Mr. BIDDER, V.P., would desire to offer his thanks to Mr. Leslie, for the interesting Paper laid before the Meeting, and he was sure that his observations would be accepted in the spirit in which they were made, if he ventured to direct attention to some parts of the Paper, which, in his opinion, required revision before being printed in the Minutes of Proceedings.[1]

The Paper might be divided into two heads;—1st, as to the accuracy of the experiments themselves;—and 2nd, as to the extent to which they might be considered, as a test of the accuracy of the formula of Du Buat. With regard to the experiments,—in the cases of low velocities and flat gradients, due precautions did not appear to have been taken for guarding against obstructions, especially from the effect of the accumulation of air. For instance, in the second series of experiments, with a pipe 540 feet long, it was obvious, that the results could not be relied upon;—in experiment 1, with a gradient of 1 in 2,200, the flow of water was stated as ·3243, whereas in the preceding series of experiments, the flow was stated to be ·7407, with a flatter gradient of 1 in 2,391.

In Experiment 5, of the second series, the flow was stated to be 2·18, with a gradient of 1 in 220; whereas in the preceding series, in Experiment 8, the flow was stated to be 3·, with a gradient of 1 in 230. These were examples of the discrepancies, more, or less, pervading most of the experiments of the class.

With regard to the test of the formula of Du Buat, Mr. Leslie had adopted a formula, which omitted from it, all those corrections which were introduced by Du Buat, with the express view of meeting the case under consideration. On applying Du Buat's formula to the present experiments, the alleged discrepancies were, however, reduced;—for instance, in Experiment 1, first series, in place of being as 4 to 1, they were only about $2\frac{1}{4}$ to 1; in Experiment 2, in place of being 2 to 1, they were as 3 to 2; and in Experiment 4, in place of a discrepancy, as represented, of about 3 to 2, the results were nearly identical.

Referring to the experiments by Mr. Provis, quoted from the Trans. Inst. C.E., Vol. 2,[2] a serious error had occurred, in consequence of Mr. Leslie having omitted, in the deductions, to allow for the head due to the velocity generated in the pipes. That allowance being made, and the correct formula applied, the results were identical with those of Du Buat; affording a strong confirmation of the accuracy of that experimenter, not only as regarded the formula, but also as to the experiments upon which that formula was based.

[1] The Paper was submitted to the revision of the Author.—EDITOR.
[2] *Vide* Trans. Inst. C. E., vol. ii., page 201.

While on that subject, as there appeared to be, at present, a strong tendency on the part of Public Boards, to invalidate established formulæ, and to introduce others affording larger results, it would be well to direct the attention of all who desired to investigate the subject, to the article "Theory of Rivers," in Dr. Robison's Mechanical Philosophy.[1] By an attentive perusal of the article, they would not only be confirmed in their faith in the experiments and formulæ of Du Buat, but they would be satisfied how little the practical result depended upon whether, in point of fact, the flow of water was under-stated, even to the extent of 20 per cent.; for instance, in a culvert of 100 inches diameter, such a discrepancy would not influence its dimensions to the extent of $7\frac{1}{2}$ per cent., nor the cost of construction probably to the half of that per centage:—in fact, practically, below the margin allowed by all careful Engineers, for contingencies that could not be estimated. And after reading and understanding the article, they would appreciate the sentence,—"We must understand their motions, and their mode of secret, slow, but unceasing action, that our bridges, our wharfs, our dykes, may not become heaps of ruin. Ignorant how to proceed in these daily recurring cases, how often do we see projects of high expectation and heavy expense, fail of their object, leaving the state burdened with works, not only useless, but frequently hurtful?"

This quotation derived peculiar significance, from certain facts attending the publication of a pamphlet, proceeding from one of the recently appointed Metropolitan Commissioners of Sewers, and purporting to be a "Memorandum on the Data employed in determining the Sizes, and estimating the Cost of the Works, designed for the Main Drainage of the Metropolis," in which it was stated, —"De Prony's formula, applied to this latter class of cases, gives results which, as Claudel states, ("Formules," p. 110,) deviate in some instances from the truth, by no less than 29 per cent." But in reality, on reference to the authority there quoted, it appeared that no such passage existed, and the inference was entirely unsupported.[2]

Mr. Haywood observed, that probably at no other period of engineering experience, had there been more evident necessity for the accurate determination of the capacity of the large ducts, for the sewerage of large towns. If several miles of huge sewers, in the Metropolis, were constructed of too large dimensions, there might be an extra expenditure of 5, or 10 per cent., which was

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[1] *Vide* Robison's Mechanical Philosophy, vol. ii., page 388; edition, 1822.

