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The eighteenth century

INTRODUCTION

Shortly before his death, in 1727, Isaac Newton said:

'I do not know what I may appear to the world, but to myself I seem to have been only like a boy playing on the sea shore, and diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, while the great ocean of truth lay all undiscovered before me.'

Newton's observation was very valid so far as hydrology is concerned. The development of the subject, even at the beginning of the eighteenth century, was extremely modest, and very few fundamental principles have been realized, let alone universally accepted. The establishment of the learned societies in England, France, and Italy during the latter half of the seventeenth century provided the impetus necessary for the rapid development of natural and physical sciences. The downfall of the old masters was rapidly nearing completion. The motto of the Royal Society of London was *Nullius in verba* (on the words of no man). According to Cardwell:

'Although the scientific achievements of the 18th century were substantial, the technological triumphs were of at least equal interest. The men associated with these triumphs, men like Newcomen, Smeaton, Watt, Wedgewood, etc., were scientific technologists, capable of using scientific method and knowledge in their practical work and often, in return, making contribution to 'pure' science. The rise of these scientific engineers was, paradoxically unaccompanied by a systematic development of applied science.'[1]

It was the same with hydrology.

ANTONIO VALLISNIERI AND MARQUIS POLENI

One of the leading figures of hydrology of this period was Antonio Vallisnieri (1661–1730), President of the University of Padua, who published a treatise[2] in 1715 on the origin of rivers, based main only on his personal observations in the Alps. He was aware of the works of Perrault and Mariotte. He observed the mountain ranges from which many of the Italian rivers originated in order to verify the pluvial theory. He found no indication of sea-water being forced out of the mountain tops, instead he found water always trickling down the slopes. He saw the continuous presence of snow and ice high up in the mountain ranges, and reasoned that its melting along with rainfall, provided the necessary supply of water to all springs and rivers.

He was surprised to find that very few small springs originated from the extensive snowfields of the Pellegrino Alps. The local shepherds, however, showed him the reason for the anomaly. The water from the constant reservoir of melting snow travelled downward, towards Modena, through hidden subterranean channels. He quoted Dante to express his feelings after the discovery of that phenomenon: 'Like a man who when in doubt is reassured, and whose fear changes into comfort because the truth was now revealed to him.'[3] He was quick to realize this was the source of the artesian wells of Modena about which there had been considerable speculations in the past. The reason was quite simple. The subterranean streams, originating from the Pellegrino Alps, passed below Modena towards Bologna. They obviously flowed under great pressure, and when a well was sunk in Modena (see Ramazzini's description in chapter 9), water gushed up to the surface and formed the artesian wells. These explanations of the source of artesian water and their mechanism were even better than those of Ramazzini.

Vallisnieri's book was illustrated by six geological sections (figure 1) which showed the structures of certain mountain ranges in Germany and Switzerland. They were drawn by the naturalist Scheuchzer, who explored the Alps between 1702 and 1711, and according to Adams, they were among the earliest geological sections ever drawn. But the Italian's concepts did not go unchallenged. Gualtieri (and some others) violently disagreed with this heretic who dared to dispute the Holy Writ. In the second edition of Vallisnieri's book were discussions of his work by various Italian authors.

Another Italian of note during the early eighteenth century period was Marquis Giovanni Poleni (1683–1761). Born in Venice, he had the distinction of having become a professor of astronomy at the early age of twenty-six at the University of Padua. Later he became a professor of physics and finally a professor of mathematics at the same university. He also served as a consultant in the field of flood control and water-supply engineering. In his treatise,[4] published in 1717, he analysed the flow of water through a rectangular opening which extended to the free surface. Assuming a parabolic velocity distribution curve, he obtained the rate of discharge per unit width as:

$$Q = \frac{2}{3} hb \sqrt{f}$$

where, h and b = depth and breadth of opening, and f = velocity function ($2gh$).

Later the analogy was extended to flow over sharp crested weirs.
The resulting equation,

$$Q = \frac{2}{3} Cb \sqrt{2g}\, h^{3/2},$$

was named after Poleni.

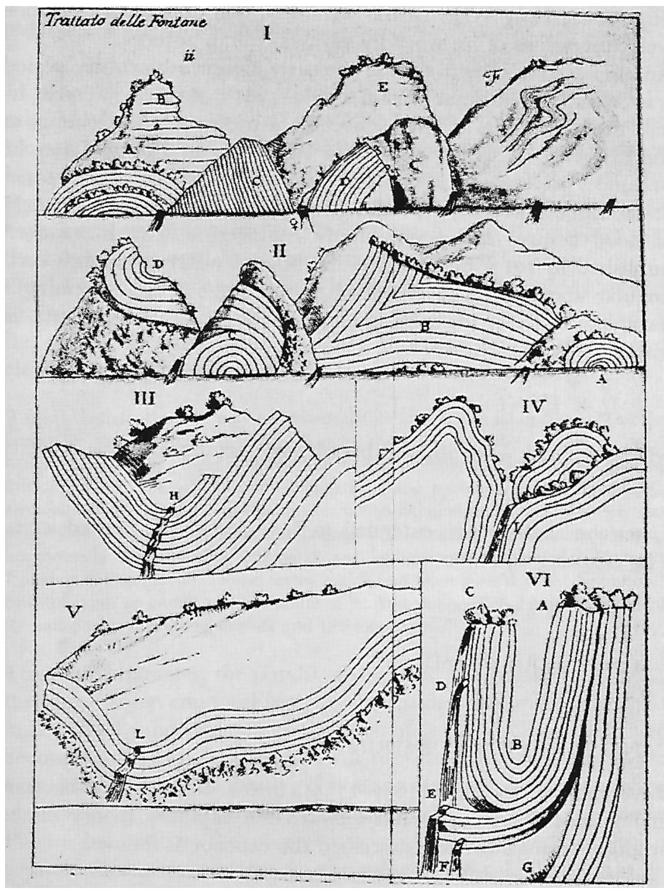

Figure 1. Geological sections of certain mountains in Switzerland and Germany as drawn by Scheuchzer.

