "[55].

The impact of Ferrel's and Mohn's texts

The Germans became aware of both Mohn's and Ferrel's theories only in 1875. In December 1874 Mohn had written to *Meteorologische Zeitschrift* that he and a colleague, C.M. Guldberg, in 1872 had found the relation between wind and pressure. Only a few days later Julius v. Hann the editor of *Meteorologische Zeitung* received a letter from Cleveland Abbe who alerted the journal to Ferrel's papers. In 1876 Guldberg and Mohn published their French speaking article which was immediately translated into German by Mohn. On request from Hann they modified it to become more easily accessible and it was published in 1877 in Meteorologische Zeitung. Extended versions of Mohn's book would see five German editions, the first one in 1879 with a preface by George von Neumayer (1826-1900), the head of the Hamburg Seewetteramt. In his preface Neumayer expected that Mohn's book, together with German works "of solid character," would have a "beneficial influence" although in many instances "diverging views" were presented[56].

Stimulated by Ferrel and Mohn opposition to Dove's and Hadley's theories grew among theoretical German meteorologists in the 1870's [57]. More articles by Finger, Sprung and Thiesen followed. The debate for and against the "Dove-Hadley Principle" ended with a victory for the theoreticians[58]. The break with the Hadley-Dove model was not always easy. For many it took some years to realise that the Hadley-Dove model was not a "simplified" or "incomplete" version of Ferrel's correct model, but completely wrong[59].

German theoretical criticism of Hadley's Principle

There were three major points in the criticisms of "Hadley's Principle" in the German meteorological community: (1) Hadley's explanation only works for north-south motion, although the deflection is valid for all directions; (2) The underlying conservation principle should not be one of absolute linear momentum, which only yields ΩV ½ the deflection, but of absolute *angular* momentum which yields 2ΩV; (3) The assumption about an impulsive force, pushing the air is unrealistic for the atmospheric mechanical system

Even today there is therefore a wide spread notion that Hadley was not really wrong, only incomplete and that his explanation was a simplified version of the correct one. This is because only the first two points have been understood and considered.

> 1. After all, being able to account for deflection only of north-south winds can be seen as a step in the right direction. It was not fully realised that the basic assumption, latitudinal difference in the rotation velocity, is inadequate. The mathematical formulation of the Coriolis effect does not contain any reference to any latitudinal variation in the rotational velocity. There is also Coriolis deflection for motions along a latitude, where there is no change in the rotational velocity.
>
> 2. Getting something wrong with a factor of 2 is not necessarily crucial, but the assumption of conservation of absolute linear momentum was physically wrong. A body set in motion by an impulse will follow an "inertia circle" path (with radius $R=V_r/2\Omega\sin\varphi$) on the earth's surface and during its circular motion conserve its absolute angular momentum around the earth's axis, but not its absolute linear momentum, as was required by "Hadley's Principle." Indeed, when it, during its circular course has a direction eastward, *with* the earth's rotation, its absolute linear momentum will be greater than when it is moving westward, *against* the earth's rotation. *The assumption in "Hadley's Principle" that the absolute velocity is conserved is therefore unphysical*.

But these objections were not really fatal because they ignored the 3rd point of criticism. *Hadley's set-up was physically wrong*. As the German meteorologist Adolph Mühry (1810-88) commented already in 1869, "One cannot compare the motions of the air with that of a fired cannonball."[60]

Mühry's criticism was supported by Adolph Sprung ten years later when he repeated that we have to distinguish between pure inertial motions and those which are affected by permanently acting forces[61]. Unfortunately, very few understood Mühry's and Sprung's criticism which turned out to be *the* really fatal objection to the "Hadley Principle." Criticism along this line would be pursued by "practical" meteorologists, although with different arguments.

The fatal error with Hadley's Principle:

The success with explaining the *direction* of the winds by invoking the rotation of the earth was not accompanied with a similar success to explain their *velocities*. From the late 1830's, when Hadley's Principle became generally accepted, there is an increasing criticism of the excessive winds it predicted. Indeed, in the mid-1800's serious doubts had formed if the rotation of the earth had any importance at all on the motions of the atmosphere!