[2] The passage did exist in the early editions of the work, but the inaccuracy of the statement was demonstrated, and it was expunged from all the editions, published after that of 1849.

the full extent of the injury; but if, by the adoption of empirical and incorrect formulæ, their dimensions were unduly restricted, the whole system might be a failure, and the expenses induced, would be enormous. Common sense indicated this evident result, and the experience of all professional men corroborated it.

Mr. HAWKSLEY coincided with the views just expressed, and remarked, that the formulæ published by authority, and insisted on by the Board of Health, gave results, differing very considerably from those of accepted practised experimenters, and men of admitted scientific attainments and mathematical knowledge; it would be desirable, therefore, to ascertain how and by whom these modern experiments had been made, in order to be assured of the degree of credence to be accorded to the results. Up to the present time, there had been no means of knowing how the results had been arrived at, nor of being assured of the competency of the persons employed; these were important elements, as nothing demanded more delicacy and habit of observation than hydraulic experiments, requiring profound scientific acquirements, as well as practical engineering skill, and if any elements were omitted, or any conditions were erroneously noted, the entire results would become useless.

Mr. BAZALGETTE, in answer to questions put through the PRESIDENT, explained, that it was not correct to attribute to the Metropolitan Sewers Commissioners, the experiments which were complained of; they had not been so made; but had been undertaken by a Committee called the 'Trial Works Committee,' by whom no official report had ever been formally made. The experiments were directed by Mr. J. Roe, formerly of the Holborn Sewers' Office,—Mr. Lovick, an officer of the Metropolitan Sewers,—and Mr. Medworth, then a clerk of Works under the Commission, and now keeping the 'Victory' Public House, at 167, Oxford Street. Records of some of the experiments did exist in the archives of the Sewers' Office, but he thought that no accurate results had ever been obtained from them.

Mr. R. STEPHENSON, M.P., V.P., would recall the discussion, to the question of the principles involved in the experiments; it was very desirable, if possible, to avoid even the semblance of personality, in any of the discussions at the Institution. He admitted, that in this particular instance, it was almost unavoidably necessary, to examine into the scientific qualifications of the persons conducting these extraordinary experiments, in order to arrive at an estimate of their value, or rather, he must say, their utter worthlessness; but all personal questions should, in general, be studiously avoided.

In the case of these experiments, it would have been more useful to have selected such as could have been relied upon, with a view to applying to the accepted formula, any correction that might be deemed necessary. The question was large and of much importance; it had engaged the attention of some of the first men of the age, and it had been hitherto presumed, that the results they had arrived at, were tolerably correct; it would be a benefit to science, to show that the discrepancies, assumed by the Board of Health and others, did really exist, in order that a question of such vital importance in the practice of Hydraulic Engineering, should be set at rest.

Mr. HAWKSLEY said, he had a motive in making, through the PRESIDENT, the inquiry with respect to the authority, upon which the conclusions of the Commissioners of Sewers and, subsequently, of the General Board of Health, had been arrived at. In dealing with a subject, which was not only one of great philosophical moment, but which, from its nature, was, at this time, of paramount interest,—one which demanded and had received the attention of distinguished philosophers and mathematicians, for a great number of years past, and to which they had devoted themselves, for the purpose of deducing formulæ for practical cases,—it certainly did become those, who professed to have made other experiments and to have arrived at other conclusions, to inform the world how the experiment had been made, and upon what data the conclusions were arrived at. Whenever any particular science had attained a certain amount of correctness, (which was the case with respect to Hydraulics), if experiments were made, with a view to check the deductions previously arrived at, it was obvious, that the persons who made those experiments, should be well versed both in the theory and practice of the science, as it was understood and accepted, at the time those experiments were made. When, moreover, there appeared in certain blue-books, conclusions which were entirely at variance with,—nay, utterly contradictory of,—all that was previously known, and admitted upon the subject, he felt that it was necessary for the world, to know the qualifications of the experimenters in this case, and for that reason, he had elicited the information relative to Mr. Medworth and the others.