CAPILLARY THEORY OF SPRINGS

Reverend W. Derham (1657–1735), in his book *Physico-theology*, first published in 1713,[5] put forward the capillary theory of the origin of springs. Switzer describes the concept as follows:

‘As to the manner how waters are raised up into mountains, and other high lands, and which has all along puzzled so many great men (Mr. Derham says) may be conceived by an easy and natural representation, made by putting a little heap of sand or ashes, or a little loaf of bread, into a basin of water, where the sand will represent the dry land, or an island, and the bason of water the sea about it; and as the water in the bason rises up to or near the tops of the heap in it, so does the water of the sea, lakes, etc. rise in hills: which case he takes to be the same with the rise of liquids in capillary tubes, or between contiguous plains or in a tube fill’d with ashes. . .’[6]

Switzer fully agreed with Derham’s concept and opined that the origin of streams and rivers cannot be entirely due to precipitation. He contended that as Derham’s idea was based on his own meteorological observations (see chapter 11), they were very exact, and hence, beyond any dispute. Had either of them made a simple experiment to determine the maximum height of capillary rise, their opinions would have changed soon enough.[7]

One of the major chapters of the book *Le spectacle de la nature* by N. A. Pluche (1688–1761), published in 1732, was devoted entirely to the origin of springs. The capillary theory was put forward very clearly and concisely by one of the characters of the book:

‘I firmly believe that the sea-water deposits its salt on the sands below, and that it rises by little and little, distilling through the sands, and the pores of the earth, which have such a power of attraction as is not easily accounted for; and that not only sand, but other earthy bodies have the power of attracting water, I am well assur’d of from an observation which occur’d to me but this very day. When I threw a lump of sugar into a small dish of coffee, I found that the water immediately ascended thro' the sugar, and lay upon the surface of it. Yesterday I observed, likewise, that some water which had been pour’d at the bottom of a heap of sand, ascended to the middle of it. And the case, as I take it, is exactly the same with respect to the sea and the mountains.’[7]

The main character, the pundit of the book, vigorously opposed the theory on three counts. Firstly, water cannot rise more than 32 ft in dry sand, and even then that height is very seldom achieved. Secondly, the growth of algae will prevent the passage of water after some time, and finally, if it was true, sea-water, due to the same reason, would saturate all the plains adjoining the coast. Pluche believed in the pluvial origin of springs, and firmly discounted Descartes’ concept on the subject. He calculated that if a cu. ft of sea-water contained only one pound of salt instead of the usual two, the daily flow of the river Seine alone (288 million cu. ft, as calculated by Mariotte) will deposit 288 million pounds of salt every day. Obviously, the quantity of salt that would be deposited by all the rivers of the world would be too vast for the theory to be true.

VELOCITY DETERMINATION BY THE PITOT TUBE

Henry De Pitot (1695–l771), born in Aramon in south-western France, was a student of the well-known scientist Réaumur at Paris. He became the superintendent of the Canal du Midi in his native province of Languedoc, in 1740, and was concerned with the construction of various flood control works, bridges, and aqueducts, and drainage of marsh lands. His greatest claim to fame, however, rests primarily on the invention of a very simple device, presently known as the Pitot tube.

De Pitot, in his papers[8,9] of 1732, discussed the importance of velocity distribution in rivers, and also reviewed the existing state of knowledge on the subject. He outlined the two theories on the variation of velocities with depths, and preferred the concept that velocity at the bottom of a river would be less than at the top because of the frictional resistance. He did not favour the use of floats to estimate velocities as the method was inaccurate on several counts. Firstly, a wax sphere is not always visible, and if a piece of wood, large enough to be kept in sight, is used as a float, it would encounter air currents which were likely to introduce errors. Secondly, repeated experiments conducted within the same stretch of a river would give different results as floats do not travel along a fixed course. Thirdly, it is almost impossible to measure correctly the distance travelled by a float during a certain time. Finally, it only measures surface velocities, and thus, velocities at different depths cannot be determined by this method. All these problems could be surmounted easily with his new instrument which had the added advantage that its operation was as simple as 'plunging a stick into water'.

De Pitot's 'machine' consisted of two parallel tubes, one straight and the other bent through 90° for a short length at the lower end (figure 2), mounted on a wooden frame having a scale. The instrument is immersed in water to the desired depth with the bent tube facing into the current. In still water, the levels of water in both tubes will be identical but in flowing water a difference of elevations would occur, the extent of which depended on the velocity. De Pitot was excited about his new instrument:

'The idea of this machine is so simple and so natural, that the moment it occurred to me, I ran immediately to the river bank to make a first experiment with a simple glass tube, and the result confirmed completely my conviction. After this first experiment, I could not imagine how so simple and useful thing could have escaped so many skilled people who have written and experimented on the motion of water.'[10]

The new instrument, conceived by theoretical analysis, was an extraordinary case wherein two erroneous concepts were used to arrive at a correct solution. De Pitot's explanations of efflux and resistance laws were as follows:

'There is no one with a slight knowledge of the theory of the motion of water who will not conceive immediately the effect of this machine; because, according to the first principles of this science, one must consider the velocity of flowing waters as a velocity obtained from a fall of a certain height, and that if the water moves upward with an acquired velocity, it will rise exactly to the same height. . .'
'Furthermore, the force of impulse of water due to its velocity is equal to the weight of column of water, which has for the base the area of the surface on which the water is impinging and for height that from which the water must have fallen in order to acquire that velocity. Thus the water must rise

in the tube of our machine due to the force of a current to exactly the same height from which it must have fallen to form this current.'[8]

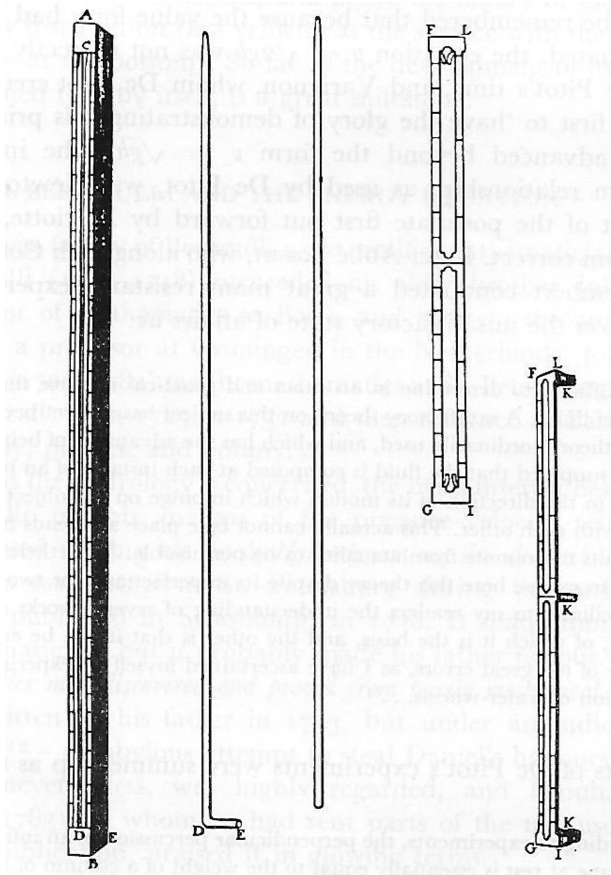

Figure 2. De Pitot's drawings of his tube.

