Civil and Mechanical Engineering.

HYDRAULICS OF RIVERS.

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Several months before the appearance of the recent series of articles upon the "Flow of Water in Rivers and Canals," contributed to the journal by Mr. D. F. Henry, it became my official duty to criticise three annual reports submitted by that gentleman to the officer in charge of the Survey of the North-western Lakes—whose assistant he then was—upon certain gaugings of the connecting rivers, made in the years 1867, 1868 and 1869. Those three annual reports furnished the groundwork for the articles in the journal, which contain some views not generally admitted by hydraulic engineers.

My reviews being easily accessible (*vide* appendices B B and B B 1, Report of Chief of Engineers, accompanying the annual report of the Secretary of War for the fiscal year ending June 30, 1870), and neither leisure or inclination favoring controversial writing on my part, the subject would not have been farther discussed, had it not been that Mr. Henry, in his recent articles, has fallen into serious misapprehensions, which call for correction, when referring to the "Report upon the Physics and Hydraulics of the Mississippi River," prepared by General Humphreys and myself. To correct those errors intelligibly, it will be necessary to state the problem presented to General Humphreys, when, in 1850, he undertook the investigation.

The low lands bordering upon the Mississippi are below the level of its floods, from near the mouth of the Ohio to the Gulf. The surplus waters accordingly poured over the natural banks, and swept through the swamps until they encountered upland ridges, which turned them back only to again leave the channel, and thus to pursue their course to the Gulf. The whole country, in its natural state, in times of flood, was thus a vast shallow lake, many miles in width. But this lake, when the floods had passed, and the waters had drained back to the channel, became a belt of alluvial land, of untold fertility and value. To reclaim it, was to win an empire from Nature. The work was begun in 1717, near the mouth of the river, by extending levees up stream. In 1850, the embankments, although quite inade-

quate, reached the vicinity of the mouth of the Arkansas, but above that point the river held undisputed possession. At that date Congress donated the swamp lands within their borders to the several river States, to provide a fund for reclaiming them, and the work was vigorously begun. But what would be the effect of this sudden and great change in the regimen of the upper river, upon the height of the floods in the cultivated region below? and, indeed, how could the upper region itself be most economically protected? This was the problem confided to General Humphreys, and upon which ten years of systematic and exhausting labor was bestowed. No small part of the difficulty was due to the fact that the science of river hydraulics was in its infancy, as the numerous citations from standard writers contained in chapter iii of the report sufficiently proves.

One cause of many of Mr. Henry's misapprehensions of the report is that he has failed to appreciate the internal evidence of the truth of its isolated conclusions, derived from the multiplicity of its checks and counterchecks. For instance: one of the problems was, to decide how much given additions to the discharge throughout the valley (known, but varying at different localities) would locally raise the water level—a problem never before attacked in a manner claiming to be even approximately scientific. To solve it, was virtually to construct a tower, each stone of which should be an hydraulic fact, established by direct observation and measurement. This was successfully done, as has been shown by subsequent observations, both by ourselves and by others; as, for instance, by the present Chief Engineer of Louisiana, Gen. M. Jeff. Thompson, who, in his report, dated December 15, 1871, says: "The facts granted in the work of Generals Humphreys and Abbot, on page 418, that an outlet of 100,000 cubic feet per second, at or near Providence, would reduce the flood level some five feet down to near Red River, has been most closely confirmed." The stability of a building carries evidence of the security of its foundations, and yet Mr. Henry attacks certain fundamental propositions, as if the evidence of their truth rested wholly on the original direct observations, and had not been abundantly verified by the correctness, experimentally proven, of numberless conclusions, both general and particular, based upon them.

But to consider his criticisms a little more in detail, and in the order in which he makes them.

He asserts that the double float which we used "had been previously tried by Mr. Charles Ellet, jr., on the same river." A little examination of the reports would have shown that the Survey had

been systematically using this float more than two months before Mr. Ellet's first trial.

He proceeds to state that "it would seem to be impossible" to read and record the angles and time while a fast float was passing through 200 feet. It would be prudent before venturing such an assertion to inspect the diagrams of our work, now deposited in the archives of the Engineer Department at Washington, and open to the inspection of any engineer. They exhibit the paths of thousands of floats, two points of each of which were fixed by triangulation, and establish by the parallelism of these paths and the accordance of the times of transit, the accuracy of the method. Long habituated to the details of exact trigonometrical surveying, Mr. Henry under-estimates the practical precision that can be secured by less elaborate field-work, and forgets that, in operating upon a moving mass of water, we may advantageously imitate the sportsman, who, in analogous circumstances, sacrifices the precision of the rifle for the superior celerity of the shot-gun.

The next point for consideration relates to Mr. Henry's own work. He gives, on page 323, a table of comparisons between the result obtained by floats and a meter at different depths below the surface, and deduces a normal difference between their indications. He states that the observations were made in the St. Clair river, "where it was over 50 feet deep;" but, by reference to the detailed tables of his official report, it will be seen that the depth, although not always mentioned, is there shown to vary between 29 and 49 feet, and that, in grouping the floats, he has considered only the depth below the surface, quite ignoring this important variation in depth of water. Moreover, although the data are the same, the column indicating "corrected difference" appears more unfavorable for the floats in his recent article than in his official report. But this question of the relative value of float and meter observations having been discussed at length in my official review, will not be considered here.