He would elucidate his position, by directing attention to a few of the extraordinary conclusions arrived at by these new authorities, and the discrepancies they professed to have discovered in the existing formulæ. A Committee named in 1848, commenced what they termed, "a very elaborate series of experiments," under the peculiar notion, that little, if anything definite was, at that time, known as to the discharge of water through pipes. They soon arrived at the conclusion,—somewhat precipitately, it might be thought,—that water flowed faster through earthenware tubes, than it would do through tubes of almost any other material;—also, that it was not necessary, with a certain declivity in a pipe, to take into account the head of water, which might be above the

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mouth of that pipe;—also, that it was of no consequence whatever, in what way the tail of the pipe might be disposed of:—and then they laid down as a sort of rule, that, disregarding the circumstances at the head and the tail, if the pipe, with a given inclination, would, when receiving the water at the head, deliver a certain quantity, it would, if perforated at intervals along its length, and connected with branches at all those points, deliver a much greater quantity at the bottom, than could have passed through from the top. This was an extraordinary conclusion to arrive at, as a matter of science; but they arrived, at the same time, at the conviction, that nothing had been previously known with accuracy on the subject, and that they had made a remarkable discovery: and immediately, as a practical deduction, it was contended, that if a sewer had branches inserted along its sides, it would discharge under precisely similar circumstances to a pipe; and consequently, a greater discharge would be obtained at the end, than would be due to the sectional area of the sewer, and the declivity at which the water flowed through it;—a conclusion which, it was scarcely necessary to say, was erroneous, inasmuch as it was not in the nature of the case, that a sewer, working without any pressure, should discharge its water in the same manner as a pipe, working under pressure, and this, merely by virtue of the inclination at which it was laid.

Feeling interested in this question, and having had his name, in particular, brought into question both by the former Commissioners of Sewers, and also by the General Board of Health, who, in their books, quoted these experiments in justification of their own diminished diameter of pipes, Mr. Hawksley was desirous of knowing how and by whom they had been made; being a ratepayer, and having, therefore, a right to go to the office and inspect the books and papers, he did so, and the result of his investigation was very curious. He found, that several thousand experiments had been made; some were made in the Fleet sewer, some in Earl-street sewer, and some were made in the back yard of the office in Greek-street. He examined the whole (so far as they had been preserved,) of the books and papers connected with the experiments, and he must state, that he found at the office every disposition to aid him, and the most perfect order among such of the papers as had been collected. He endeavoured to discover the manner in which the experiments had been made, and to ascertain in what manner the conclusions had been arrived at. To a considerable extent,—although the documents were in a most imperfect state,—he succeeded; and he would attempt to give some of the results.

First mentioning, that the nature and the expense of the experiments, which were continued for about a year and a half, were equally preposterous, he found, that some of them had been made upon large glass pipes obtained, at great cost, from the best manufacturers in London, although it was well known, by all who had an adequate knowledge of the subject, as well as from the published experience of the best authorities, that such investigations could lead to no useful conclusion; as it was easy to show, that the friction in glass pipes was practically identical with that in brick sewers, or pipes of earthenware, iron, or lead. The cost of these experiments amounted, in some months, to £800, in others, to £900, in others, to £300, or £400, and on the whole, the expense of the experiments, made in relation to this and some other almost equally childish matters, was £7,189. Such an amount should have procured the finest sets of experiments, and the best-established data, the world had known; and yet, although the results were, indeed, given, they would not bear the most cursory investigation. Still they would enable the Members of the Institution, to judge of the value of the experiments. From one set of experiments, the conclusion was arrived at, that a pipe of 6 inches diameter, 100 feet long, laid perfectly level, (the inclination being 0 and the head 0,) would discharge 46 cubic feet in a minute!—in another set, with an appreciable fall of 1 in 800, which was not an unusual gradient,—(having established that with an inclination of 0, the discharge was 46 cubic feet per minute,)—the delivery appeared to reach only $46\frac{7}{10}$ths cubic feet per minute. These were the experiments which had been most dwelt upon. Other conclusions arrived at were, that, with a fall of 1 in 240, the delivery reached 50 cubic feet, and that an increase of 4 cubic feet only had been thus obtained on 46 feet,—the discharge of a pipe, with an inclination $= 0$; or rather, that if a pipe with so considerable a fall as 1 in 240, discharged 50 cubic feet, another similar pipe, having no fall at all, might be depended upon to discharge 46 cubic feet. Then going on to a double declivity, namely, to 1 in 120, they obtained the extraordinarily anomalous result of 59 cubic feet, although it was well known that, in such case, the discharge must necessarily increase in the ratio of $1 : \sqrt{2}$, or be upwards of 70 feet.