Problems with the trade-wind velocities:

The problem to explain unrealistic winds had been there even with the Galilee-Mariotte explanation of the trade-winds. Why did the air, lagging behind the earth's rotation do so by only 5-10 m/s at latitudes where the rotational velocity was more than 400 m/s? [62] Whatever one might think of the contributions by Halley and d'Alembert, their disregard of the earth's rotation salvaged them at least from the problem to explain excessive winds!

In 1840 in a book about the wind systems on the world's oceans a renowned French sea captain ("Capitaine de Corvette") Jean Lartigue (1791-1876)[63], expressed doubts that the earth's rotation around its axis really was important for the trade winds. In the next edition of his book (in 1855) this chapter was omitted because, as Lartigue explained, "a presentation of facts is sufficient" instead of an insufficient explanation."

From initially having endorsed Hadley's Principle, in later editions of his *Physical Geography* Maury became more critical and finally reached the verdict that diurnal rotation should therefore not be regarded as the sole cause of the easterly direction of the trade-winds.

Although John Herschel's view was that Hadley's model afforded "an easy and satisfactory explanation of the magnificent phenomena in question [the trade-winds]" there were complications: "…Were any considerable mass of air to be suddenly transferred from beyond the tropics to the equator, the difference of the rotary velocities proper to the two situations would be so great as to produce not merely a wind, but a tempest of the most destructive violence."

These hurricane winds, Herschel assured the reader, would not 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 also thought that at least ½, more probably 2/3 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[64].

More puzzling results came from trajectory calculations. A German meteorologist who wanted to test the hypothesis that the 30 m/s warm föhn winds in the Alps had tropical origin, 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 and had travelled over southern India, via the Arabic Peninsula and the eastern Mediterranean, it would also have had an initial velocity of 121 m/s, which decreased slowly as the air progressed poleward[65].

Excessive wind velocities in the extra-tropics:

Hadley had himself advocated that his model could be applied to extra-tropical latitudes. This was also one of Dove's contributions. However, in doing so the physical consequences became even more absurd. A wind moving meridionally in the mid-latitudes would increase its velocity by on average 5 m/s for every degree passed[66]. Just moving from 50º to 53º would make the wind increase to gale force, moving to 55º it would increase to full storm. French meteorologists wondered that air from Paris blowing to Newcastle would increase to a westerly hurricane of 35 m/s. In his 1735 paper Hadley had explained why this did not occur by frictional losses towards the earth's surface. This argument lost its validity when the winds at higher levels were considered where the friction is almost negligible. On the other hand, observations from the higher parts of the atmosphere seemed to give some credence to the existence of very strong winds.

The strong winds in the upper "anti trade wind"

Already Halley in 1686 had predicted the existence of an upper-air return flow and had given it the name the anti-trade wind: "…by a contrary Current, the upper Air must move from those parts where the greatest Heat is: So by a kind of Circulation, the North-East Trade Wind below, will be attended with a South Westerly above, and the South Easterly with a North West Wind above."

Hadley had in 1735 extended his model to the upper levels in a combined attempt to account for Halley's "Anti Trade Wind," and to explain the extratropical westerlies, the "West Trade Winds":

> [The heated and lifted air] will then spread itself abroad over the other Air, and so its Motion in the upper Regions must be to the N. and S. from the equator. Being got up at a Distance from the Surface of the Earth, it will soon lose great Part of its Heat, and thereby acquire Density and Gravity sufficient to make it approach its Surface again…those Parts beyond the Tropics where the westerly Winds are found.

Since the air now has the greater velocity than the earth it will now appear as a strong west wind, its strength "proportional to the Difference of Velocity [at the equator]." The "Anti-trade wind" would figure prominently during the coming centuries, in particular when cloud observations in the subtropics revealed the existence of rapid south-westerly winds of 20-40 m/s, sometimes the 60 m/s predicted by the "Hadley Principle."