Mr. Henry proceeds to write: "In the Mississippi, the new theories of flowing water place the maximum velocity three-tenths of the depth below the surface. As these observations are the only ones which do not conform to the general law, it will be well to examine them."

This language contains several errors. Theory had nothing to do with the location of the fillet endued with the maximum velocity. Our observations established that, in any vertical plane parallel to the current, its locus varied greatly with the wind; but that, in calm

weather, it was usually below the surface. Upon these and other observed facts the new theory was based, but no part of this theory assigns any definite depth for this locus. Upon the Mississippi in still weather it is usually at three-tenths of the depth below the surface; on some other streams we found it to be higher. No general law was announced, nor is such a law of any great practical importance, because in the general formulæ for discharge the quantity disappears by cancellation. As to our observations being the "only ones" which do not conform to the Darcy-Bazin results in little canals, Mr. Henry is greatly in error. The examples he cites (all of which are quoted in the Mississippi Report) do not comprise the "only ones" ever made, nor, indeed, do they all conform to the so-called "general law." Observations upon the Po, the Chiese, the Arno, the Rhine, the Waal, the upper Mississippi, and other streams, unite with ours taken upon the Mississippi and its bayous, to disprove the general character of this "law."

Mr. Henry next devotes a couple of pages (p. 384—6) to attempting to throw discredit upon the conclusions we derived from our field data. The simple facts, which refute all his inferences, were these: Two objects were in view—to gauge the daily discharge, and to determine the law governing the action of cohesion among the fluid particles. The former required daily measurements extended quite across the river; the latter, exact and greatly multiplied observations in single vertical planes parallel to the current. In each case, the path of every float was accurately located by careful triangulation where it crossed each line, and nothing was "assumed." The intended use was made of every float observed, exactly as stated in the Report, and no other use could properly be made of it, although the observations were all entirely "reliable" for solving the problem for which they were designed.

Mr. Henry's attempted tabular exhibit of our results (p. 386) is fundamentally erroneous. No such simple analysis is possible; for the reason that all individual curves of observation contain anomalies, which must be eliminated by the cancellation resulting from the multiplication of data, before they can be discussed in detail. Until a curve is thus obtained which conforms closely to the law of continuity, only a general discussion is admissible. It is therefore hardly ever practicable to fix upon the exact locus of the true maximum velocity in a curve based upon a few observations; and this can never be done by Mr. Henry's method, of taking for the maximum the greatest velocity actually observed, without regarding the relative velocities

of other floats in the vicinity. But, even if this method of discussion were not entirely inadmissible, his proposal to reject about three-eighths of the whole data, for no better reason than that it "must certainly be erroneous," hardly accords with usual scientific rules.

As to the statement, twice repeated, that "one of the verticals in the first series gives the maximum at the bottom, or rather one foot below the bottom, as recorded," it need only be said that no observations were made at the bottom or below it, neither were any so reported.* The true meaning of the record referred to is so evident upon inspection of the original text that it is quite needless to explain it here.

The next subject inviting attention is the form of sub-surface velocity curves as discussed by Mr. Henry. He gives (page 30—1, vol. lxiii) several mean curves deduced from his Lake Survey observations. Before accepting these data, reference should be made to the detailed tables contained in his official reports, whence they are derived. An inspection of these tables reveals a mathematical solecism which deprives the published means of all claims to exactness, viz., that interpolation is wholly ignored. To defend this singular method, Mr. Henry makes use of the following language: "If there are but few observations missing in a large number taken, then it will make but little difference whether we interpolate or not, for it will make but a slight variation in the mean; but if the missing observations are numerous, then (as they must be interpolated either in a straight line drawn between the known points, or in a curve) the mean will approximate more or less to the line or curve by which the interpolations were made."

The fallacy of this reasoning is best illustrated by an example taken from one of his own detailed tables, giving the subsurface velocities in the Niagara river, as observed in 1868.

Observed Velocity of River at the following Depths.

DATE.	20	25	30	35
July 22d,	2.730			2.789
27th,	4.680	4.412	4.119	3.969
29th,	4.551	4.220	3.857	3.676
Means as published,	3.987	4.316	3.983	3.478
True means,	3.987	3.794	3.582	3.478
Mr. Henry's error,		0.522	0.401	

A simple diagram will make the error of such a method of grouping observations apparent to the eye, and will show that before any well-grounded opinion as to the form of curve indicated by Mr. Henry's observations can be formed, an entire recomputation of his published means is necessary.

He proceeds to test by two of these supposed mean curves, the various forms proposed by hydraulic engineers—among others the "parabola of Humphreys and Abbot," which he concludes to be the "worst form" of all. This adverse opinion is due to his assuming an unwarranted value for the depth of axis, as may easily be shown. Since his own figures indicate that our parabola, when applied to his supposed mean curve for the St. Lawrence, has a maximum discrepancy of only 0.058 ft., and a mean discrepancy of only 0.022 ft. per second, it would be a waste of time to see if the accordance is not really more close. For the St. Clair river, however, his error of applying the formula is more serious, and calls for correction. In so doing, the surface velocity is neglected, since he frankly admits that it is "not properly taken."