It was impossible to wade through the whole of these experiments, but the inevitable conclusion at which he arrived was, that there was nothing in all that mass of thousands of experiments, from which a single safe conclusion could be drawn. These experiments were ordered to be reduced by an eminent calculator, and Professor Airy, being communicated with on the subject, recommended for the purpose, Mr. Stoneham, a gentleman from the Royal Observatory, who appeared to have devoted much labour and attention, to reducing the experiments, and tabulating the results, which he accomplished in a very excellent manner. In course of time, this gentleman made a Report of his proceedings, and applied for payment for his services, which were ultimately valued at, and paid for with, the munificent sum of eight guineas! On inquiring, however, for this Report, it was announced to be missing, and the deductions and the diagram of this gentleman were alike absent; therefore, Mr. Hawksley was induced to look more fully into the Minutes of the Commissioners. Further research soon exhibited records of such extraordinary discrepancies and anomalies, that the Committee themselves had been startled, and had evidently ordered some of the experiments to be tried over and over again, until at last, the whole affair resulted in a perfect chaos. The Committee ceased to exist, and the Commissioners went out of office, without having come to a single useful conclusion. The diagrams of the experiments also exhibited most extraordinary results; and upon some of the documents, there were endorsements of this kind, "Not credible—Morgan Cowie," "Not credible—M. C.;" thus demonstrating the value attached to them by that mathematician, who was himself one of the Commissioners. The succeeding body of Commissioners, not knowing what their predecessors had been about, ordered that the papers should be collected, and reduced into a form for being printed; but when this was done, they unanimously decided, that the experiments were so indifferent, the results so anomalous, and the conclusions so worthless, that they would not sign the report, nor allow the book to be issued under their authority: and ultimately, they ordered the whole to be suppressed. In the meantime, however, some few copies did get into circulation, and Mr. Hawksley was fortunate enough to secure one, from which he made his observations to the Meeting.

In one part of the book, the very experiments to which attention had been directed, were the subject of remark, and that too, in a very singular way. There was Appendix B, in which it was said,—"In Table No. 5, the experimental results are given, with the calculated results of Mr. Hawksley."

He was naturally desirous to see this Table No. 5, but on turning over the succeeding pages, in which he found Tables No. 1, No. 2, No. 3, and No 4 duly recorded, he was astonished to find that, by some singular accident, No. 5 appeared to be wanting, and was the only Table that was omitted. This seemed to be very surprising, so he determined to search the records again, and then discovered the Paper, being the identical Table No. 5, from which he had quoted some examples. It was not necessary to comment upon this proceeding, nor indeed, should he have noticed it, but that at a subsequent period, the Engineers, as a body, were very generally attacked by another Public Board, on the ground, that they knew nothing whatever of Hydraulics; the premises from which that conclusion was arrived at, being the very singular proceedings just exposed, collected by the 'Trial Works Committee' of the Commissioners of Sewers. These data had thus been re-published, after they were known to be perfectly incorrect; nay worse, they had been twice re-published by Government authority, and they had been presented to Parliament in that official form, and had been circulated gratuitously by the Government, to the extent of eleven thousand copies; and upon that information, nearly every magistrate, every chairman of a local board, and almost every clergyman taking a part in public affairs, —every man, in fact, upon whom it was thought an impression, favourable to the conclusions of that authority, could be made, had been supplied with copies of these worthless books. The result was, that wherever any Engineer was consulted upon public works connected with Hydraulics, such as the supply of water to towns, or on a question of drainage, he was met by some one, antagonistically armed with one or other of these blue-books, who said, "You are not an Engineer connected with the Government, and therefore, of course, practise according to the erroneous data which were in vogue, before the discoveries of the General Board of Health." Then some one, who either really had the interest of the rate-payers at heart, but acted in equal ignorance and good faith, or who had his own little job to perpetrate, or who, while his real mission was to prevent any but his own scheme from being carried out, ostensibly professed to have the interests of the rate-payers at heart, said, "The plan which is submitted, is not a plan which can be adopted,—it is a plan which will occasion enormously unnecessary expense, for it is stated in the blue-book, that you can pass three times as much water through a pot-pipe, as through a brick sewer; and therefore, if you employ a Board-of-Health Engineer, you will not want more than one-third of the area, which this gentleman requires." The consequence of all this, was great confusion and contention, resulting generally in great expenditure, under the guise of economy, and ending in lamentable failure; because forsooth, it was stated, that these data, coming from the Government, must be considered as authentic. Now all this evidently proceeded from the present mischievous tendency of unscientific amateurs, who, being appointed to Government offices, took upon themselves to meddle with purely professional matters, and employed persons still more ignorant than themselves, to perform experiments and design works, which required and had occupied the attention of men of the highest scientific and practical attainments.