Hadley and the British meteorologists

When the meteorological world slowly came to realize that the "Hadley Principle" was not correct, one would assume that Hadley's compatriots, the British meteorologists were the last to abandon him. On the contrary – *they had been the most sceptical from the start.* To the British meteorologists of the late 19th century, "Hadley's Principle," rather suggested that the rotation of the earth hardly affected the atmosphere or the oceans at all!

J.K. Laughton

The man who most forcefully formulated this criticism was John K. Laughton (1830-1915), instructor, teacher and textbook author in astronomy, meteorology and oceanography in the Royal Navy, best known as the founder of modern naval history[67]. In his scientific publications Laughton comes over as a strong empiricist with a deep-rooted scepticism about theories. In his powerfully argued 1873 book, *Physical Geography in its relation to the prevailing winds and currents*[68] Laughton dismissed the prevailing concept of the circulation of the atmosphere as unscientific and incompatible with known phenomena. He found the trade wind frequently dying away on the equator rather than "a storm of unheard-of severity, whose fury nothing could withstand…. The commonly received theory of the trade-winds and of the circulation of air over the globe [was] open to very grave objections." Laughton concluded that the friction between the air and earth was so great 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."

Together with five other prominent British meteorologists Laughton published in 1878 a collection of articles in a book with the title *Modern Meteorology*.[69] The book is mainly empirical and there is no mentioning of Hadley or the rotation of the earth. Only in the article about the use of the barometer there is a mentioning of "the common, well-known theory of the wind," but then in a quotation from Francis Galton. However, the man who by all standards delivered the most fatal criticism to the "Hadley Principle," gave it what should have been its final *coup de grace,* was an American.

W.M. Davis

William M. Davis (1850-1934) graduated from Harvard at the age of 19. After completing a Masters of Engineering degree he spent three years as a meteorologist in Argentina. He was recruited by Harvard as a geologist and in 1885 became professor of physical geography. He was a leading force in the *American Meteorological Journal.* In the 1880s he published several articles on the deflective force which culminated in his 1890s textbook *Basic Meteorology*,[70]

Like Sprung and others Davis' road to a new and correct understanding of the deflective mechanism was hampered by initial misunderstandings, in particular the recognition of the fundamental errors in the "Hadley Principle." But once all the bits and pieces had come together and fitted, he ventured on a crusade to promote a correct view as laid out by Ferrel. He did not hesitate to criticise the British:

> It is perhaps because of too great attention to mathematical form and relative neglect of the ideas that it clothes that the English mathematicians and meteorologists as a whole have been so little affected by Ferrel's suggestions. His principles as yet have not really touched meteorological science in that conservative country.

In 1899 Davis was invited to speak at the Royal Meteorological Society on the circulation of the atmosphere. Most of the talk consisted of a condemnation of 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":

> 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…. 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 suggested that neither Hadley nor scientists in the first half of the 19th century were to explain the lack of excessive winds using simply the geostrophic wind relation, "Buys Ballot's Law." In the same way as horizontal pressure gradients accelerate the wind, they can also decelerate them in case they are super-geostrophic when they will point toward higher pressure and be retarded. The violent storms didn't need any tremendous friction to abate; the resistance of the pressure field would have accomplished that.[71] According to Davis the conventional explanation of the circulation of the atmosphere, as ordinarily stated "is seriously incompetent" and he called for a "rebellion" against unscientific teaching:

> If [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.

Now, when the global pressure distribution was known, Davis pointed out, "it is curious that students who are 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."

Consequences for British meteorology

At the end of the 19th century British meteorology was left in a bewildered state. Laughton's and Davis' well argued and correct criticism of Hadley's Principle was not followed by any alternative explanation of the Coriolis effect. The British physical scientists seemed to be split into three groups: (1) *Mathematicians* mastered the correct derivations of the Coriolis effect, often based on complicated trigonometrical arguments, but were not particularly interested in the geophysical background or applications; (2) *Meteorologists* tended to discard all theories and stuck to observed empirical facts, because the mathematical derivations were complex and "Hadley's Principle" gave wrong predictions; (3) *Physicists* with no direct involvement in meteorology found "Hadley's Principle" an illuminating explanatory model, more convenient to refer to than the complicated mathematical derivations.