It should be remembered that because the value for g had not yet been evaluated, the equation $v = \sqrt{2gh}$ was not correctly proved during De Pitot's time, and Varignon, whom De Pitot credited being the first to 'have the glory of demonstrating this principle', had not advanced beyond the form $v = \sqrt{gh}$. The impulse momentum relationship, as used by De Pitot, was Newton's improvement of the postulate first put forward by Mariotte, but it was far from correct. Later Abbé Bossut, who along with Condorcet and d'Alembert conducted a great many resistance experiments, remarked on the unsatisfactory state of affairs as:

'It is very difficult to determine in an exact and practical manner the laws of the impact of fluids. A satisfactory theory on this subject has not yet been found. In the one [theory] ordinarily used, and which has the advantage of being rather simple, it is supposed that the fluid is composed at each instant, of an infinity of parallel jets in the direction of its motion which impinge, on the object without interfering with each other. This actually cannot take place and leads in certain cases to results too remote from actuality to be permissible. Nevertheless I have undertaken to expose here this theory despite its imperfections, for two reasons: one is to facilitate to my readers the understanding of several works on naval architecture, of which it is the basis, and the other is that it can be employed, without fear of too great errors, as I have ascertained myself by experiments, in the calculation of water-wheels. . .'[11]

The results of De Pitot's experiments were summed up as follows:

'Now, according to experiments, the perpendicular percussion of an infinite fluid against a plane at rest is essentially equal to the weight of a column of this fluid, which has for base the surface impinged upon and for height the height of the velocity with which this impact takes place. Thus if P is this percussion, s^2 the surface, h the height due to the velocity, and w the specific weight of the fluid, we have approximately

$$P = ws^2h,$$

h can be determined from the laws of fall of heavy bodies.'[11]

Even accounting for the fact that De Pitot's understanding of the flow process was imperfect the new instrument was a splendid invention. Through its use it became possible to determine velocities in a river at various depths, and prove the fallacy of the parabolic velocity distribution (less velocity at the surface with the maximum velocity at the bottom). So far as the development of hydrology is concerned this, by itself, is a great milestone.

DANIEL BERNOULLI AND THE ENERGY EQUATION

The Swiss family of Bernoulli were prolific mathematicians. Johann Bernoulli (1667–1748) succeeded his elder brother Jakob as the professor of mathematics at Basle, and prior to the appointment, he was a professor at Groningen in the Netherlands. Johann's son Daniel (1700–1782) taught mathematics at St. Petersburg (presently Leningrad) from 1725 to 1732 and then returned to Basle to teach anatomy, physics, and botany.

So far as the hydrology is concerned, the main interest lies in Daniel Bernoulli and his analysis of the pressure-velocity relationships. He started writing his book *Hydrodynamica, sive de viribus et motibus fluidorum commentarii* in St. Petersburg during the early thirties. It was published in Strasbourg, in 1738. It made him a leading mathematician, but it probably made his father jealous. The book *Hydraulics now discovered and proved from purely mechanical foundations* was written by his father in 1743, but under an indicated date of 1732[12] – an obvious attempt to steal Daniel's honours. Johann's book, nevertheless, was highly regarded, and Leonhard Euler (1707–1783), to whom he had sent parts of the manuscript prior to its publication, praised it in glowing terms.

Daniel Bernoulli stated the principle behind his analysis as follows:

‘But we must give an account of the principles which we have so often mentioned. Of first importance is ‘tire conservation of live forces’, that is, in my notion, the ‘equality between the actual descent and the potential ascent’: I will use this last notation because, though it has the same significance as the first, it is found that it is perhaps less shocking for certain philosophers, who get excites at the mere name of ‘live forces’.

Like his predecessors, Huygens and Leibniz, he assumed that the sum of potential and kinetic energies of a freely falling body is constant (the terms potential and kinetic energies had not yet been coined).

The main goal of Bernoulli was to establish the relationship between pressure and velocity. He said of his theory that it was new as ‘it considers at the same time the pressure and the motion (velocity) of fluids’. It is difficult to say who first tried to solve the pressure-velocity relationship, but certainly Bernoulli was one of the first, even though he was unable to derive a general expression. Instead, he solved some special cases.

The ‘Bernoulli equation’, as it is internationally known today, was not his work. His analysis included, as previously indicated, only the conservation of summation of potential and kinetic energies, and the effect of pressure was not embodied. The Bernoulli equation,

$$\frac{v^2}{2g} + \frac{P}{\gamma} + z = \text{constant},$$

is basically the principle of conservation of energy, and is a very useful integral form of Euler’s equation of motion. The question at once arises as to whether it is fair for Bernoulli to be credited with a principle that he did not propound. The answer is: it may not be wholly fair, but since he was one of the pioneers who directed a much-needed attention to the pressure-velocity relationship, the honour bestowed on him by later investigators is largely justified.

CALCULATION OF DISCHARGE BY CHÉZY AND DU BUAT

Antoine Chézy (1718–1798; figure 3), born at Châlons-sur-Marne, studied and later taught for some time at the local parochial school. He entered what was later named Ecole des Ponts et Chaussées, in 1748, and graduated with honours. Later he joined the Ecole (whose first director was Perronet) and retired, a very poor man, in 1790. Through the efforts of Baron Riche de Prony, one of his former pupils, he was appointed the director of the Ecole in 1797, only a year before his death.

Figure 3. Antoine Chézy (by courtesy of Ecole des Ponts et Chaussées).

