Major Cunningham.

severe test. On the other hand, the placing the axis vertical was certainly wrong, being incompatible with a maximum velocity-line below the surface, a condition the evidence of which was overwhelming.

The only evidence adduced by Mr. Latham as to the convexity or concavity of a river when rising or falling was very indirect; the observed facts might well be due to other causes; for instance, the set of driftwood towards the banks might be due to wind or to surface-currents.

As to evaporation, dryness of the air was probably the most important exciting cause. In northern India the dryness of the air during the hot winds was excessive, the difference between dry and wet bulb thermometers being often 40°; so that evaporation must then be active. The smallness of the evaporation from the canal at Roorkee seemed to be due to the coldness of the water. The use of oil inside an evapometer would be hardly safe, as a little oil would probably escape and form a surface skin over the water, and so reduce the evaporation.

## Correspondence.

Mr. A. Flamant.

Mr. A. Flamant considered the Author's experiments to be extremely important, and that his book,[1] full of practical details of the manner in which he worked, should be read by all those who proposed to undertake similar experiments, or who desired to study the phenomena, still very obscure, which occurred in the flow of water in large open channels. It was for this reason that he had referred to the work in a note inserted in the "Annales des Ponts et Chaussées,"[2] desiring thereby to induce French students of the subject to consult Major Cunningham's book. It was impossible to render too much praise to the care and exactitude exhibited by the Author in his operations, and the remarks that followed referred wholly to questions of theory or of rightful interpretation.

The Author said (chap. v.) that according to theory the interior pressure of flowing water in permanent motion was less than that in the case of still water, and that it diminished with the velocity. Consequently, if a body of still water were connected, by a tube of small aperture, with a running stream, the level of the latter would be higher than that of the still water.

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[1] "Recent Hydraulic Experiments."

[2] "Annales des Ponts et Chaussées," Série 6, Tome 4, 1882, p. 43.

Mr. Flamant. He had proved this by experiment, but the difference of level shown had been very small (about 0·07 foot). Here, Mr. Flamant thought, was an error of interpretation. The tube in communication with the running stream would project from the bank. It would in consequence oblige the particles of water impinging against it to take a curved trajectory instead of preserving their rectilinear direction. In the path of this trajectory, of which the concavity would be turned towards the tube, was developed a centrifugal force tending to throw the particles of water from the tube. It this way a kind of suction was set up in the tube, which would diminish the pressure, and consequently lessen the height of the water in the adjacent reservoir, making the latter less than that of the stream, which indicated the real pressure, except in the immediate neighbourhood of the tube. This effect would be the more marked the greater were the centrifugal force, *i.e.*, the velocity of the stream. The Author had been able to show that the difference in the two levels increased with the speed of stream, but he would not have observed any difference at all if the tube had been placed close to the slope of the stream's bank without projecting into the water.

The same would apply, Mr. Flamant thought, to the Author's statement in chapter viii. that the surface of the water should be convex transversally. The Author endeavoured, without success, to measure this convexity. Nevertheless, as the velocity of the water at the banks was nearly nil, the level should be, according to his theory, sensibly that of still water, and, logically, he should have been able to find the same difference between the level at the banks and in mid-stream as between the latter and the water in the reservoir. But between the water near the banks and in mid-stream there existed free communication, without the intervention of any tubes to interfere with the conditions of flow, so that the pressure was the same at equal depths, *i.e.*, the surface was necessarily horizontal. The instance of convexity of surface noticed by Baumgarten in the case of the Garonne did not correspond to a permanent condition, but to a period of rise in the stream, at the beginning of which water would mount more quickly at the middle than near the banks.

Lastly, in chapter xvii., the Author stated that he had noticed near the banks, and on the surface, a persistent current from the bank towards the middle, which current was greatest inshore, and rapidly diminished as it approached the centre. It seemed to Mr. Flamant that this current towards the middle could only be an illusion. When there was placed quite close to the bank a float, whose dimensions could not be neglected in respect of its distance from the bank, the fluid veins which impinged against it differed in force sufficiently to make the float turn on its axis, and drive it from the shore. This effect would diminish in proportion as the distance from the bank being greater the velocities of the fluid veins became more assimilated. Moreover, a current implied a displacement of liquid; it would therefore be necessary that the water flowing from the bank towards the middle should be replaced by the product of a similar current from the centre towards the bank. The Author thought that there really did exist such a current, for he had noticed that loaded rods placed near the bank were less acted upon than surface-floats, and that they preserved a movement sensibly parallel to the bank. This might be explained by the fact that loaded rods were endowed with a motion comparable to the mean velocity on a vertical, and that all things being equal, this mean velocity varied less than local velocities, and especially than the surface velocity. The Author had himself observed that the curve of mean velocities was often more flat than the transverse curves. Mr. Flamant.

These objections referred only to secondary points; they in nowise vitiated the observations themselves, which had been made with a care and good faith which must be generally acknowledged, neither did they affect the main conclusions arrived at by the Author concerning the discussion of the formulas of mean velocity, and which were amply justified by the observations.

Mr. Gordon. Mr. ROBERT GORDON remarked that the hydraulic researches published by the Author of the Paper at Roorkee, in 1881, formed a most important connecting link between the previous experiments of Messrs. Darcy and Bazin, and others, on small regular channels with uniform flow, and the more numerous experiments on large natural channels with varying flow, of which the elaborate investigations inaugurated on the Mississippi by Messrs. Humphreys and Abbot formed the type, and, perhaps, the most important example. Neither in his aims nor in his results did the Author strive to raise any new problems; but throughout his inquiries, as narrated in his Report, he restricted himself to ascertaining what light the careful and accurate experiments conducted by himself, and involving an immense amount of conscientious labour of the highest class, could give to the problems now perplexing hydraulicians, and to placing on record for students of the science the elaborate and clearly arranged data gathered in his inquiries. The Ganges canal, at the part where the experiments were conducted, reached the magnitude of a river, both in its cross-section

Mr. Gordon. of nearly 200 feet wide by 12 feet deep, and in its discharge of nearly 7,500 cubic feet of water per second. It verged on the conditions of a river also in its varying and irregular flow; while, from the complete control over some of the principal elements influencing the flow, it retained all the advantages of an experimental channel. No person could peruse the Report without recognising its high value as a permanent contribution to the science. It did not mark out a new era, like the works of Guglielmini or Du Buat, or those of Darcy and Bazin, and Humphreys and Abbot; but it consolidated on a firm basis the results acquired up to the present, enabled the deficiencies of the sciences to be seen, and might inaugurate a new departure with definite aims and scope. A brief notice of the present condition of the science, as ascertained by the Author, and generally confirmed by other writers, might be presented. Before doing this two exceptions might be taken to Major Cunningham's work, one relating to the matter, the other to the treatment. These were the only ones that occurred to him, and were suggested with diffidence. First, the Ganges canal itself did not offer, in any of the experiments taken, all the conditions for free flow. At the end of each reach of the canal a permanent weir had been constructed to check the velocity of the water; and on the permanent weir temporary weirs were placed, which had the effect of changing the character of the flow throughout the whole reach of the canal above it. These changed conditions were recognised and fully stated by the Author, but the fact remained that in not a single instance was the discharge in the condition of free flow normal to ordinary rivers, and also to experimental canals. How far the analytical results were changed, or if at all, especially in the relations of the velocities on verticals, and in the comparison of the measured discharges with those given by formulas, it was impossible to say.