Among the latter we find James Thomson (1822-92), elder brother of Lord Kelvin, who in 1892 held his Bakerian Lecture for the Royal Society on the general circulation of the atmosphere and mentioned Hadley's name almost 40 times, twice as much as that of William Ferrel's: "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."[72] Thomson, who died shortly afterwards, seem to have been completely unaware of the fundamental criticism which German, American and British scientists had launched against "Hadley's Principle." Thomson's lecture appears to have made the British start to appreciate "Hadley's Principle" out of patriotic reasons, unaware that it would distort their understanding of a fundamental mechanical principle.

This is reflected in the man who would soon elevate his country's meteorology to new heights. Sir William Napier Shaw (1854-1945) would at his death be hailed as the "Father of

Modern British Meteorology" and as the first theoretical meteorologist in Britain. But he had a slow start. In a speech to the distinguished Royal Society in June 1904 he gave an awkward mathematical expression for the Coriolis Effect and offered the following explanation: "…the horizontal acceleration, arising from the earth's rotation is along the normal to the path, and to the left (sic!) in the Northern Hemisphere. On that side, therefore, the low pressure (sic!) must lie."

In his 1913-14 *Principia Meteorologica*, a meteorological pastiche of Newton's *Principia*, Shaw hardly included the rotation of the earth in any of his "laws" and "lemmas." Shaw and his colleagues tried to understand the deflection from Tracy's 1843 flawed explanation[73] based on the assumption that inertial, frictionless motion over the earth's surface would follow great circles. As late as 1946 L.F. Richardson still thought in terms of great circle motion when he was trying to become familiar with the Coriolis effect[74].

The survival of Hadley's model

Today the computers are fed with the correct information with respect to the equations of motion. Today's meteorologists, however, try to interpret the output in terms of Hadley's flawed mechanical model, where only the mathematical errors have been corrected but left *Hadley's mechanical arrangement intact*. Parcels of air, or more correctly, hemispheric rings of air, are still supposed to be displaced meridionally by an impulse. Doing so they will, when displaced from the equator to 30° latitude increase to 130 m/s while correctly conserving absolute angular momentum, twice as much as in Hadley's erroneous model. So what is wrong? The answer was given in the book by Davis (1894), in articles in *Monthly Weather Review* by Marvin (1920) and Clough (1920)[75], and finally in the influential textbook by Brunt (1934, 1944)[76]. Let us quote the latter:

> It is frequently stated in meteorological treaties 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 then makes the point that a frictionless moving body at the equator given an impulse to the north of 20 m/s would travel to the north not more than a distance of the order of 1000 km, equivalent to about 9º of latitude, before it is turned back by the Coriolis force in an inertial circle. A parcel of air at latitude 60º given the same impulse would only travel 160 km to the north before it was back in an inertia circle.

Brunt's explanation repeated the observation already made by Ferrel that 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. While the pressure gradient force tries to even out horizontal pressure differences by accelerating the air, the Coriolis force tries to restore the same differences by bringing the air back in an inertial circle motion. The global circulation can be seen as an eternal contest between these two tendencies. According to Brunt, "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." The angular momentum conserving model is therefore out of place for the atmosphere. How it was introduced into dynamic meteorology in the 19th century in order to facilitate the understanding of the effect of the earth's rotation on the atmosphere, will be a topic for a future article.