The water supply of Paris was not in a very efficient condition during the latter half of the eighteenth century. To alleviate that circumstance it was recommended by a committee that additional water should be brought from the river Yvette. The city administrators, in 1768, entrusted Perronet and Chézy with the design aspects of the project. Chézy was to determine the cross-section of the channel and its corresponding discharge. In the absence of any acceptable methodology, he had to carry out his own investigations. His final recommendations were handed over to Perronet. The Chézy formula, according to Prony, was established in 1775. The original report on the Canal de l'Yvette, however, was unavailable to him, and it was not until 1897, that the American civil engineer Clemens Herschel, found it among the files of the Ponts et Chaussées. He later had it published.[14]

The following is a translation of the relevant passages that established what is now the famous Chézy formula:

'When we have a flow of water to convey, either to procure some at a place where there is none, or to drain a territory which has too much of it, we should always cause a maximum quantity to flow with the least possible slope.

After having designed a ditch or channel, and having adjusted and regulated its slope, it is very interesting to know if the section of this channel will be sufficient to conduct the water which is to flow in it. To know this, it is necessary to know the speed which the water will have in the ditch, which we will suppose to have a uniform slope.

This is not now a question of initial or momentary velocity, which may be very great if it is causes by a head of water, or very little at first, if it is caused by no other force than that of gravity and the slope of the ditch. Whatever be the initial velocity, it will diminish or augment quite rapidly, and will become that uniform and constant velocity which is due to the slope of the ditch and to gravity, whose effect is impaired by the resistance of friction against the sides of the ditch. It is this velocity which we are now to learn, at least approximately. The question thus proposed presents its own solution, for it is evident that the velocity due to gravity alone, which acts continuously (abstracted from the velocity which may have come from any other cause, and which, being dissipated, no longer concerns the question), this velocity due to gravity is only uniform when it is no longer accelerated, and gravity does not cease to be accelerated, except when its action upon the water is equal to the resistance occasioned by the wetted perimeter of the ditch, but its resistance is as the square of the velocity, on account of the number and force of the particles moving in a given time; it is also as the length of the wetted perimeter of the ditch. The resistance of the air against the surface of the water may he neglected.

If we call *V* velocity, and the wetted perimeter *P*, the resistance of friction will therefore be as *VVP*.

On the other hand, the effect of gravity is as the area of the section of the flowing water and as the slope of the ditch, or as the height which it falls for each toise (6.4 ft) of length.

Calling now the area of the section *a*, and the slope of the ditch *h*, the effect of the gravity will be as *ah*.

This granted, if by a good observation one knew the slope of a ditch … … … … … …	H
the area of the section of the flowing water … … … … … … … … … … … … …	A
its velocity …	V
and part of the perimeter of the section of the flowing water touching the confines of the ditch …	P
it would be easy to find the velocity … … … … … … … … … … … … … … …	v
of the flowing water of another ditch of which one would know the slope … … …	h
the area of the section … … … … … … … … … … … … … … … … … … …	a
and the quoted portion of perimeter … … … … … … … … … … … … … … …	p

for one would have the proportion

$$VVP : AH :: vvp : ah$$

Whence, … … … … … …. … …… … ……… … … … … … … $VVP \cdot ah = vvp \cdot AH$

$$v = V \sqrt{\frac{a\,h\,P}{AHp}} .$$’[14]

To prove the formula thus derived, Chézy conducted two experiments in the Courpalet canal in the forest of Orleans and in the river Seine, during the months of September and October of 1769. Both the stretches chosen were ‘as straight and as uniform as possible’, and measurements were taken on a calm day. Velocities were determined by the float method, using a ball of wax, and the surface velocity was assumed to be the mean velocity. Details of his investigation are shown in the following table.

TABLE
Details of Chézy experiments.

	Courpalet canal	River Seine
Date of experiment	September 23, 1769	October 7, 1769
Velocity	0.468 f.p.s.	2.576 f.p.s.
Slope	0.07224 per 1000	0.1157 per 1000
Area	7.265 sq. ft	3066 sq. ft
Wetted perimeter	7.679 ft	338.988 ft

If the proposed Canal de l’Yvette had a trapezoidal cross-section (5 ft width at bottom and 6 ft at top), slope of 0.2083 ft per 1000 ft, and 5 ft depth of flow at full-supply discharge, the velocities, as calculated by his formula, would be as follows:

from 1st experiment – 1.14 f.p.s (C = 56.5),
2nd experiment – 1.599 f.p.s (C = 79.3).

Chézy decided that the first result was too little and the second was too large because of inequalities in the river slope as well as the cross-sectional areas. He reasoned that the velocity in the proposed canal would be slightly more than 1 f.p.s. with a discharge of 33.3 cu. sec, an amount which is more than sufficient if the desired flow is around 17 to 24 cu. Sec.

If from the two experiments, Chézy had decided to calculate the velocity of the Seine from that of the Courpalet canal, he would have found it to be 1.83 f.p.s instead of 2.576 f.p.s. The reason of the discrepancy was that the equation was derived by simple comparison of flow conditions in the two streams, and hence, to obtain good results, they should have very similar characteristics.

In a later memorandum (dated 1776), he simplified[15] his equation to facilitate quick calculations as follows:

$$v = 272\sqrt{\frac{ah}{p}} \quad \text{in French units}$$

or

$$v = 57.3\sqrt{rs} \quad \text{in English units.}$$

However, he realized that the numerical factor was not constant in every case – in fact, his own calculations showed it to vary from one river to another.

Unfortunately Perronet did not include Chézy's analysis in his report on the Canal de l'Yvette even though he used his results. Thus, the extraordinary piece of work gathered dust in the archives until it was mentioned by Girard in one of his memoirs, in 1803, and by Prony a year later in another memoir. Strangely enough, it attracted more attention in Germany than in France, and it was eventually published by Herschel, in 1897 in the United States!

Pierre Louis Georges Du Buat (1738–1809), a contemporary of Chézy, was born in Tortizambert in Normandy. He became a count on the death of his older brother. The title, however, did him no good as it was only two years before the French revolution, and he had to leave his native land in 1793. His property was confiscated, and when he returned in 1802, he could recover only a small portion of it.