Now, turning to Mr. Leslie's Paper, there appeared to be a great number of experiments and calculations, of great value in relation to the subject to which Mr. Hawksley had adverted, and the more so, because there had been deduced, what were considered to be certain discrepancies between established formulæ, and the results of the experiments now brought forward. These experiments were made with care and due attention to one important point; for Mr. Leslie was aware, that in determining the velocity of water through pipes, it was necessary to ascertain, not the apparent declivity of the pipe, but the actual declivity,—the actual difference of level between the surface of the water at one end of the pipe and the other. The Paper was written in a most honest spirit, and had reference, to what the Author had conceived to be, certain discrepancies between existing formulæ and the results of the new experiments, quoting what was therein stated to be Du Buat's formula. This latter was, however, erroneous, as would be seen by comparing the formula given in the Paper, with that given by Du Buat's original commentators.[1]

Now the singular fact elicited by this was, that when Du Buat's own formula was really employed, the results coincided in a remarkable manner, with those produced by Mr. Leslie as corrections; for instance, with a pipe of $2\frac{1}{2}$ inches diameter, 1,086 feet long, the discharge was, according to Leslie, 0·44, and according to Du Buat's formula, it would have been 0·48; thus showing extraordinary coincidence, even under extreme circumstances. Taking other examples,—

	Head.	Result by Leslie's Experiments.	Result by Du Buat's Formula.
	Ft. in.		
The discharge under . .	2 $9\frac{3}{8}$	2·22	2·1
	4 $9\frac{1}{8}$	3·00	2·85
	7 $0\frac{1}{8}$	3·53	3·50
	9 $11\frac{7}{8}$	4·28	4·30
Total .	. . .	13·03	12·75

[1] Du Buat's formula for the velocity, expressed in English inches:—

$$V = \frac{307\,(\sqrt{d} - 0{\cdot}1)}{\sqrt{s} - \mathrm{L}\sqrt{s + 1{\cdot}6}} - 0{\cdot}3\,(\sqrt{d} - 0{\cdot}1),$$

in which, V represents the mean velocity,
d „ the hydraulic mean depth,
s „ the denominator of the slope of the pipe,
L „ the hyperbolic logarithm of the number to which it is prefixed.

Dr. Young's formula for the velocity, expressed in English inches:—

$$V = \left(\frac{d\,s}{a} + \frac{c^2}{a^2}\right) - \frac{c}{a},$$

in which, V represents the mean velocity,
d „ the diameter of the pipe,
s „ the sine of the inclination,

$$c = {\cdot}0000001\left(\frac{900\,d^2}{d^2 + 1136} + \frac{1}{\sqrt{d}}\left(1085 + \frac{1321}{d} + \frac{1{\cdot}0563}{d^2}\right)\right),$$

$$a = {\cdot}0000001\left(413 + \frac{75}{d} - \frac{1440}{d + 12{\cdot}8} - \frac{180}{d + {\cdot}355}\right).$$

Many other examples might be given; but these would suffice, to show the perfect coincidence of the formula with experiments, to which there could not have been any attempt to adjust them; they demonstrated also, that the experiments, as well as the deductions of the 'Trial Works Committee,' and the conclusions of the General Board of Health, must have been totally wrong, and as such, that they ought to be expunged from any book used by Government authority, and not be any longer enforced as the groundwork of conclusions, upon which Engineers should base any practical operations. It was not necessary to dwell further on the subject of these errors, except to draw attention to their influence upon practical operations, up to the present time; more especially, in the drainage of the Metropolis. Each body of Commissioners of Sewers appeared to come into office with foregone conclusions, or to arrive, whilst there, at conclusions different to their predecessors, since all these Commissions had meddled with matters, with which they had no business to have interfered; namely, the data on which their practical men should have proceeded to execute works. Thus, from 1847 to 1855, nothing useful had been done, but vast sums of money had been expended on trivial experiments, and a great deal of paper had been spoiled, by having printed upon it, the worse than useless discussions of the Commissioners. In reference to this latter point, Mr. Hawksley would only direct attention to a pamphlet, now being circulated rather extensively. It was entitled, "Memorandum on the Data employed by Mr. Bazalgette, in determining the Sizes, and estimating the Cost of the Works, designed for the Main Drainage of the Metropolis." Now the character of this pamphlet, wherein Mr. F. O. Ward, a medical man, and one of the Commissioners, was found taking up the subject of scientific data, was stamped by one single paragraph.