The second exception as to treatment referred to the use made of the method of least squares in displaying the results of the velocities on the verticals. Obviously, before it was used at all, some law must be assumed to exist and to be known, or else an arbitrary formula must be chosen, and all that the method of least squares could do was to assign the most accurate indices and coefficients to the factors already assumed in the law or formula. There was, therefore, an objection to its use before the law had been ascertained by which the order of the elements changed. The Author assumed that the order of the velocities on each vertical changed liked that of a parabola, and determined the particular curve in each case. He also assigned an arbitrary weight, Mr. Gordon.

or value, to the velocities according to their distances from the surface, when taken by the double-float, giving the lower velocities a very much inferior value to the upper ones (chap. xi., p. 4). Theoretically he was correct; but his practice appeared exaggerated, and not sufficiently supported by evidence. As the vertical velocity-curve was the most important of all the analytical results yet obtained, it was desirable to preserve it as free from alteration as possible. Probably the future science of hydraulics would take its principal direction from the teachings of this curve.

That the science needed a new direction was evident from the Author's words, where he summed up the results of his comparison of the measured and calculated discharges of the canal. He tested all the best known formulas in ordinary use, and said:—"The general results of trials of these formulas, which are all empirical, shows that at present increased approximation can only be obtained by increased complexity; there is no guide as to the form of such improved approximations, whilst the labour of tentative research is excessive. Until some guide is obtained from a rational theory, it seems to the Author hopeless to attempt further improvement." This statement was but the echo of almost every writer of authority on the subject within the last few lustres. It had been the one leading object of almost every investigator for a long time to, in the first place, improve and facilitate the mode of research, and the means for calculation for practical engineers; but the further the inquiry was pursued, and the more accurate and elaborate the data, the wider were the discrepancies between practice and theory. Indeed, hydraulics, as it existed, was a science without a guarding theory, as the old theories had been entirely discarded by writers of judgment and experience. But there seemed a diffidence, or a reluctance, or a shrinking from the responsibility of originating a new theory, or even of examining tentative theories on the part of the investigators, to whom practical men looked for guidance in developing the resources of the science to the urgent requirements of modern life. The experienced practician by tact and judgment might overcome difficulties in the field, often at an immense expenditure of time and money, but he could not transmit his personal acquirements, nor enable schoolmen and professors to embody in their text-books and lectures the rationale of his results, unless they were prepared with a well-grounded theory to fit them in to the series of facts constituting the science. So far from throwing new light on old problems, each new investigator, from lack of a rational theory, only rendered them more obscure.

Mr. Gordon. That this was no exaggerated view was shown from Major Cunningham's other results. He investigated the amount and value of the surface-slope of the canal. That this slope was one of the most important elements in calculating the discharge of a stream was well known, yet this was what the Author said of it:—"The general conclusion from over five hundred cases was, that surface-slope measurement is so delicate a matter that the results are of doubtful use." Mr. Gordon could not agree in this conclusion as a general one, though it seemed applicable to the Ganges canal, solely on account of the weirs at the lower end of each reach. From experiments made on the Irrawaddy, now extending over several years, he believed that it was possible, by increasing the number of points and spreading them over a considerable space in order to make continuous or daily observations, to ascertain the general slope of a river or canal with sufficient accuracy to permit it to enter into a formula as a factor. But a very few observations, neither simultaneous nor extended, such indeed as had been too often used in some published results, were most misleading, and, taken with the utter absence of theoretical guidance to indicate the degree in which they should enter into the formula of calculation, landed the user of them in a dilemma of bewilderment.

The Author did not leave the discussion on the practical methods of measuring the discharge of an open channel in a satisfactory condition. For small regular channels there was not much difficulty; but for natural channels, and even for the Ganges canal, the final solution as to the best instrument of measurement had not been obtained. He favoured the double-float; in this agreeing with Messrs. Humphreys and Abbot and himself. But the Author did not treat the results with sufficient respect in his use of the method of least squares. He also approved of the rod, which apparently answered well in experiments like his own, and those of Mr. Francis at Lowell, in regular-shaped channels, not of great depth. But in moderate-sized channels of irregular shape, it had been found by the experimenters in the Rhine in Holland, that the rod was incorrect and unsuitable. They and other continental investigators appeared now to have entirely adopted the Woltmann meter in one or other of its various forms. The Author, however, seemed unequivocally to condemn these, so far as his experience extended, and quoted General Abbot with approval, who held the same view still more strongly. Mr. Gordon had for several years carried on extensive experiments on the Irrawaddy with double-floats, and had endeavoured during the last flood-season (1882) to

Mr. Gordon.

test these with three of Deacon's electric current-meters. The Indian Government gun-boat "Irrawaddy," with a large staff, was at his disposal, and most elaborate tests had been made. Two of the meters had been sent to be re-tested at Torquay by Mr. Froude, who originally ascertained their coefficients at the Admiralty experimental tank, and the result was not yet known. But from a careful examination of the results of the experiments, which were conducted in depths of from 12 to 100 feet, and up to velocities of 8 feet per second, it was feared that the instruments changed their rate in the silt-laden water, which also carried an immense quantity of fibrous vegetable matter in a fine state, the lowering wire and meter often coming up covered and clogged with this.

While, therefore, he believed that hydraulicians were deeply indebted to Major Cunningham for his valuable labours, and for placing the true state of the science so conspicuously forward, it was much to be regretted that it was not found possible to bring within the scope of the work some discussion or indication of the rational theory, by which alone the present confusion could be overcome, and real progress ensured. The Author had already shown in separate Papers that when he took up this branch of the science he would do much to elucidate it, and when the clue was found it was hoped that the present experiments would find their true interpretation, with those of others, in building up a sound structure of theory.

Mr. Hagen.

Mr. G. HAGEN, of Berlin, held that the laws hitherto discovered on the motion of water in rivers were of no value practically. The subject was too complicated, and very difficult, and the observations had not been sufficiently exact. He had already given his views respecting the Roorkee hydraulic experiments in the "Zeitschrift für Bauwesen."[1]

Mr. Hollings.

Mr. J. S. HOLLINGS remarked that, were it possible to ascertain with accuracy the mean evaporation of the whole globe, it would most likely be found to be exactly equal to the rainfall (for rain was but condensed vapour); otherwise the sea would be either advancing or receding on all shores alike, for the ultimate goal of all water not evaporated was the sea. Living as he did in Montserrat, a small island in the tropics, he had ample opportunity of observing how very little rain fell on the sea, compared with the land. He felt sure, if correct measurements of evaporation from the sea could be obtained, they would show a large excess over the

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[1] Band xxxi., p. 403. Berlin, 1881.

Mr. Hollings. rainfall on its surface; whilst on the other hand, the rainfall in a mountainous country would be in excess of the evaporation. So many causes, however, tended to modify evaporation, that it was almost impossible to keep such a gauge correctly. The temperature in the sun in the West Indies rose to about 150° Fahrenheit on hot days, and was rarely lower than 78° in the shade, calling the shade the coolest place available for a thermometer. This latter was also the mean temperature of the sea, whilst 80° was that of the air over the land in the coolest place. On some days with little wind, the wet bulb of the hygrometer in the shade was only 1° less than the dry, whilst on other days, with the same sun-heat, 10°, and occasionally 12°, difference were registered, the only altered condition being the movement of the air. The evaporation from equal surfaces of water on sea and land would most likely also differ materially under the same conditions of sun-heat and air-movement, for the vast reserve of unheated water in the sea would continually retard evaporation from the surface water, whilst the comparatively thin stratum exposed to the sun in lakes, rivers, canals, reservoirs or pools, would soon get heated through and rapidly evaporate. It was therefore almost impossible to obtain sufficiently reliable results from any number of evaporation gauges on land, or floating in terraqueous suspension, to form a basis for calculating the mean evaporation of the globe. He believed an evaporation-gauge had been kept at Barbados for several years, and that the average evaporation was about 3 inches per month, which agreed with the Author's $\frac{1}{10}$ inch per day. The maximum evaporation was, he thought, under 4 inches in any month; whilst the average rainfall was about 54 inches per annum. The hygrometer readings in Montserrat agreed pretty closely with those at Barbados, whilst the rainfall at sea-level was about 46 inches; at 250 feet above sea-level, 56 inches, and at 1,200 feet above, 84 inches. Thus, wooded mountains of only a little over 1,000 feet elevation, led to nearly double the rainfall, whilst at the same time they probably lessened evaporation by more than half; for at the higher elevation he had never seen a difference of more than 5° Fahrenheit between the wet and dry bulb, and rarely more than 2°. All this tended to prove that, until a series of observations embracing different grades of level of land, and varying depths of sea, both for evaporation and rainfall, were collected, the question of whether the one was in excess of the other could not be answered.