Conclusion

To be a good 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 you made them. In that sense Aristotle and Ptolemy were great scientists, but not those who echoed them in the 16th century when new facts showed that they were wrong. Copernicus kept numerous epicycles in his heliocentric system, Kepler believed in number mystique and Galileo was convinced that the planetary orbits were perfect circles, still we rightly regard them as great scientists. In the same way Hadley, MacLaurin, Kant, Laplace, de Luc and others have their place in the history as great scientists who pointed to the rotation of the earth as a crucial mechanism for the motions of the atmosphere. But it is not scientific to echo those parts of their explanation that new observations and theories have shown to be wrong.

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[16] H.L. Burstyn, (1966): 178-80.

[17] M. Schneidemühl,"Kant und die moderne Theorie der Winde," *Das Ausland* 63, no 34, (Stuttgart 26 August 1890): 661-64, (also in *Naturwissenschaften*, V, nr 42, 19 oktober 1890); M. Jakobi,"Immanuel Kant und die Lehre von den Winden," *Meteorol. Z.* 20 (1903): 412-21; F. Dorsch,"Kant und die Meteorologie," *Meteorol. Z.* 41 (1924): 280-82; H-G Körber,"Meteorologische Anschauungen bei Immanuel Kant," *NTM-Schiftenr. Gesch. Naturwiss., Technik, Medicin.* 14:2 (Leipzig 1977): 29-36.

[18] I. Kant, *Neue Anmerkungen zur Erläuterung der Theorie der Winde*, (Königsberg 1756), in *Kant's Werke* Band I, (Berlin und Leipzig, 1910): 489-503, comments 582-84.

[19] I. Kant, I., *Physische Geographie* (Königsberg 1802), in *Kant's Werke Band IX*, (Berlin und Leipzig 1923): 289-94, § 67-71, comments 510-13, 517, 536-38

[20] I. Kant, *Handschriftlichen Nachlass*, Band I (Berlin und Leipzig 1925): 553-63

[21] Meteorological discussions are also found in the following articles by Kant: "Von dem ersten Grunde des Unterschiedes der Gegenden im Raume" (1768),"Entwurf Ankündigung eines Collegii der physischen Geographie nebst dem Anhange einer kurzen Betrachtung über die Frage: Ob die Westwinde in unsern Gegenden darum feucht seien, weil sie über ein großes Meer streichen" (1757) and "Fortgesetzte Betrachtung der seit einiger Zeit wahrgenommenen Erderschütterungen" (1756).

[22] Kant, I., *Handschriftlichen Nachlass*, Band I, (Berlin und Leipzig): 555.

[23] References to Kant in W.A. Lampadius, *Systematische Grundriss der Atmosphärologie* (Freiburg 1806):179. A. v. Humboldt, *Kosmos*, Band II (Stuttgart, 1847); A. v. Humboldt,"Beobachtungen über das Gesetz der Wärmeabnahme," *Ann. d. Phys*. (1806): 16-17; K.W.G. Kastner, *Handbuch der Meteorologie* (Erlangen 1823). We know that Helmholtz, who wrote some important papers on meteorology, was much influenced by Kant's works.

[24] P-S. Laplace,"Recherches sur plusieurs points du systeme du monde," *Mém. Acad. Roy. des Sciences* 88 (1775): 91-92, see also Laplace, *Ouevre Complete* 9 (Paris, 1893), 90.

[25] P-S. Laplace, *Exposition du système du monde,* XII(1796): 267-68, see also *Ouevre Complete* 6, livre IV, Ch. XIII (Paris, 1886), 324.

[26] P-S. Laplace, *Méchanique céleste*, Tome II, Ch. V, §44 (Paris, 1878): 312-14.

[27] J-A. De Luc, *Idées sur la meteorology,* Tome II, Sec. II, §840, (Paris 1787).

[28] J. Dalton, *Meteorological observations and essays*, (Manchester, 1793, 2nd edition 1843), viii; N. Shaw, *Manual of Meteorology,* Vol. I (Cambridge 1926), 289-90. The reference to de Luc given by Dalton and Shaw is completely wrong.

[29] J. Dalton, (1793, 1834): 85-87.

[30] W.A. Lampadius, Systematische Grundriss der Atmosphärologie, (Freiburg 1806).