Du Buat was educated in Paris, and worked as a military engineer during the period of 1761 to 1791. Over a period of years, he carried out a number of experiments under the sponsorship of the French Government. The first edition of his work, entitled *Principes d'hydraulique vérifié par un grand nombre d'expériences, faites par ordre de Gouvernment*, was published in 1779. The book was enlarged into two volumes in 1786, and was subsequently published posthumously (in 1816) in three volumes. His work was considered so valuable that it was translated into German twice, as well as into English, which version is said to have been praised by no less a person than George Washington.[16]

Du Buat was evidently not happy with the existing state of the science of hydrometry as he said in the preface of the second edition of his work:

'We are still, after so many centuries, in almost absolute ignorance of the true laws to which the motion of water is subjected: hardly after 150 years of experiment have we discovered the quantity and the velocity of the flow of water from any orifice whatever. All which relates to the uniform motion of the stream which water the surface of the earth is unknown to us, and to have any idea of what we know it is only necessary to glance at what we are ignorant of.

To estimate the velocity of a river of which one knows the width, the depth, and slope; to determine to what height it will rise if it receives another river in its bed; to predict how much it will fall if one diverts water from it; to establish the proper slope of an aqueduct to maintain a given velocity, or the proper capacity of the bed to deliver to a city at a given slope the quantity of water which will satisfy its needs; to lay out the contours of a river in such a manner that it will not work to change the bed in which one had confined it; to calculate the yield of a pipe of which the length, the diameter, and the head are given; to determine how much a bridge, a dam, or a gate will raise the level of a river; to indicate to what distance backwater will be appreciable, and to foretell whether the country will be subject to inundation; to calculate the length and the dimensions of a canal intended to drain marshes long lost to agriculture; to assign the most effective form to the entrances of canals, and to the confluences or mouths of rivers; to determine the most advantageous shape to give to boats or ships to cut the water with the least effort; to calculate in particular the force necessary to move a body which floats on the water. All these questions, and infinitely many others of the same sort, are still unsolvable: who would believe it? ... Everybody reasons about Hydraulics, but there are few people

who understand it . . . For lack of principles, one adopts projects of which the cost is only too real but of which the success is ephemeral; one carries out projects for which the goal is not attained; one charges the state, the provinces, and the communities with considerable, costs, without fruit, often with loss; or at least there is no proportion between the cost and the advantages which result therefrom.
The cause of such a great evil, I repeat, is the uncertainty of the principles, the falsity of theory which is contradicted by experience, the paucity of observations made up till now, and the difficulty of making them well.[17]

Du Buat reasoned that in case of uniform flow, the accelerative force causing the motion should be equal to the sum of the resistances encountered due to viscosity as well as boundary friction.[18,19] The same principle, as already has been noted, was advanced by Guglielmini, but the Frenchman was the first to express it analytically. The principles on which the formulae of Chézy or Du Buat were based, were very nearly identical. Du Buat reasoned that the resistance is proportional to the square of the velocity or equal to

$\frac{V^2}{m}$, where *m* is the constant of proportionality; which should be

equal to the gravitational force in the direction of flow, i.e.

$\frac{V^2}{m} = gS$, or $V^2 = mgS$.

This, obviously, is a form of the Chézy equation. He realized that the term *m* will be constant only for the same section of the canal and it should vary, in different sections, with the first power of the hydraulic radius R or A/P. He tried to fit an equation of the type $V = \sqrt{gRS}$ to his numerous experimental data, and suggested the following cumbersome equation:

$$V = \frac{\sqrt{243.7\,g}\ \ (\sqrt{R} - 0.1)}{\sqrt{1/S} - \log\sqrt{1/S} + 1.6} - 0.3\,(\sqrt{R} - 0.1)\ \text{pouces/sec}$$

$$= \frac{297\ (\sqrt{R} - 0.1)}{\sqrt{1/S} - \log\sqrt{1/S} + 1.6} - 0.3\,(\sqrt{R} - 0.1).$$

It is to be noted that the equation does not take into account the surface resistance at all; not because Du Buat neglected it, but because of his abstract conception that a thin layer of water adhered to the boundaries, and the only effective friction taking place was between the fluid molecules themselves. He said:

'Considering how the water itself prepares the surface over which it flows, one can see that the different boundary materials will not have an appreciable influence on the resistance. We have not, as a matter of fact, found any variation in the friction which one could attribute to this cause, in the different cases when water flowed over glass, lead, iron, wood or various kinds of earth.'[17]

If this was the beginning of the present day boundary layer theory, Du Buat was not aware of that fact!

The impact of his equation on the eighteenth and the nineteenth century hydrologists was enormous. It gave excellent results so long as it was used within the range of his experiments, and according to Dugas, it was the best algebraic expression available for the next seventy years – within its limits.[20] However, it failed when the conditions were beyond the range of his experiments, and thus, could not be the all-purpose equation its originator wanted it to be. Besides, the expression as it stood was too cumbersome to use, and even though it was arrived at independently, its introduction came four years after Chézy's pioneer work.

PAOLO FRISI

Paolo Frisi (1727–1784) was born in Milan, and his early studies were directed by the Church of St. Barnabas in his native city. He became a professor of mathematics at the University of Milan, and was a member of the most of the major scientific societies of his time. His patrons included Maria Theresa, Catherina II, and Joseph II. His reputation in Italy, in the field of hydrometry, was so high that plans for all major works executed during his time were submitted to him for his comments. His book *Del modo di regolare i fiumi e i torrenti*[21,22] was published in 1762, and was well received. It was considered to be of such important that the British government paid for all the expenses incurred for its English translation so that it could be made available to the British engineers engaged in irrigation and river regulation works in India.

The book admirably discusses the previous works in the fields of hydrometry and open channel flow by the Italian school – Castelli, Viviani, Zendrini, Manfredi, Poleni, Grandi, and especially Guglielmini.