Mr. Leslie. Mr. JAMES LESLIE observed that the experiments showed a smaller velocity at the surface of the water than at some depth.

Mr. Leslie.

Might not this arise from the floats being slightly above the surface of the water, and so being impeded by the air, if calm, or still more if there were some wind in a direction opposite to that of the water? The average velocity seemed to have been taken with great care from many points at different depths, and at different horizontal distances, and if the sectional areas were given, would afford accurate data for calculating the discharges. The surface fall in any ascertained length might have been given with advantage, and either two or more cross-sections in that length, or the sectional areas and rubbing surfaces, so as to afford means of ascertaining the mean hydraulic depth, and thereby the proper coefficient for any formula adopted, for arriving at an approximation to the velocity and discharge of any river, without having to go through the troublesome operation of finding the average velocity. He thought the alleged difficulty in ascertaining the surface-level, owing to the undulations, might have been obviated by having a vertical tube or cylinder, with a small orifice at the bottom like a marine barometer or a tide gauge.

The experiments on the evaporation from the surface of water, showing it to be only $\frac{1}{10}$ inch per day, were important, as correcting a widely entertained notion that in Great Britain the evaporation from the surface of water was much greater than that stated, and even considerably more than the loss from agricultural or pasture land.

He had found, from many observations, that in dry summer weather, evaporation from a surface of water in Scotland was only $\frac{1}{12}$ inch per day, and throughout the year it was considerably less than the loss from the surface of land having vegetation on it, showing that it was a mistake to deduct the surface of a lake or reservoir from that of a gathering ground.

As the Indian observations on the evaporation from a surface of water were generally made during dry weather, those observations during wet weather being mostly rejected, it might be assumed that the average evaporation for the whole year would be much less than $\frac{1}{10}$ inch per day.

The evaporation was from the surface of water in a vessel about 1 foot in diameter, and from two zinc tubs about 2 feet in diameter, filled with earth and growing grass, the one outlet being at the surface representing undrained land, and the other at the bottom representing drained land.

Mr. Leslie.

OBSERVATIONS MADE at FERNIELAW, COLINTON, on the NORTH SLOPE of the PENTLANDS and 500 feet ABOVE SEA-LEVEL.

| Date. | Rain Gauge. | Loss from Surface of Water. | Loss by Deep Drainage. | Loss by Surface Drainage. |
|---|---|---|---|---|
| | Inches. | Inches. | Inches. | Inches. |
| 1871 | 35·2 | 19·4 | 16·4 | 20·4 |
| 1872 | 49·1 | 13·0 | 19·4 | 18·2 |
| 1873 | 34·4 | 12·8 | 15·5 | 14·7 |
| 1874 | 32·0 | 18·0 | 18·7 | 18·9 |
| 1875 | 34·1 | 14·2 | 17·6 | 17·0 |
| 1876 | 42·9 | 15·4 | 19·7 | 19·3 |
| 1877 | 45·7 | 15·2 | 17·1 | 16·3 |
| 1878 | 34·4 | 18·9 | 20·0 | 22·2 |
| 1879 | 39·1 | 12·7 | 15·9 | 15·5 |
| 1880 | 34·4 | 14·5 | 18·7 | 18·3 |
| Average | 38·13 | 15·41 | 17·90 | 18·08 |

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RAINFALL, and EVAPORATION from the SURFACE of WATER in a TUB 6 feet in DIAMETER, and 3 feet deep at GLENCORSE FILTERS on the SOUTH PENTLANDS, 650 feet ABOVE SEA-LEVEL. The LEVEL of the SURFACE of the WATER when set at ZERO is 1 foot BELOW the TOP, thus making the DEPTH only 2 feet.

| Date. | Rain. | Evaporation. | Date. | Rain. | Evaporation. |
|---|---|---|---|---|---|
| | Inches. | Inches. | | Inches. | Inches. |
| 1857 | 34·90 | 12·30 | 1869 | 34·15 | 14·15 |
| 1858 | 28·75 | 10·90 | 1870 | 27·20 | 11·90 |
| 1859 | 35·05 | 12·60 | 1871 | 34·45 | 10·30 |
| 1860 | 30·00 | 9·50 | 1872 | 52·20 | 9·25 |
| 1861 | 38·50 | 10·00 | 1873 | 36·30 | 10·15 |
| 1862 | 42·80 | 10·25 | 1874 | 37·75 | 11·60 |
| 1863 | 38·90 | 12·45 | 1875 | 37·90 | 11·95 |
| 1864 | 35·75 | 12·15 | 1876 | 45·35 | 12·11 |
| 1865 | 35·60 | 9·50 | 1877 | 54·30 | 11·05 |
| 1866 | 37·50 | 9·10 | 1878 | 38·40 | 13·95 |
| 1867 | 37·75 | 10·45 | 1879 | 46·75 | 11·65 |
| 1868 | 46·00 | 14·70 | 1880 | 45·00 | 12·10 |
| | | | Average | 38·80 | 11·42 |

Mr. McMath.

Mr. R. E. McMath, of St. Louis, Mo., U.S.A., observed that the theory upon which slope-formulas were based required uniformity of cross-section as an indispensable condition. The longitudinal section, Plate 1, "Roorkee Hydraulic Experiments," showed that the immediate vicinity of the experimental sites did not satisfy this condition. Therefore verification of slope-formulas could hardly be expected from the experiments, and it would not be fair, upon their evidence, to press the conclusion that empirical formulas were of "uncertain applicability." Limited in application they certainly were.

Likewise the underlying theory of discharge-tables did not allow of change in the conditions of out-flow from the reach. The experimental sites were subject to the influence of the "State of control," and consequently observations should alone be compared, which had been made under similar conditions of obstruction in distributaries and at the tail of the reach. Any change in these conditions required a new table rather than complication by "double entry."

As a first impression, it might seem that the tendency of the work was to discredit formulas and discharge-tables. A truer interpretation of its teaching was that both must be used within rigid limits, and not loosely as had been the practice. Both methods were useful when properly applied, and were indispensable to the engineer, the one when designing, the other for continuous-discharge determination. But this necessity attached to the essentials, and not to forms.

The Author said in regard to formulas, "Until some guide is obtained from a rational theory, it seems hopeless to attempt further improvement." An important step towards a rational theory and practice would be effected when recognition was taken of the wide difference between an artificial channel of unvarying section and uniform bed-slope, and the natural stream in which both varied continually. The use of a formula containing slope and hydraulic mean depth was strictly limited to the former. Otherwise $S = \frac{\text{Fall}}{\text{Distance}}$, and $R = \frac{\text{Sectional Area}}{\text{Wet Border}}$, were not properly associated together, S being for a distance, and R for a single point in that distance. To be logical, $\frac{\text{Cubical Contents of Reach}}{\text{Wetted Surface}} = \rho$ should be taken instead of R. In such case the particular section chosen for a site should be that in which $R = \rho$, the latter being determined for the full slope-length. This involved another condition. The local values of R must either increase or diminish

Mr. McMath. throughout the slope-length, in order that $\rho$ might be a hydraulic mean, as distinguished from mere arithmetical mean.