[31] H.W. Brandes, *Beiträge zur Witterungskunde*, (Breslau 1820): 365-70 and 15.

[32] H.W. Brandes (1820), 78-79.

[33] Contradictory evidence came from the Scandinavian countries located in the middle of the storm track. A Danish professor J. F. Schouw, who had published a book on the climate of Denmark, expressed doubts about the general validity of Dove's "wind law." Dove started a vitriolic campaign

against Schouw which lasted almost ten years and in 1833 prompted Schouw to submit a letter of complain to "Annalen" about Dove's "guerrilla war" (Ge= Kleinkrieg).

[34] H.W. Dove,"Über Monssons und Passat," *Ann. d. Phys*. 21(1831): 177-200.

[35] H.W. Dove,"Über den Einfluss der Drehung der Erde auf die Strömungen ihrer Atmosphäre," *Ann. d. Phys.* 36 (1835): 321-51; translated into English "On the influence of the rotation of the earth on the currents of the atmosphere: being outlines of a general theory of winds, *Phil. Mag.* 11 (1837): 227-39, 353-63.

[36] The notion of two contesting air masses seems to have been quite common already in the 18th century. See pages 519-46 in *Transaction of the Royal Society* in 1735 for an anonymous letter discussing the "cause of the winds" in a way that more or less pre-dated not only Dove but even the Bergen school frontal theory by almost 200 years.

[37] H.W. Dove,"Über die von Drehungsgetsetz abhängigen Drehung der Windfahne in gegensatz der durch Wirbelwinde veranlassen," *Ann. d. Phys*, 67 (1846): 297-303.

[38] J. Dalton,"Notice relative to the Theory of Winds," *Phil. Mag*. 11(1837): 390, translated into German and published as J. Dalton,"Bemerkung über die Theorie des Windes," *Ann. d. Phys.* 42 (1837): 315.

[39] H.W.Dove, *Meteorologische Untersuchungen* (Berlin 1837), 344 pp.

[40] H.W.Dove,"Über die verschiedenen Theorien des Windes, als Erwiederung auf vorstehende Bemerkung," *Ann. d. Phys.* 42 (1837): 316-24.

[41] H.W.Dove,"Mémoires sur la physique publiés a l'étranger, Mémoire sur la Théorie génerale des vents par M. Dove, Extraits par Monsieur Verdet," *Annales de chemie et de physique*, 3e serie, 51 (1857): 242-55.

[42] Gregory A. Good, "A Shift of View: Meteorology in John Herschel's Terrestrial Physics," *Intimate Universality: Local and Global Themes in the History of Weather and Climate*, edited by J.R. Fleming, V. Jankovic, and D.R. Coen (Sagamore Beach, Mass.: Science History Publications/USA, 2006), 35-68.

[43] F. Galton,"A Development of the Theory of Cyclones," *Proc. Roy. Soc*., 12 (1863): 385-86.

[44] H.W. Dove, *Das Gesetz der Stürme*, (Verlag Dietrich Reimer, Berlin 1861), 2nd ed. , translated into English as *Law of Storms*, (London 1862, Berlin 1866), 346 pp. See pp. 4-9 for effects of the earth rotation. More than 20 years earlier 1840 Dove had presented a paper "Über das Gesetz der Stürme" in the Academy of Science in Berlin, *Ann. d. Phys*. 52 (1840): 1-41.

[45] J. Burton,"Pen Portraits of Presidents: Robert Henry Scott, MA, DSc, FRS," *Weather* 49, no. 9 (1994): 323-24.

[46] C.H.D. Buys Ballot,"Einiges über das Dove'sche Drehungsgesetz," *Ann d. Phys* 73 (1853): 417-38, 553-66.

[47] C.H.D. Buys-Ballot,"Note sur le rapport de l'intensité de la advection du vent avec les écarts simultanés du barometrè," *Compt. Rend.* 45 (1857): 765-68.