St. Clair River—Depth from 44 to 47 feet.

Depth of Obs.	Observed velocities per second.	Humphreys and Abbot parabola as computed by Mr. Henry.		Humphreys and Abbot parabola correctly computed.	
		Curve.	Difference.	Curve.	Difference.
	3.868	3.564	—0.304	3.990	
5	3.907	3.573	—0.334	3.907	+0.000
10	3.821	3.564	—0.257	3.804	+0.017
15	3.709	3.536	—0.173	3.683	+0.026
20	3.608	3.489	—0.129	3.543	+0.065
25	3.496	3.423	—0.073	3.384	+0.112
30	3.309	3.339	+0.030	3.206	+0.103
35	3.100	3.237	+0.137	3.010	+0.090
40	2.678	3.115	+0.437	2.794	—0.116
42.4	2.388	3.050	+0.662	2.684	—0.296
45.4	1.428	2.962			
Sums,	33.884		2.536		0.825

One important fact is not to be forgotten in any comparison of these several proposed forms of curve. Other engineers simply give

a general form, leaving such values of the constants to be selected, in each individual case, as shall cause the best possible accordance between the theoretical and observed velocities. In our parabola, a general equation for the parameter, depending upon the mean velocity and depth, is given; and the form of the curve is, therefore, definitely fixed. Bearing this in mind, it is considered that the comparison instituted by Mr. Henry, freed from his errors of misapprehension, shows our curve to be fully equal to any of the rest, even as tested by these uncertain means.

Mr. Henry concludes by regretting that we did not select an ellipse, instead of a parabola, as most nearly indicating the law of variation. This was not done, because our ample data did not so indicate the law. But even if it had been found that an ellipse would give a slightly closer accordance with observations than a parabola, that curve would not have been selected; because we had to make a practical use of it in framing new formulæ for the discussion of higher branches of the subject of river physics; and the areas bounded by parts of an ellipse form a complicated series, while those bounded by parts of a parabola may be expressed in terms of a simple rectangle. This advantage would have been decisive, even if we had found (which we did not) that an ellipse would accord more closely with observation than a parabola, by a few thousandths of a foot of velocity per second.

But the truth of the Humphreys-Abbot theory for this curve does not, at this date, rest simply upon the Mississippi observations. The matter has been carefully studied since—among others by von Heinr. Grebenau, who has submitted thirteen old sets of observations, made by Zandrini, Lecchi, Lorgna, Zimenes, and Brüning, and not accessible to us, to careful analysis, with the following conclusion: "It is remarkable that the law of decrease" (our parabolic law), "which is obvious in all of these earlier experiments, could have remained so long unknown." He also made some careful measurements himself on the Queichbach, near Gernersheim, which not only confirmed our parabolic law, but also verified our general equation for the parameter of the curve. Other observations of similar character might be named.

In his remarks upon the mean velocity of rivers, among other errors relating to the Mississippi Report, Mr. Henry has given a partial extract from a paper written by me for the Essayons Club of the Corps of Engineers, which wrongly conveys the impression that the

new data published in the work of Darcy and Bazin did not confirm our formula. In reality, I showed that they verified it in the most complete manner, and even afforded authority for extending its use to a class of small natural streams, with great slopes, to which it was not claimed to be applicable in the Mississippi Report. The valuable work of MM. Darcy and Bazin extended the table of trustworthy observations upon natural channels to a total number of seventy-three, only four-tenths of which were to be had in framing our formula; yet its average percentage of error is only six per cent., or about that of careful actual measurement. Our formula was based upon every observation we could procure, of an exact character, published either in this country or in Europe; and no "selection," except upon grounds which were fully explained, was made. That good judgment in this respect was used is sufficiently shown by these new Darcy-Bazin data.

Mr. Henry also gratuitously infers a "disagreement" in our Mississippi levels, which had no existence in fact.

In giving the results of his own slope measurement on the St. Clair river, and applying the several formulæ to them, he does not state—what his official reports show to be the fact—that the discharge was measured at widely different dates from the slopes, and hence, probably, bore no relation to them.

In proposing to substitute six-tenths of the depth for the mid-depth recommended by us as the uniform locus of velocity measurements for discharge, Mr. Henry has failed to appreciate that he would thus introduce a variable error due to wind effect. His own figures and computation show that our simple mid-depth method, when compared with his own laborious and costly operations, exacting observations at all depths, gave results differing only by seven-tenths of one per cent. in the discharge of the St. Clair, by two and seven-tenths per cent. in that of the Niagara, and by one-tenth of one per cent. in that of the St. Lawrence.

In now finally closing this discussion, so far as I am concerned, I would cheerfully accord much credit to Mr. Henry for his ingenuity in the application of electricity to recording the revolutions of a submerged meter axle. In this he has made a real contribution to the accuracy of hydraulic measurements in wide harbors and tidal ways, where operations from the shore are hardly practicable.