These conditions had not been observed, even by the originators of slope-formulas, and yet, he thought, need only be stated to be accepted as controlling.

In a natural stream the unequal distribution of fall suggested that surface gradient, local slope, measured the retarding rather than the accelerating force, answering to the force expended in overcoming resistances, rather than to that which produced motion. Momentum and facility for transmission of pressure had to do with motion in open as well as in inclosed channels, for the current often continued when slope was negative. A rational theory recognising these facts would have regard to head and not to its accidental distribution.

The possibility of useful discharge-tables—and there were such —was so closely connected with the better theory, that to show the conditions of the regular law of discharge implied in a table was, so far as it went, the development of the theory.

1. For a discharge-table, it was essential that the preliminary observations should be made at the site for which the table was to be prepared; or that simultaneous gauge-observations should be made and referred to a common datum, so as to refer each observed discharge to the proper local height.

2. The cross-section should be a regular figure, at least above the level of no-flow, preferably a rectangle.

3. There should be a considerable wet area remaining when the water was drawn down to the lowest possible level. For area controlled the increments of velocity, and determined a discharge-curve, which, if not simpler, was at least flatter, and therefore determinable with less percentage of error than when area became small. This condition located the discharge section in a "marked hollow in the bed-slope," and required it to be in the obstructed sub-reach.

4. In the obstructed sub-reach, the crest of the fall defined the level of no-flow. In a river, the crest of the shoal defined that level in the reach above. This level was different from the plane of low water. It fixed definitely the origin of the discharge-curve.

5. The conditions of discharge from the reach must be invariable. The utility of discharge-curves rested upon their affinity to a weir-formula; or that the discharge over a given weir, whether free or submerged, was for a given head a definite quantity. If the opening at the weir were changed, in any way, the discharge would

be affected. The opening of side-channels (distributaries) might be the equivalent of lowering the crest, as well as of increasing the area of discharge.

6. The conditions of approach to the reach should be such, that currents should follow the same general direction at high and low stages, and that no eddy should at any time exist at the discharge-section.

7. In a river it was important to select a site remote from a tributary, if possible, but below rather than above.

By observing the above conditions the results would be reliable if the local law of discharge was well determined.

For such determination a series of observations would be necessary, covering a range of stage as wide as practicable. The conditions stated were intended to make the discharge conform to an equation of the second degree. Since adopting the Author's notation, $D = AV$, an equation of the second degree implied that the varying values of A and V should each follow the law of a straight line. In a rectangular section this was secured for A.

$$A = b\,h.$$

Also if the section was irregular below the level of no-flow and rectangular above, for the area below no-flow level might be represented by a constant, $a$, and

$$A = a + E\,b.$$

E was the elevation of surface above the level of no-flow.[1]

For the velocity-law recourse must be had to the discharge at the fall, and to the conditions of motion in the obstructed sub-reach.

The head of the sub-reach was defined by the intersection of the line of no-flow with the "thalweg" profile. Taking the elevation of the surface at the head of the sub-reach above the crest of the fall (the weir, not water-crest), E was obtained, the head which controlled motion in the reach. A certain part of E was consumed in overcoming resistances; the remainder, $E_1 - E_2 = E_3$, was found at the fall where the discharge D, definite for a given value of $E_3$, passed through a section of fixed dimensions A, with a mean

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[1] In a paper read before the American Society of Civil Engineers, No. ccxxxix., vol. xi., Transactions, 1882, this element, new to hydraulics, was named "ruling depth," and was represented by Δ, but as that symbol was appropriated, Mr. McMath therefore now substituted E, an abbreviation for "elevation above weir-crest."

Mr. McMath. velocity V, due to the fall $E_3$. At no other point in the sub-reach was there a velocity approximating that due to the fall, local or aggregate, being clearly too great for the local $\Delta$ E, and too small for the aggregate E. At any section with an area A, the mean velocity, V, would be the quotient of $\frac{D}{A} = V$, and $\frac{V_1}{V} = \frac{A}{A_1}$. This relation of velocities at the section in the reach and at the fall held at all stages; consequently, if there was a law at the fall, there must be at every section in the reach a direct relation between E and V.

If consideration was given to the increments of V at any section for definite increments, $e$, of E, evidently $\frac{A}{A + e\,b}$ and $\frac{A + e\,b}{A + 2\,e\,b}$ would differ but slightly, and by a nearly constant difference, if the initial value of A was large. This was provided by the third condition, therefore the actual values of V would be closely approximated by the equation of a straight line,

$$V = c\,E + d.$$

Combining with $A = a + b + E$, then

$$D = A\,v = c\,b\,E^2 + E\,(a\,c + b\,d) + a\,d.$$

For the coefficient $c$ and constant $d$, the measured discharges must be depended upon.

Within the ordinary range of measured discharges it would be found that, the better the observations, the more closely would the mean velocities approach a straight line. But if accuracy was reached in the first decimal place, the fact that the law of mean velocity was of the second degree became apparent. The curve was very near a hyperbola,

$$V = \left(\frac{A^2}{B^2} E^2 + A^2\right)^{\frac{1}{2}} \pm A,$$

A being the transverse axis, and an apparent function of $\sqrt{2\,g\,E}$, B being the conjugate axis and an apparent function of mean depth, or hydraulic mean depth.

Since curves were by observation concave or convex to the axis of E, according as mean depth was less or greater than an undetermined limit, it was suggested, that when the hydraulic mean depth of discharge section was equal to $\rho = \frac{\text{Cubic contents of reach}}{\text{Wetted surface}}$, Mr. McMath. the velocity law was a straight line.[1] This furnished another condition of choice for a permanent discharge site.

8. The local value of R should approximate that of $\rho$ for the obstructed sub-reach.

According to the view taken, the section at the crest of the fall was the limiting section for the sub-reach; but this was true only as it was the section of least area. For the head, E, in the reach was that required to force the discharge through the smallest area. The relations shown would be destroyed, if the site chosen was above a part of the reach so narrow that the area at high water might be less than at the crest of the fall. The limiting section must be least in area and depth. The argument had followed the case of a dam of free overflow, in order that the definite value and influence of E might be unmistakable. The relations would be no less useful if the dam was submerged, or if it was a natural shoal.

Hints of the more rational theory had appeared in the foregoing discussion. It might therefore be appropriate to add, in closing, that the better theory could only be reached and tested by hydraulic experiments, whose results should be cleared of the effect of "unsteady motion." This could not be done by any method of direct velocity measurement; but could be by making an absolute measure of the quantity discharged in a given time. Mr. J. B. Francis, in his Lowell experiments, obtained mean velocities accurate to the fourth decimal place, as was evidenced by systematic residuals when compared with approximate computed curves, good to the second decimal place. With such data to work from, the way to a rational hydraulic theory would not be long.

Mr. Neville. Mr. John Neville thought the measurements to obtain the average velocities and depths (pp. 5 and 6) should have been extended for longitudinal as well as for cross-sections. The width should have been divided into several sub-runs or sub-channels, moving side by side according to circumstances, but not less than three. Such measurements would give a simple and sure operation for finding the discharges from top to bottom within the widths of each sub-run; and between the banks, by adding the sub-discharges together (similar to the use of offsets to a chain line in surveying), which would obviate the use of formulas (p. 25), by suitable admeasurements.