[48] A. Persson,"The Coriolis Effect: 400 years of conflict between common sense and mathematics," *Hist. Meteorol.* 2 (2005): 1-24.

[49] S.D. Poisson,"Extrait de la prémière partie d'un mémorie," *Compt Rend*, 5 (1837): 660-67; S.D. Poisson,"Sur le mouvement des projectiles dans l'air, en avant regard a la rotation de la terre," *J. Ecole Polytech*. 18 (1838): 1-69, translated by F. Waldo and C. Abbe in *Smithsonian Misc. Collections* (1910): 8-15.

[50] W. Ferrel,"An essay on winds and the currents of the Ocean," *Nashville Journ. Medicine Surgery*, 11 (1856): 287-30. Reprinted in *Professional Papers of the Signal Service*, 12 (Washington, 1882): 7-19.

[51] H. Peslin,"Sur le Relation Entre les Variations du Barometre et les Grandes Courants Atmospherique," *Bull. Int. de l'Observatoire de Paris et de L'Observatoire Phys. Centrale de Montsouris*

29 mai-7 julliet. There is an error in his derivation which should have yielded the wrong result of only half the Coriolis force. Full biography and bibliography in M. Rochas,"H. Peslin, ingenieur des Mines à Tarbes," *La Météorologie* 49 (2005): 42-45.

52 A. Colding,"Einige Bemerkungen zu den Strömungsverhältnissen der Luft," *Österr. Meteorol. Z.* 10 (1875): 133-42 An abridged version of two articles in Danish from 1869 and 1971 on the dynamics of water and air currents. Colding was a devoted supporter of Dove's "Law if the Winds." However, he did only consider the geostrophic relation for sea currents, but an attempt failed. When the editor-in-chief for *Meteorologische Zeitung* in a footnote gave the correct equation as g dz/dt = 2Ωsinφ V he was "jumped at" by a reader who from the traditional view argued for the equation g dz/dt = Ωsinφ V.

53 H. Mohn, *Det Norske Meteorologiske Instituts Storm Atlas* (Christiania, 1872).

54 H. Mohn, *Grundzüge der Meteorologie, Die Lehre von Wind und Wetter nach dem neusten Forschungen* (Berlin, 1879).

55 The "young" generation consisted of meteorologists that we tend to regard as "old." When Dove died Julius v. Hann was 40, Wladimir Köppen 33, George von Neumayer 53, Theodor Reye 41 and Adolph Sprung 31.

56 von Neumayer in the introduction to H. Mohn (1879), vi,

57 Guldberg, C.M. and H. Mohn, *Études sur les mouvements de l'atmosphère.* I Partie, (Christiania 1876). II Partie (Christiania 1880), reprinted in Vogt, H. (Ed.) *Norwegian Classical Meteorological Papers Prior to the Bergen School* (Universitetsforlaget Oslo 1966). These papers by Mohn and Guldberg must, together with the Bergen School papers, be counted among the most important Norwegian contributions to meteorology.

58 A. Persson,"The Coriolis Effect: 400 years of conflict between common sense and mathematics," *Hist. Meteorol.* 2 (2005): 1-24.

59 A Swiss professor H. Wettstein in *Die Strömungen des Festen, Flüssigen under Gasförmigen* (Zürich, 1880), argued on pages 218-220 on one hand, from the Foucault experiment, that the deflection was independent of direction, on the other hand invoked "a second form" of influence, the Hadley-Dove model.

60 A. Mühry, *Über die Theorie und das allgemeine geographische System der Winde*, (Göttingen 1869); A. Persson, (2005): 11.

61 A.Sprung,"Ûber das Hadley'sche Prinzip, Anhang zu Studien über den Wind und seine Beziehungen zum Luftdruck," *Archiv des Deutschen Seewarte*, II Jahrgang 1 (1879): 33.

62 Chambers (1728): 328, Musschenbroek (1751): 856-57, §1747 and Eeles (1755): 135 among others.

63 J. Lartigue, *Exposition du système des vents* (1st edition 1840, 2nd 1855). A short biography of Jean Lartigue (1791-1876) is in *Meyers Konversationslexikon* (1888), 526. Lartigue travelled in the South American waters 1820-44 and published some books on their marine and oceanographic conditions.