Frisi was confident that his opinion on the origin of rivers and springs was correct:

'In short, all the phenomena of floods; the laws, by which they increase and diminish; the substance, which they sweep along with them; all clearly point out, that they derive their origin from the rains that fall on the declivities of the mountains and into the beds of the rivers. Seeing then, the greatest quantity of water, as has been before observed, is brought down by the rivers in the time of their high and moderate floods, it would be unreasonable not to admit that the waters, when low, have the same origin.'[22]

Frisi was wrong about the velocity distribution pattern in an open channel. However, he was in good company so far as the parabolic distribution law was concerned, with people like Zendrini (1679–1747), Lecchi (1702–1776), Michelotti (1710–1777), and Lorgna (1730–1796). He discussed at length the relevant passages from Zendrini's[23] and Father Grandi's[24] works, and erroneously concluded that:

'It must appear to be sufficiently ascertained that the velocities of water, though arising from different causes, either from the free fall, or from the pressure of the higher waters, have only one law, and are proportional to the square roots of the heights, either actual or effective; that is, they are in proportion to the square roots of the actual and absolute heights of the sections, when the surface of the water has no perceptible motion; and, when the motion of the surface is perceptible, they are proportional to the square roots of the actual heights augmented by the height due to the velocity of the surface.'[25]

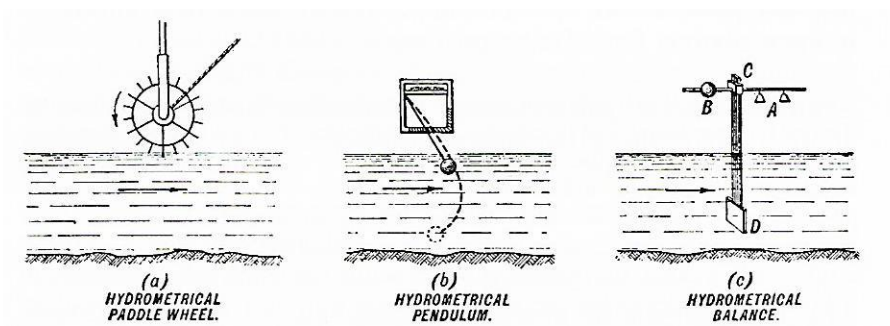

Figure 4. Velocity measurement devices (after Leliavsky).

Leliavsky[26,27] has suggested that the origin of the parabolic velocity distribution lies in the methods used for measuring velocities. The 'hydrometric pendulum' (figure 4) evaluated velocities from the angle of inclination of the pendulum, i.e., greater the angle of inclination, higher is the velocity. Such a device, according to Leliavsky, will overestimate velocities at lower levels as the curvature of the cord is neglected. He further stated that the parabolic velocity distribution theory was so firmly established by that time that any reading which did not conform to the theory was rejected as an error of measurement, an unfortunate human tendency. But this argument is not valid as under normal streamflow conditions, the suspension cord could assume such a backward curve against the current (as it appears in Leliavsky's drawings) under one condition only, namely, for just a brief instant during which the operator might release a few extra feet of line. But during the next instant, the line would, in its entirety, slope in the downstream direction. The very lowermost end of it, could, if the water at that depth had a zero velocity, approach the vertical, but that is the closest it could normally come to any upstream direction.

Frisi also discussed the use of floats and paddle wheels (figure 4a) for measuring surface velocities. The number of revolutions of the wheel, whose vanes touched the surface of the stream, per unit time was taken to be an indication of the velocity.[28] Leliavsky[26,27] also suggested that use of such a device would result in underestimation of velocities because of the effect of water lifted by the vanes. Probably Frisi's greatest contribution to hydrologic knowledge was his contention about the limitations of purely theoretical approach to open channel flow. He pointed out:

'One single reflection is sufficient to show that all hydraulic problems are beyond the reach of geometry, and of calculus. The difficulty of all problems is increased in proportion to the number of the conditions, of the cases, and of the differences which are stated. Thus, mechanical problems become so much the more complicated as the number of bodies, whose motions are sought, and which act in any way on each other, is augmented ... Then, in a fluid mass, which moves in a tube, or in a canal, the number of bodies acting together is infinite; whence it follows, that to determine the motion of each body is a problem depending upon an infinity of equations, and which it is of course beyond all the powers of algebra to reach.'[29]

Thus, concluded Frisi, hydrometry is a branch of physics rather than of mathematics,[30] and that idea was later echoed and re-echoed from various parts of the world.

Paolo Frisi did not contribute any significant original concepts to the development of hydrology, but his work was an excellent compilation on the subject, and it did much to disseminate the available knowledge all over the world, which, in itself, is a praiseworthy achievement.

During the last quarter of the eighteenth century, the prestige of the Italian school gradually dwindled. For some time, previous to the above-mentioned period, no significant new ideas were put forward, even though a series of treatises on rivers and canals – all of them discussing the existing state of hydrologic knowledge – were printed.

GIOVANNI BATTISTA VENTURI

Giovanni Battista Venturi (1746–1822) was a professor of Natural Philosophy at the University of Modena and later at Pavia.[31] He was a noted Italian civil engineer, builder of bridges,[32] and obviously a highly talented experimental hydraulician. His experiments were carried out in the physical laboratory of the University, of Modena over a long period of time, and the results of his investigations were later collected and published at Paris, in 1797, as *Recherches expérimentales sur le principe de la communication latérale du mouvement dans les fluides*.[33]

Venturi experimented with various types of constrictions and expansions in open channels as well as with orifices and short tubes of various shapes.[34] Clemens Herschel was primarily instrumental in making Venturi well-known by associating his name with a particular type of boundary-profile now universally referred to as the Venturi flume. But, it must be realized that Venturi was by no means the first man to experiment with conduits of that particular shape – many, notably Bernoulli and Borda, had been interested in them before him.

Venturi's book is a straightforward report of his investigations, and, one immediately becomes aware of its lack of analyses or computations. He discussed the effect of eddies in open channels and pointed out similar phenomena can be seen in the motion of the atmosphere. He showed that formation of eddies is a cause of retardation of the, current and hence of the discharge. One of the interesting ideas put forward by Venturi was the use of the principle of the hydraulic jump for drainage problems as shown in figure 5. His explanation was:

'water at F tends to return and descend along FK; but the current, by its lateral action, constantly carries it away, and does not permit it to slide down to K. If an opening G be made in the lateral sides of the tank, the waters from lands lower than the current of the inferior stream FL may be drained off.'[35]

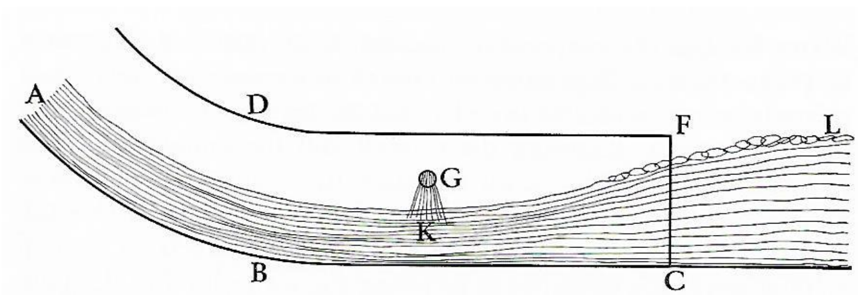

Figure 5. Venturi's use of the principle of hydraulic jump for drainage problems.