The means of determining the bed-, side-, and mean velocities

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[1] If this suggestion was not strictly correct, it was at least an approximation, and of value as a practical guide in selecting a site.

Mr. Neville. was not very clear. The Author said (p. 23), "The decrease of forward velocity is so rapid close to the banks as to make it clear that the forward velocity must be very small;" "it does not admit of direct measurement." And again, "In a float-course only 7½ inches from a straight vertical bank one hundred surface-floats were run before three (3) were obtained in fair course over a 12½-feet run." Mr. Neville had himself often found a backward surface-flow near the banks of rivers. It was stated (p. 8) "that the range of velocities, deduced from a number of similar floats run in rapid succession over nearly the same float course, was commonly 20 per cent. of the mean. In some of Harlacher's experiments a current-meter was fitted with electrical connections so as to record every revolution; the variations amounted to from 20 per cent. in surface-velocities to 50 per cent. in bed-velocities in a few seconds." He had often observed that the flowing section of a river did not always correspond with the section of the channel taken to the water-level. Yet an average forward discharge took place, notwithstanding many changes of direction in the motions. Practically, taking the mean velocity as $v$, and the maximum surface-velocity as V in feet, the formula deduced from Du Buat's experiments by Prony, viz., $v = \frac{7 \cdot 783 + V}{10 \cdot 345 + V} \cdot V$, gave more consistent practical results for all channels in the run of professional work than the more complex formulas of later writers on the subject.

The falls, of about 8 feet, at the lower ends of the reaches on which the observations were made, must have affected the results, and depressed the surface maximum velocities down towards the level of the crest of the overfall or sill of sluices. Great disturbance must also have existed below the foot of an upper, and head of a lower, reach. The depression of the maximum surface-velocity from wind and atmospheric causes was only occasional and partial, although the earth and the atmosphere formed a sort of compound tube for the water section to flow in. Notwithstanding that the vertical velocity-curves were "all very flat" (p. 14), so flat that "any geometric curve could be fitted very close to them" (p. 15). The statement, therefore, that the "last result is of great scientific importance," viz., that "the vertical curve is nearly a common parabola, the error of whose computed parameter is often very large," was scarcely justified. Also the statement that "no confidence can be placed in values of the parameter not formed by the method of least squares;" concluding with: "After many attempts to construct a new formula, the conclusion is drawn that the data are too uncertain to admit of it!" Page 16 would seem to show that these experiments were productive of little useful result. Mr. Neville.

That the transverse surface-curves (pp. 12 and 13) must have their convexity or concavity always small, dependent on the depths below and corresponding surface-velocities, has been well known for a long time. The great number of Roorkee experiments added nothing new to the scientific knowledge of these curves.

Observed levels of the water-service at and above the falls, in the stretches, would have given useful data for the construction of the backwater curves of much more practical and scientific value than any vertical-velocity or transverse surface-curves.

The surface-slope or longitudinal inclination of rivers was apt to lead to a great deal of misapplication of hydraulic formulas. It was so flat that, unless at rapids, a considerable change was not always apparent. The range in these observations is from 1½ inch to 30 inches per mile, or from 0·0284 to 0·568 inch, nearly, in a float-run of 100 feet. The levels themselves varied 0·07 foot, or 0·84 inch in the transverse section (p. 6). "From twelve trials of slope-lengths of 2,000 and 4,000 feet, symmetrically situate about the same site," the slopes differed 25 per cent. (p. 10), and slopes at opposite banks differed 50 per cent. (p. 11). To find the value of the slope and for what run to take it so that it should correspond with the hydraulic inclination was practically next to impossible. Mr. Neville often found wind to affect the bank water-levels; and on the callow lands along the Shannon between Athlone and Portumna he had observed sometimes a difference up to 1½ inch at different times on each bank. There is an undulating wave-surface and ripple on rivers of any depth which affects the forward motion, causing it to vary from time to time in the same transverse section, and even simultaneously in different ones, the slopes varying very considerably, although the difference in level may be small. Slopes of 1½ inch and 6 inches in a mile gave only about 0·3 inch and 1·2 inch in over 1,000 feet, yet the small difference, 0·9 inch, in this distance involved the doubling of the velocity. He remembered a law-suit in which one engineer, calculating the flow in a mill head-race from the surface-slope, and another from floats, differed as 3 to 1. Not only did the surface-slopes vary in the same river, but so also do the depths, including the hydraulic quantities $r$ and $s$ and the area of the section in motion.

Mr. Neville had considered elsewhere[1] the various formulas

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[1] "Hydraulic Coefficients, Tables, and Formulæ." Second edition, pp. 182 to 226. Also Third Edition, pp. 188 to 251.

Mr. Neville. enunciated by their Authors from Du Buat to Weisbach and since; most of which were applicable to pipes and rivers, properly understood, $r$ and $s$ being factors in all. In pipes there was no limit to the inclination from flat to vertical; for canals and rivers it never exceeded a few feet per mile. The experiments from which those formulas were deduced were select and varied, ranging from pipe-diameter of $\frac{1}{2}$ an inch and less to 18 inches, to open channels, large canals and rivers. There were differences, and would continue to be, between a formula grasping so great a range, in size and inclination and particular experiments; but this was of no practical importance. Very little had been done since to improve this practical branch of hydrodynamics. A formula should be framed meeting any particular group of hydraulic experiments working within practical limits; it must be in part empirical, not too complex, and capable of application to vertical short pipes as well as flat long rivers. Du Buat conceived, and first constructed, a formula to meet these conditions, using one hundred and twenty-five experiments, carefully corrected for the entrance-velocity on both closed and open channels, pipes, and rivers. Nothing better had been done since.

The results to science of his hydraulic experiments was stated by the Author to be "that at present increased approximation can only be obtained by increased complexity; there is no guide as to the form of such improved approximations, whilst the labour of tentative research is excessive. Until some guide is obtained from a rational theory, it seems to the Author hopeless to attempt further improvement." "A guide," and "rational theory" were given by Du Buat long ago. The surface-slopes of open channels of any good size were so small in fall, and varied at the same time so much in ratio, that they would never be used by competent engineers or by experimenters to determine or test the observed mean velocity by the form of equation $v = m \sqrt{r s}$, or any other; though they might from the equation $s = \frac{v^2}{r m^2}$ determine the slope where experiments would fail. Besides, the surface-slope in most cases was not the hydraulic inclination, nor was the average slope in 2,000 or 4,000 feet, more or less, that also found at intermediate points or at the float-runs from $12\frac{1}{2}$ to 100 feet long. It rested with the experimenter to determine accurately the values of $s$, $r$ and $m$ before giving an opinion on the value of any formulas containing these elements.

Lieut.-General Rundall. Lieut.-General F. H. Rundall, R.E., C.S.I., had already reviewed the subject of the Paper under discussion in "The Royal Engineers'

Lieut.-General Rundall.

Journal."[1] Two points of special interest connected with the experiments might be referred to. One point, theoretical, was the convexity of surface in rivers at certain stages of their floods; the other point, practical, was the effect of silt in suspension on the flow, the proportionate quantities carried at different velocities, and the quantity or proportion deposited on a diminution of velocity. This last was of great importance in the case of canals in India, where silt clearances had often to be annually made, entailing one of the heaviest items of "annual repairs." But "silt" was understood differently in Northern and Southern India. In the Ganges Canal it consisted mainly of material derived from erosion of the bed and sides of the canal, owing to the comparatively high velocity maintained in the upper reaches. In Southern India silt was understood as the solids held in suspension in the river, and deposited at every change in the velocity. That in the Ganges Canal was not fertilizing, except at periods of the year when the River Ganges happened to be in flood. In the Delta rivers of Southern India the silt was invaluable, and the more of it that could be conveyed to the fields the better. The problem with engineers was how to take into the canals as much silt as possible, and to allow as little of it to be deposited in the canal consistent with maintaining as low a velocity as was advisable for navigation purposes. He noticed that the Author stated that the result of his experiments in this respect was disappointing, and that the Ganges Canal was not a suitable work for testing the relations of silt to velocity.