The pamphlet said—

"Mr. Bazalgette has, I think, fallen into unintentional error, in his mode of applying the formula he employs, to calculate the sizes of the great intercepting sewers.

"He (Mr. Bazalgette) says ('Data,' p. 4), that the various formulæ propounded, lead practically to the same results. So far is this from the truth, that De Prony's formula, based on the results of Du Buat's experiments, leads to conclusions totally different from those obtained by Eytelwein's formula, with the modified coefficients which he deduced from the experiments of Brünings, Woltmann, and Funk, on larger streams of a wider range of velocities. De Prony's formula applied to this latter class of cases, gives results which, as Claudel states, ("Formules," p. 110),[1]

[1] The passage alluded to is, "La formule de Prony, modifiée par les nouvelles valeurs de *a* et *b* d'Eytelwein, convient mieux aux cas des grandes rivières; mais

deviate in some instances from the truth, by no less than 29 per cent. The approximate formula adopted by Mr. Bazalgette, and applied in the manner he describes, to determine the discharging power of each tunnel, 'at all those points where a change in its dimensions has been made,' leads necessarily, I think, to fallacious conclusions."

Feeling curious upon this matter, Mr. Hawksley referred to the latest edition of Claudel's book, which was a collection of formulæ, or a handbook of engineering data, compiled by a clever man, but, as was too commonly the case, a mere book-maker, who had never made any original experiments. After ineffectually seeking for the passage, quoted with such confidence by Mr. Ward, it turned out, that the passage was expunged from every edition after that of 1849; in consequence, apparently, of its incorrectness having been demonstrated. Now, when amateurs undertook to correct professional men, they should at least be well assured of the truth of the theories they promulgate. All reflecting men would, no doubt, concur with him in thinking, it was a sad conclusion to arrive at, that such a large, important, and imperatively demanded work, as that of the drainage of the Metropolis, should be impeded and delayed, whilst Commissions were, for years together, debating, whether one formula or another was the right one to apply, or whether there was a little per centage of difference, between any two sets of experiments. It was evident, that this sort of obstruction might go on for an indefinite period, and the results might be, that within an ordinary life-time, the public would not have the benefit of the existing advanced state of scientific knowledge.

Reverting again to Mr. Leslie's experiments, they appeared to be of great value in fixing that which had been doubted, viz., the increased discharge which occurred in practice, from orifices elongated by the application of a short tube. The formula in use amongst practical men, gave a co-efficient of 6·6, and Mr. Leslie's experiments showed the co-efficient to be 6·7.

Going to another branch of the subject,—the discharge of large and long pipes,—the result of the experiments on the Crawley pipe was, that the pipe delivered less than the formula, but that was to be accounted for, by its being old and very much encrusted: while with respect to the Colinton pipe of 16 inches diameter, Du Buat's formula gave a discharge of 604 cubic feet, and by Mr. Leslie's experiment, the discharge was found to be 600 cubic feet.

With the view of testing the accuracy of the co-efficients employed by Civil Engineers, for the discharge from sluices, Mr.

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elle ne s'applique pas egalement bien aux quatre-vingt-onze expériences discutées par Eytelwein; ainsi, pour vingt d'entre elles, les vitesses données par la formule, s'écartent de 0·11 à 0·29 des vitesses observées."—Formules, etc., par J. Claudel. p. 110. Paris, 1849.