The principle was used by Venturi in an actual case, and, according to him, it was very successful.

MEASUREMENTS OF EVAPORATION

After Halley, the first noteworthy measurements of evaporation were taken by Dobson,[36,37] who maintained a record of the rainfall, evaporation, and temperature for four consecutive years, from 1772 to 1775. Dobson used a site overlooking Liverpool, 75 ft above sea level, on rising ground having free exposure to sun, wind, and rain, with the measurements being taken at the middle of a grass-covered plot. He used:

'two well-varnished tin vessels; one of which was to serve the purpose of rain-gauge; the other to be employed as my evaporating vessel. The evaporating vessel was cylindrical, twelve inches in diameter and six inches deep. The rain-gauge consisted of a funnel twelve inches likewise in diameter, the lower end of which was received into the mouth of a large stone-bottle; and to prevent any evaporation from the bottle, the pipe of the funnel was stopped with grooved cork.'[36]

Water level in the evaporation measurement vessel was kept at 2 in. below the rim. Depending on rainfall or evaporation, water was either taken out or added in order that the level in the vessel would remain constant. Knowing the rainfall and the amount of water added or removed, he could calculate the monthly evaporation. The temperature was measured every day at 2 p.m. by a thermometer attached to a shaded wall. The main defects of such a method were: the difficulty of restoring the water level in the tank to its exact original height, the precise measurement of water added or removed (including that lost by splashing), and the possibility of water being lost by overflowing.

Rodda[38] has estimated the evaporation of the same period from Dobson's data using the equation:

$E = 0.17T — 7.18$

where, E = monthly pan-evaporation in in.,

T = monthly mean maximum temperature in ºF.

Even allowing for the fact that no factors were included in the equation to compensate for the climatic differences between Wallingford (Rodda) and Liverpool (Dobson), or to account for other controls of evaporation, the values obtained by Dobson seem relatively high. This supposition is substantiated in part by the fact that Pennman[39] estimated the annual average evaporation at Southport to be 26 in.

John Dalton later used Dobson's method to determine evaporation at Kendal for eighty-two days in March, April, May, and June, and found it to be 5.414 in. During the period, the maximum evaporation recorded in a day was little above 0.2 in. Dalton stated that a certain Dr. Hale, from a few experiments conducted, concluded that 6.66 in. of water evaporated annually from 'green ground and moist earth', which according to him 'must be far below truth'. The Bishop of Llandoff had found that:

'in a dry season there evaporated from a grass plot that had been mowed close about 1600 gallons in in acre per day which amounts to 0.07 of an inch in depth; and that after rain the evaporation was considerably more.'[40]

With the help of his friend Thomas Hoyle, Jr., Dalton determined the evaporation at an unspecified site near Manchester from the autumn of 1795. A cylindrical vessel of tinned iron, 10 in. in diameter and 3 ft deep, was used. Two pipes were connected to the vessel – one at the bottom and the other an inch from the top. The vessel was filled for a few in. with gravel and sand and the rest with good fresh soil.

‘It was then put into a hole in the ground and the space around filled up with earth, except on one side, for the convenience of putting bottles to the two pipes; then water was added to sodden the earth, and as much of it as would was suffered to run through without notice, by which the earth might be considered as saturated with water.’[40]

Initially, the soil was kept about the level of the upper pipe for some weeks, but later it was below the pipe so as to preclude any water from flowing down the pipe. Moreover, soil at the top was bare during the first year but was covered with grass for the following two years. A regular record was kept of the quantity of rain water which ran off from the surface of the earth through the upper pipe and also the quantity that percolated through the sample to the bottom pipe. Rainfall during the corresponding time was measured by a cylindrical vessel having the same dimensions as the one used for evaporation measurements. Dalton assumed that:

evaporation — rainfall = quantity of water in the two bottles.

From the experiment conducted Dalton concluded that:
(1) the annual evaporation under the circumstances stated was 25 in., (2) quantity of evaporation increases with the rain but not proportionally, and (3) there is no difference between evaporation from bare earth and vegetating grass.

In a subsequent paper[41] Dalton gave the results of his observations of evaporation from the water surface of a cylindrical vessel 10 in. in diameter during the period 1799–1801.[42] In 1802, he also put forward a generalized theory of vapour pressure[43] which provided an excellent basis to estimate the rate of evaporation from water surfaces. The theory is based on the observation that under given conditions evaporation is proportional to the deficit in vapour pressure. Expressed mathematically, it takes the following form.

$$E = C\,(e_w - e_a),$$

Where

E = rate of evaporation in in. per day,
C = a coefficient (depending on various uncounted factors affecting evaporation),
e_w = maximum vapour pressure (in mercury),
e_a = actual vapour pressure (in mercury).

The method is still extensively used today except for slight modifications which take into account effects due to wind and/or temperature.

The beginning of experimental research in the field of suppression of evaporation by a film of oil was initiated by Benjamin Franklin (1706–1790). In 1765, he conducted experiments on the spreading of oil on water surfaces in a large pond at Clapham Common in England. In a letter to one William Brownrigg he pointed out that if a drop of oil was placed on a horizontal mirror or a highly polished table, the drop remained in its place, whereas:

‘when put on water, it spreads instantly, becoming so thin as to produce prismatic colours, for a considerable space, and beyond them so much thinner as to be invisible, except in its effect of smoothing the waves at a much greater distance.’[44, 45]

Franklin’s main interest seemed to have been the use of oil as a method for wave damping, and from the experiments he concluded that the minimum thickness of the film should be about 25 Å.