Mr. Stone.

Mr. T. W. Stone considered that the results given in the Paper did not admit of dispute, as they agreed generally with received opinion on such questions. There were, however, a few points on which he desired to remark, solely with the desire of eliciting further information. The primary objects of the experiments were, he apprehended—

1. The discovery of a good method of discharge-measurement.
2. Testing the applicability of known mean velocity formulas.
3. The discovery of a good approximation to mean velocity.

The two first objects had been attained. In regard to the third, he thought that although a practical result had been achieved in regard to existing canals, the like could not be said in reference to projected canals, or the investigation of floods in rivers, beyond

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[1] Vol. xii., p. 65, March 1882; also p. 92, April 1882.

**Mr. Stone.** the statement that Kutter's formula for variable coefficients, applied to the old expression $C\sqrt{RS}$, would give results seldom exceeding 7½ per cent., and that such results fall far short of that given by direct discharge-measurement. In the concluding paragraph of Section XXI., "Discharge-Verification," a 5 per cent. difference was called a close approximation, and it was shown in Section XX., "Mean Velocity," that Kutter's formula would give results seldom exceeding 7½ per cent.; it followed, therefore, that the Author's experiments tended to establish Kutter's formula as a close approximation for mean velocity in canals and channels 200 feet wide and under. Now Kutter's formula in effect proposed the use of the old formula $v = C\sqrt{RS}$ with variable coefficients, and to obtain such coefficients the two variable factors S and R were used. The formula agreed nearly as well as the discharge-verifications, yet the Author said, Section XIV., "It was found that the direct measurement of any velocity was far more likely to give an approximation to this mean velocity than any expression yet known not involving velocities." And again: "It seems, then, that at present the direct measurement of velocity, such as the central mean or central surface, is more likely to give a near value of the mean velocity than any formula involving surface-slope." It should be constantly borne in mind that with factors R and S Kutter's formula for variable coefficients agreed to 7½ per cent., while tests for discharge-verification varied, on the Author's showing, 5 per cent. and more. A wide range of experiments would be necessary to determine what the central surface or central mean velocity was likely to be in any proposed channel without use of formulas; and such experiments must extend to channels of not only different sizes, but of all various inclinations and descriptions. It was necessary, therefore, that some formula should be used for the design of projected channels, the investigation of floods in rivers, &c., &c., and Mr. Stone maintained that a set of variable coefficients of mean velocity of discharge should be used in accordance with the circumstances of each special case, and the nearest similar recorded observation that could be obtained. This should be so with all hydraulic calculations. There was no necessity to alter the old theoretical formulas; the coefficient expressions might vary as experience became enlarged. Such expressions as $v = C\sqrt{RS}$, $v = C\sqrt{R(f\,2)}$, used with a variable coefficient determined on the lines proposed by Kutter, would give fairly approximate results. The importance of using variable coefficients was pointed out in Mr. Stone's recent work, "Hydraulic Formulæ," and was, he thought, always followed by engineers

Mr. Stone. experienced in hydraulics. The Author's experiments had been valuable in showing that such a course led to fairly approximate results. The channels constructed by Mr. Stone in Australia were calculated from the formula $C \sqrt{R\ (2f)} = v$, and C was varied from 35 to 75, to give mean velocity in feet per minute, according to the nature of the proposed channel. The channels varied in section from 2 feet square to 6 feet at bottom 6 feet deep, and slopes of 1 to 1 and 1½ to 1, and some of the channels had a circular lining; the inclinations varied from 2 feet to 10 feet per mile, and the formulas gave very fair results. To establish the statement of the Author, above quoted, further confirmation would seem desirable. He thought that the assertion in Section III., "As the canal-water is often full of silt, this shows that the water is in pretty rapid motion close to the actual bed, and disproves the idea sometimes advanced that an obstruction across a channel causes a still-water pool above it roughly flush with its crest," should be accepted with caution. Whilst Government Engineer in Australia, Mr. Stone built a series of obstructions from 8 to 11 feet high across a channel for the same purpose as the falls on the Ganges Canal were built up, viz., to reduce the velocity. The channel in question was about 30 feet wide at the top, and irregular in section, on account of the previous velocity having been too great for the bed and banks. The large amount of silt which accumulated in the reaches above the obstructions in this case went to show that the Author's deductions was not invariably correct; possibly the frequent exercise of the control spoken of at the falls for working the canal might have tended to produce scour in the bed, and so prevent silting. At Section VI. the Author first referred to the fact that both parallel motion and steady motion did not exist even approximately in flowing water, and assumed in effect that all formulas based on such motions must be incorrect. Finally, however, he admitted that there was an average steady motion. Though of opinion that formulas constructed on the theory of steady flow would be fair approximations in connection with variable coefficients, he agreed with the Author that any single velocity-measurement would be clearly an accidental value. At Section VIII., "Surface-convexity," the Author stated that the fair conclusion seemed to be that "the water-surface across is probably level on the average." As a general rule Mr. Stone thought the cross-section of a stream showed a horizontal line for the surface of the water only when a uniform depth and velocity obtained right across, or nearly so. If one part of a channel were much deeper than the rest, and the velocity of the water was

Mr. Stone. much greater in that part than in the other parts, the level of the water became raised there, so that the surface line became uneven. When the stream was confined to only a portion of the channel, the rest being still, or backwater, the irregularity of the surface was greatest. This was exemplified by observations taken during an investigation of the floods in the river Barwon in Australia, in 1880.[1] It had also been observed in some of the large drains in Melbourne, Australia. According to Mr. Culcheth, the velocity of the stream of the Barwon was 5·4 feet per second, and the river 1,400 feet wide. The mid-stream level was convex with regard to one bank, and concave with regard to the other. The Melbourne drains spoken of were about 9 inches wide at the bottom, with sloping sides of 1 in 5 on one side, and 1 in 10 on the other. They sometimes ran from 1 foot to 1 foot 6 inches deep, and had varying inclinations.

Prof. von Wagner. Professor VON WAGNER did not possess the complete work of Major Cunningham, but only the Paper read before the Institution, which treated in separate chapters the principal conclusions, and in some cases dealt with them more fully. It would therefore be simplest to give his opinion in the order of those chapters, and to add in conclusion some remarks related to the subject.

IV. He agreed with this entirely as regarded surface-floats. Large bodies, such as boats, floated, it was said, with greater velocity than the water, but small bodies, such as spheres, or disks, might well be used for the measurement of normal velocity, because, as was proved by all experiments, no sensible acceleration could be detected. Of many experiments on this point, he would only name those on the Rhine at Speyer, in which two carefully-adjusted instruments, a screw Current-Meter and a Darcy Tube both gave a velocity of $v = 2 \cdot 17$ metres, while nine surface-floats, with a path of 100 metres, gave $v = 2 \cdot 18$ metres. To get accurately the horizontal curve of surface velocities he selected in the transverse water-line not too many positions (for a float-path of 100 metres, about seven or eight), and then for each position he observed at least ten to twenty floats. He considered this better than to select twenty positions and to observe at each four or five floats. It was very prejudicial that the wind had so much influence on floats, and that good results were only to be obtained when the air was nearly still.

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[1] W. W. Culcheth, M. Inst. C.E., Paper "Floods on the Barwon," read before the Royal Society of Victoria, December 8th, 1881.