64 J. Herschel, *Outlines of Astronomy* (London, 1853,1878), §242.

65 A. Mousson,"Über die Bahnbewegung eines freien Theilchens auf einer sich drehenden Kugel," *Ann. d. Phys.*(1866): 652-58. Adolph Sprung later calculated that 30 m/s inertial motion would move clockwise in a circle of 100 km radius trough Nice, Genève, Lindau, Munich and so on. See J.F.W. von Baeyer,"Über die Bahnlinien der Winde auf der sphräoïdischen Erdoberfläche," *Ann. d. Phys.* (1858): 377-404.

66 Already Dalton (1793, 1834) had calculated that air brought from 60º to 70º N would acquire a westerly velocity of 60-70 m/s.

67 D.A.Lambert,"Pen Portraits of Presidents, Sir John Knox Laughton, RN," *Weather* 54, no. 1 (1999): 27-30.

68 J.K.Laughton, *Physical Geography in its relation to the prevailing winds and currents* (London, 1870). Laughton's book was well received by the British meteorological community and led to

his lifelong connection with the Royal Meteorological Society. He was elected a Fellow in 1873 and served as Foreign Secretary in 1880-81 and President in 1882-83.

[69] J.M.Mann, J.K. Laughton et al, *Modern Meteorology, Six lectures under the auspieces of the Meteorologuical Society* (London 1878), German version *Die Moderne Meteorologie* (Berlin 1882). Only the contribution by Richard Strachan "On the Use of the Barometer" comes close to the point by mentioning "the common, well-known theory of the wind," but then in a quotation from Francis Galton.

[70] W.M.Davis, *Elementary Meteorology* (Boston, 1893), 367 pp.

[71] See also E. Lorenz, *The Nature and Theory of the General Circulation of the Atmosphere*, (Geneva, 1967), 69-70 how Helmholtz 1888 introduced what Lorenz called "turbulent viscosity" but I would call "geostrophic viscosity."

[72] J. Thomson,"On the grand currents of atmospheric circulation" *Phil. Trans. Roy. Soc*. 183A (1892): 653-84.

[73] In W.N.Shaw's Preface to E. Gold, *Barometric gradient and the wind force, Report to the Director of the Meteorological Office on the Calculation of Wind Velocity from pressure Distribution*, His Maj. Stat. Off. (London, 1908): 6, he discussed the deflection in terms of "great circle" motion. See also Shaw's preface to J. Fairgrieve,"On the relation between velocity of the grqdient wind and that of the observed wind," *Meteorological Office Geophysical Memoirs* 9 (London, 1914) for a history of the geostrophic wind concept. Shaw's incomplete mathematical understanding of the Coriolis Force is reflected in his statement that $-2m\Omega\times V_r$ is only an "approximation." The British at this time called the geostrophic wind the "gradient wind."

[74] L.F.Richardson,"The geostrophic wind," Unpublished notes, see catalogue of the papers and correspondence of Lewis Fry Richardson deposited in Cambridge University Library, Compiled by T.E.Powell and P.Harper, National cataloguing Unit for the Archives of Contemporary Science, University of Bath 1993. See also A.Persson,"Back to basics: Coriolis: Part 2 – The Coriolis force according to Coriolis," *Weather* 55 (2000): 187.

[75] W.M. Davis (1893): 103; H.W.Clough,"The Principle of the Conservation of Angular Momentum as applied to Atmospheric Motions," *Mon. Wea. Rev.*, (1920): 462-65; C.F.Marvin,"The law of the Geoidal Slope and fallacies in Dynamic Meteorology," *Mon. Wea. Rev.* (1920): 565-82.

[76] D. Brunt, *Physical and Dynamical Meteorology* (Cambridge Univ. Press, 1934, 1944), 392-393, esp. 404-405.