OTHER EQUIPMENT AND PROCEDURE DEVELOPMENTS

William Herberden was probably the first to study the variation of rainfall with elevation.[46] His interest was first aroused when he studied the results of observations from two identical rain gauges in London which were placed about a mile apart. He found that the rainfall at one station continually exceeded that of the other – not only every month but also almost every time it rained. He reasoned that ‘this unexpected variation’ must have something to do with elevation because one of them was fixed above all the neighbouring houses whereas the other one was at a considerable lower level. In order to verify his theory, Herberden used two rain gauges – one on the chimney of a house (probably in London) and the other in the garden of the same house. The results confirmed his theory, and he decided to use a third one on the roof of the Westminster Abbey. Observations were taken at monthly intervals for a year, but he was unable to determine the reason for the variation of rainfall. He surmised, erroneously, that:

‘Some hitherto unknown property of electricity is concerned in this phaenomenon. This power has undoubtedly a great share in the descent of rain, which hardly ever happens, if the air and electrical apparatus be sufficiently dry, without manifest signs of electricity in the air.’[46]

Reinhard Woltman (1757–1837), a German engineer, spent nearly all his working life in the Department of Ports and Navigable Waterways in Hannover. In his book *Theorie und Gebrauch des hydrometrischen Flugels*,[47] published in 1790, he describes a spoke vane type current meter (figure 6) with a revolution counter to determine river discharge. For many years after Woltman’s death, every improved version of his current meter was called a ‘Woltman current meter’ out of courtesy to him. Hydrology and hydraulic textbooks and many articles have published drawings of those improved models, and have erroneously describes them as having been designed by Woltman.

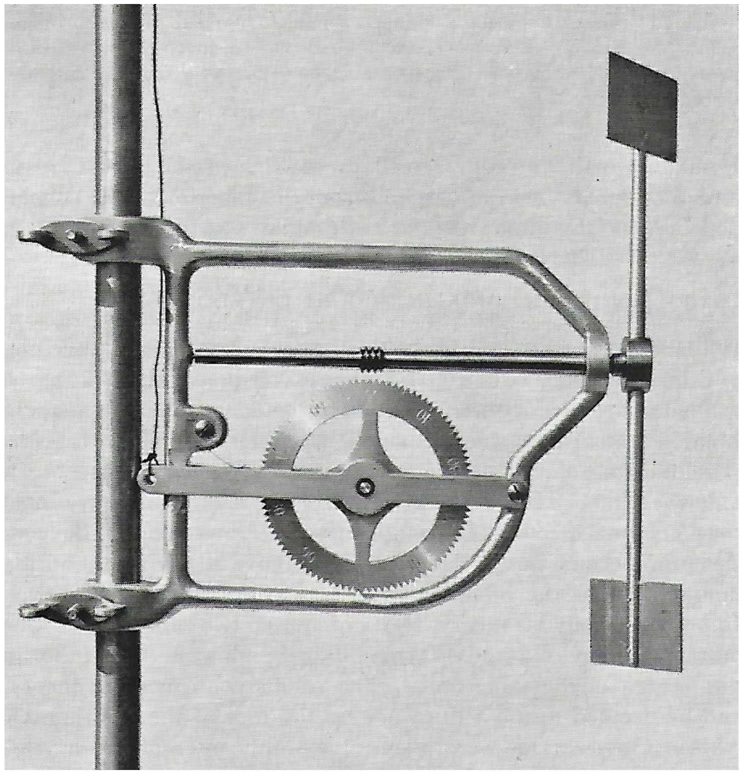

Figure 6. The current meter of Woltman (reconstructed by Arthur H. Frazier).

Extensive experimental works were carried out by Michelotti in Turin and Abbé Bossut (1730–1814) in Paris. Michelotti carried out a series of experiments under the patronage of the King of Sardinia, and his results were published in 1774.[48] Bossut's tests were carried out at the expenses of the French government, and the results were published between 1771 to 1778. Both of them made several experiments on river and canal flow problems.

The concept of the hydrologic cycle was extended by jean-Claude De La Methiere (1743–1817). He explained[49] that the total rainfall is disposed of in three ways: by the first if flows off directly to rivers and canals; by the second it is released through evaporation, transpiration from plants, or moistening of the soil; and by the third it infiltrates to greater depths and provides a source of water for springs.

Probably one of the major contributors from England to water science of the eighteenth century was the Leeds-born John Smeaton (1724–1792), who was responsible for the design of various harbours and drainage works. His main contributions, however, were the tests he made on scale models of water wheels,[50] and windmills. Many treatises and papers were written in the field of open channel flow during the eighteenth century, the more important of which have been discussed in this chapter. Other contributors during this period included Pierre Varignon (1654–1729), Bernard Forest De Belidor (1693–1761), Alexi Claude Clairaut (1713–1765), Jean Le Rond d'Alembert (1717–1783), Jean Charles Borda (1733–1749), and Jean Antoine Fabre (1749–1834) in France; Antonio Lecchi (1702–1776), and Antonio Mario Lorgna (1735–1796) in Italy; Christiaan Brünings (1736–1805) in Holland; and John Theophilus Desaguliers (1683–1744) and James Jurin (1684–1750) in England.

CONCLUSION

The main development concerning hydrology in the eighteenth century was in the field of surface water. Considerable experimental results were obtained particularly by the Italian and French hydrologists. The influence of the Italian school was predominant at the beginning of the century but it gradually dwindled towards the latter half, and its place was taken over by the French school.

The reason seems obvious: the Italian hydrologists were writing and rewriting what had already been said before, and, hence, most of such treatises did not introduce any important fundamental or new concepts.

The new ‘machine’ of De Pitot, although developed on the basis of two erroneous principles, revolutionized the method of measuring velocities in rivers. By its use, the concept of the parabolic distribution of velocity in rivers and canals was proven by actual experiments to be fallacious. This absurd theory had done considerable harm to the natural development of concepts of open channel flow.

Undoubtedly, the most influential man of this period was Du Buat, and his influence extended well into the nineteenth century. Along with Borda, he was one of the first scientists to write the efflux equation correctly as $v = \sqrt{2gh}$. But, Du Buat’s major claim to fame was his experimental work in the field of quantifying discharge. Using the mass of data collected, he produced an algebraic expression for computing the discharge of open channels. The expression obtained was no doubt cumbersome to use, and is only valid within the narrow range of his experiments, but, nevertheless, it was a very significant development of his time. Modern students of hydrology are more familiar with the Chézy equation than with that of Du Buat. Both of them were based on almost identical assumption, but Chézy was the first to produce such a formula. Unfortunately, Du Buat was not aware of Chézy’s contribution, nor as a matter of fact, was anyone else till it was published in the nineteenth century. Chézy’s equation remained in its most primitive form, whereas Du Buat tried to develop a generalized expression which could be applied universally. Furthermore, Chézy’s expression relied on experience for adapting it to specific problems, but the Du Buat formula did not. Looking back, it seems extremely unfair that all students of hydrology or open channel hydraulics should become aware of Chézy’s equation, while very few of them are even aware of Du Buat’s existence!

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