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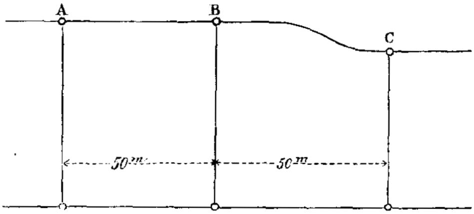
Further he thought it important to consider the longitudinal profile of the water-surface (Fig. 8). If, for example, the surface-velocities at a cross-section B were to be ascertained by a number of floats, the path of the floats being about 100 metres (A B = B C = 50 metres), then it might happen that in one-half of the path B, the fall might be greater than in A B. If the surface had such a form, then in his opinion the float-path should be shortened and the number of floats observed should be increased, in order to obtain a mean velocity.

FIG. 8.



As to those floats which reached deeply below the surface (loaded rods or coupled balls), he had hitherto not been convinced that they gave accurate results. His objections to them were stated in his "Hydraulic Experiments on the Weser, Elbe, and Rhine," p. 3.[1] Whenever accurate measurements were required he preferred good current-meters, such as those of Amsler-Laffon, Harlacher, and others. But of floats he only used surface-floats, and these only to check the coefficients of the current-meters, or to obtain approximate values as indicated below.

V. Agreeing with this, he would only add in regard to the measurement of the slope, that, if there was a rippling surface, he had always made a small side basin close to the river, in which the surface was kept still by a floating plank or even by pouring on some oil, and thus the reading was made more definite. He was led to do this by the opinion that the water-level so obtained would be a correct mean between the wave-crest and wave-trough.

He quite agreed with VI., especially with the final words "Taking" to "motion." As to theory in the proper sense of the word (hitherto everything was but empirical formula), it could

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[1] "Hydrologische Untersuchungen an der Weser, Elbe, dem Rhein und kleineren Flüssen." Braunschweig, 1881.

Prof. von Wagner. not concern itself with eddies, &c., but must be based on normal conditions. The chief difficulty was the necessary modification by experimental values, coefficients, &c.

His views also coincided with those expressed in VII. Whenever the longtitudinal profile of the water-surface had the form of a long curve, then, according to Harlacher's method, the true slope was the tangent to the water-surface at the intersection of the wave-form with the cross-section considered.

He had found as a rule that the longitudinal slopes were the same at both banks, if the river at the site had a normal bed and a straight direction. When there were irregularities, then two levellings, one at each bank, were sufficient; but with large rivers one might be desirable in the middle, *i.e.*, in the thread of the stream; its measurement was, however, exceedingly difficult.

VIII. He had also had an opportunity of observing the transverse water-line, and had given the particulars in "Hyd. Untersuch.," p. 42.

X. The statements confirmed what he had found in other rivers:—

(*a.*) That generally the vertical curves were more bent, the smaller the depth of water, and *vice versâ*.

(*b.*) That the mean velocity of a vertical was smaller than that at the half depth.

XI. Differences of opinion still existed as to the position of the parabolic axis. Some placed it vertically, with the vertex of the parabola at the river bed. The majority placed the parabolic axis horizontal and at the point of greatest velocity. It was interesting to him that Major Cunningham placed it horizontally, as he did himself. His experiments, giving a considerable number of carefully measured vertical curves, proved that with a vertical parabolic axis were obtained results altogether useless, because the normal parabola deviated so quickly from the measured curve, that there was no dependence on the position of the vertex at the river-bed. On the contrary the results were satisfactory for the other position of the axis. In his investigation of sixty-four curves of small streams and large rivers he had come to the following conclusions:—

(*a.*) Down to a point at depth M, Fig. 9, (on the average at 0·8 of the whole depth) the curve consisted of a parabola A M, but below M, of a curve deviating from the parabola, the water being retarded by the bed. A similar deviation, though to a smaller extent, was found from A to W, possibly in consequence of the friction of the air. The vertical curve thus consisted of three

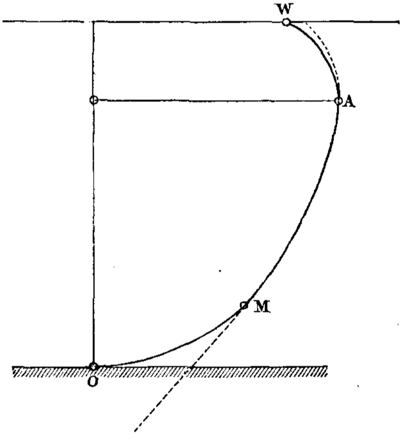
Prof. von Wagner.

parts, of which the middle one, which was parabolic, had the greatest length.

(*b.*) The axes of the parabolas were horizontal (or strictly parallel to the water-surface).

(*c.*) The maximum velocity (or parabolic axis) was at from 0·0 to 0·28 of the whole depth below the surface. In the Mississippi, at 0·3. Even those curves, which were measured in a perfect calm, showed the parabolic axis below the water-surface. More details of this would be found in "Hyd. Untersuch.," pp. 39, 40, and Plate 8, Figs. 86 to 86*c*.

Fig. 9.



XIV. The formulas here given he had compared with the values in a number of different measured curves (Wesen, Elbe, Rhine, Danube, &c.), and he had arrived at the conclusion that the second formula,

$$U = \tfrac{1}{2}\left(v_{0\cdot211\,H} + v_{0\cdot789\,H}\right),$$

gave more accurate results than the other. In most cases it agreed perfectly, the differences being never more than $2\frac{1}{2}$ per cent.

With regard to the other statements in Section XIV., as to the position of the mean velocity on a vertical, he would refer to p. 37 of his "Hyd. Untersuch." It was there shown how little the position of mean velocity differed in the most different curves. Now if to these results were added those on the Mississippi, as well as that given in Section XIV. of 0·62 or $\frac{5}{8}$, the following figures resulted:—

Prof. von Wagner.

| River. | With Breadth in Metres. | Observer. | Number of Measured Curves. | Depth of Mean. Velocity. Arithmetic Mean of the slightly varying values. |
|---|---|---|---|---|
| | Metres. | | | |
| Weser | 80 | von Wagner | 6 | 0·587 |
| Elbe | 114 | " | 7 | 0·594 |
| Rhine | 215 | " | 4 | 0·589 |
| " | 215 | " | 1 | 0·600 |
| Oker | 14 | " | 3 | 0·600 |
| Elbe | 120 | Harlacher | 9 | 0·620 |
| " | 120 | " | 10 | 0·598 |
| " | 120 | " | 9 | 0·580 |
| Danube | 425 | " | 15 | 0·599 |
| Mississippi | | Humphreys and Abbot | .. | 0·580 |
| Ganges Canal | | Cunningham | 46 | 0·620 |

The arithmetic mean of these eleven values was 0·597.

If the question was simply to ascertain the discharge for an engineering purpose, then he thought it would be accurate enough to take five to seven verticals on the cross-section of a river and to ascertain the velocity on each vertical at 0·6 of the depth, measured from the water-surface.

To this he would add that the maximum velocity in the sixty-four curves, given in the Table, varied in position, from the surface (for example in the Weser), down to 0·25 of the depth from the surface (as in the Rhine), and yet the proportionate depth of the mean velocity differed but little.

XVII. Perfectly agreeing with these explanations, he begged leave to add the following remark. He was of opinion that at the bottom the velocity must be zero. From that point the velocity increased rapidly. The idea of "bottom velocity" was therefore very indefinite. In practice it was frequently stated, and its value was taken as equal to O N (Fig. 8), obtained by prolonging the normal parabola A M to meet the bed, although it was proved that the velocities in the part L O decreased quicker than was given by the parabolic law. In his opinion "bottom-velocity" meant only that velocity which existed at the height of the centre of gravity of the boulders, on which the local average size of the boulders depended. If at any place the boulders had an average size of 10 centimetres, then there the bottom-velocity was greater than at a place where the bottom was formed of sand of an average diameter of 2 millimetres (Fig. 9). It was therefore fruitless to seek a relation between the bottom and other velocities. As to the measurement of bottom-velocity, the current-meter was

Prof. von Wagner.

not very suitable because of the distance which must be allowed for the rotation of the screw-blades. By Darcy's gauge, measurements could be made at $1\frac{1}{2}$ centimetre above the bottom, provided it was level. This was shown by the curve Fig. 86 on Plate 8 in the "Hyd. Untersuch.," where, at each of the given depths, the difference of the water-columns had been observed thirty times.

FIG. 10.

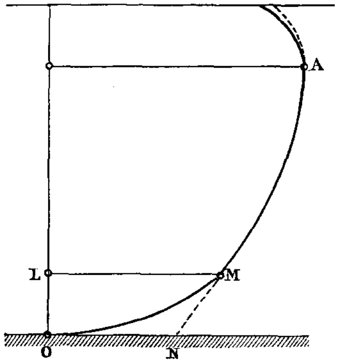
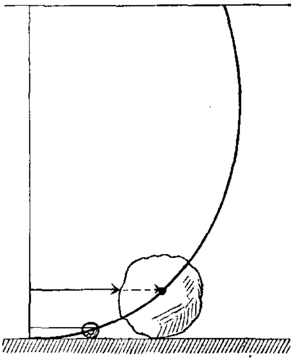


FIG. 11.



XVIII. Here the remarks confirmed the opinions in "Hyd. Untersuch.," pp. 40, 41, on the approximate conformity of the horizontal velocity-curve with the form of the wetted border, which was illustrated in Plate 8, Figs. 87 to 93. It would be seen that the conformity (which he had in vain sought to express algebraically) ceased in the same river with increase of discharge, (Figs. 92, 93), and besides differed near the shores (Figs. 87, 89, 93), in analogy with the alteration of the vertical curves.

XIX. He considered Harlacher's the simplest and most accurate method of determining the volume of discharge. It was explained in "The Measurements of the Elbe, Danube, &c.," by H. R. Harlacher, 1881. He had used the same method in his "Hyd. Untersuchungen," p. 18.

XX. The valuable work of the Author confirmed what Professor von Wagner had said, on p. 33 of the "Hyd. Untersuch.," about the degree of reliability of different formulas, and the statements in the Table, p. 32.

As the measurements in the Ganges Canal had been conducted with great care, it seemed desirable to make use of them to test two laws given in the "Hyd. Untersuch." If a continued confirmation of these laws resulted, then it would be of importance to practical engineering.

Prof. von Wagner.

Let,

$v$ = mean velocity of the whole cross-section.
$c$ = maximum surface-velocity in the whole transverse width.
$v_s$ = velocity at the centre of figure of a cross-section.

(I.) *Relation between $v$ and $c$.*—In twenty-four different gaugings of small and large streams, he had found

$$v = a\,c + b\,c^2.$$

The constants $a$ and $b$ were only discussed in a preliminary way in "Hyd. Untersuch." In his article in the "Deutsche Bauzeitung" (No. 82, 1882), more exact values had been obtained by the method of least squares, and these gave

$$v = 0{\cdot}705\,c + 0{\cdot}01\,c^2.$$

From an article in the "Deutsche Bauzeitung," it would be seen that another gauging (of the Elbe, above Hamburg) agreed well with this equation; the calculated value was only 2½ per cent. greater than that obtained by measurement. Should this equation prove sufficiently accurate, then the engineer would only need to measure $c$ by surface floats, in order to get a good approximation to the value of $v$, and from that the discharge.

(II.) *Relation between $v$ and $v_s$* (p. 38 of "Hyd. Untersuch.")—For this an equation had been formed from seven rivers, or, adding the gauging of the Elbe above Hamburg, from eight rivers. Using the method of least squares to determine the constants, the equation was—

$$v = 0{\cdot}738\,v_s + 0{\cdot}05\,v_s^2.$$

The gauging of the Elbe above Hamburg gave by this equation $v = 1{\cdot}16$ metre, while by direct measurement $v = 1{\cdot}17$. However, eight examples for determining the constants were far too few, although the differences were only—

| | | | |
|---|---|---|---|
| 1·10 | 1·10 | 0·10 | per cent. too great. |
| 2·30 | 1·00 | 0·70 | „ „ „ small. |
| | | 0·00 | |

Further the equation did not apply in cross-sections such as that of the Danube, Fig. 12, given by Harlacher. He presumed the equation would only apply to pretty regular and symmetrical cross-sections. Should the equation be confirmed it would only be necessary to determine the centre of figure of a cross-section, and

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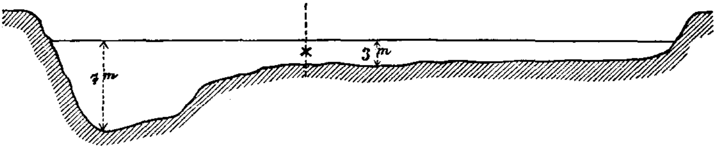
measure the velocity at that single point. He wished that both equations might be tried by the Ganges Canal results.

Fig. 12.



Major Cunningham.

Major Allan Cunningham observed, in reply to the correspondence, that the points noted by Mr. Flamant about comparison of levels of still and running water in free communication, and as to non-existence of surface convexity in permanent regimen, and non-existence of transverse surface-flow, were of high scientific interest. Further direct experiment was very desirable. As to the mode of weighting the velocity-data of a vertical curve, objected to by Mr. Gordon, it was known that with double-floats the velocity-data increased in accuracy from the bed upwards, so that the weights assigned should also increase from the bed upwards; but at what rate was of course unknown, and therefore a matter of judgment. No stress was laid on the weighting used. Admitting with Mr. Leslie that the action of the air and wind on the projecting portions of the floats used might have exaggerated the observed depression of the maximum velocity-line, still all instruments alike agreed in showing this depression as an existing fact. The irregularity of the bed of the Ganges Canal was not greater than that of most so-called natural streams, as for instance the Thames, in which lumps, bars, hollows, &c., of 1 foot or 2 feet depth were common: on the vertical scale used in the plates (Vol. II. of the Roorkee Work) these features were enormously exaggerated. Most of the data asked for or suggested as requisite by several of his critics had been actually printed in great detail in Vols. II. and III. of the original work. Whilst acknowledging Du Buat's great advance in hydraulics as previously known, it could not be admitted, as said by Mr. Neville, that "nothing better had been done since" in way of a mean-velocity formula. The Darcy-Bazin and the Kutter formulas were both important advances. Also Du Buat's "rational theory," quoted by Mr. Neville, was merely a highly general principle: the detail was still wanting to enable mathematical investigation to be properly applied to the flow of water, and in this sense a "rational theory" was still wanting. If Professor von Wagner's conclusions, p. 88 (*a*), Major Cunningham. that the vertical velocity-curve deviated greatly from a parabola near both surface and bed were correct, and that the real bed-velocity was zero, it would be right to give up the use of the parabola approximation. Lastly, Major Cunningham desired to thank the several speakers and writers for the great value of their remarks to him in his special research; his only regret now was that these remarks were not available to help him when the experiments were being made.

21 November, 1882.

JAMES BRUNLEES, F.R.S.E., Vice-President,
in the Chair.

The discussion upon the Paper on "Recent Hydraulic Experiments," by Major Allan Cunningham, occupied the whole evening.