Leslie had made some experiments on a very large scale, on the shuttles of the Docks at Leith, and in the Monckland Canal. The formula generally used, had a co-efficient, for the discharge in feet, of 5·0, but theoretically, it was given as 5·3, as the true formula, with little friction; out of seven experiments, the first four gave 5·3, and the other three gave 5·25. Then some further experiments were made on the discharge of water, as in the open channel of a river; and for that purpose, Mr. Leslie employed a uniform channel, 2 feet in width, with rectangular sides, and a bottom of smooth stones, having a regular fall of 1 in 1000, made for conducting water to Dundee, with these results:—

Depth of Water in Channel.	Calculated Discharge by accepted Formula.	Actual Discharge.	Actual Average Ascertained Velocity.	Velocity by Floats at Surface.
Inches.	Cub. feet.	Cub. feet.	Lineal feet.	Lineal feet.
6	109·6	110·09	110·09	128·5
7	134·	134·83	116·	129·7
8	160·	162·16	121·6	133·1
9	186·	184·61	123·	136·7
10	213·	214·28	128·6	138·1
11	240·3	240·	131·	140·
12	268·	266·6	133·3	146·7
Totals .	1310·9	1312·57		

There was some little variation each way, not because the formula was wrong, for being right in two or three instances, it would be right in all the others, but it was natural to suppose, that the observations of the experiments would err a little; therefore, he must assert, that Mr. Leslie's Paper proved the most complete establishment of the existing data.

Although much still remained to be said on the subject, it would scarcely be necessary to do so at that time, especially as he purposed going more deliberately, on another occasion, into the description of how the formulæ were arrived at, by which these calculations were made; he was confident, that whatever might be the errors of the Author, alluded to by Mr. Bidder, they would be found to arise from some misconception on the part of Mr. Leslie, relative to the formula stated to be that of Du Buat, and that the errors were capable of satisfactory correction, before the Paper would be published. The Institution might, at all events, be satisfied in having in its possession, at the present moment, every kind of scientific data necessary for carrying out any hydraulic works that might be required.

Mr. Beardmore, on behalf of the Author of the Paper, said, it must be remarked, with respect to the alleged discrepancies in the second series of experiments, that instead of impugning the results, they rather proved the honesty of the record, and demonstrated their useful character, whilst they pointed out the difficulties to be encountered in making accurate hydraulic experiments, and where failures might be anticipated, in their application to engineering practice.

In reference to the formula commonly used for the discharge of pipes, he contended, that the rules adopted by Prony, Eytelwein, Poncelet, and others, were all substantially the same, varying only in the constant for friction; 45·5 being the lowest, and 50·, the highest constant, for feet per second, now more commonly used, and referred to as Du Buat's, in the Author's Paper; but he must further contend, that the formula of Du Buat provided for the varying diameter of pipes, and also for the reduction of discharge by the loss of head, required for overcoming friction, at flat rates of inclination, in a manner similar to, but much more complicated, than the plan proposed in the Paper.

The conclusions of Du Buat and of the Abbé Bossut, a previous writer, were founded on experiments, detailed in a scientific Paper by M. Couplet, the Engineer of the Versailles Water-works, in the year 1732; and those experiments were confirmed in a remarkable manner, by the large practical conclusions given in Mr. Leslie's Paper.

He maintained also, that the conclusions of all mathematical writers of the present century, were based on the formula of Du Buat; but that Prony, Eytelwein, Poncelet, Robison, and the elder Leslie, as well as the Engineers of the present day, had all agreed, in practice, in omitting the more complicated part of Du Buat's formula. In verification of this, he gave a Table (page 301), showing comparisons of the French experiments, from 1732 down to those recently made by the Author, and exhibiting the most striking coincidences of theory with practice; the variations, for practical purposes, in the different rules being small, and the correction proposed by the Author, affording an excellent application of the principles adopted by Du Buat, for providing the most ample allowance in extreme cases, such as all Engineers must meet with in hydraulic operations.

Dr. Young's original formula, as given in the "Encyclopædia Britannica," 1802, was a simplified form of Du Buat's; but, in a further communication to the Royal Society, in 1808, in reference to investigations on the Circulation of the Blood, he applied certain difficult corrections to this formula, with the object of adapting it to small pipes. Calculations by the formula, so corrected, were most tedious, and gave results which differed only by minute quantities, from those obtained by the simple form of expression, used by the Author of the Paper, in common with other Engineers.

To show this clearly, a few of the Author's experiments had been calculated, by this and several other formulæ, and the results were given in the subjoined Table, in which the formula, designated as 'Young's, 1802,' was actually, that now commonly known as Eytelwein's:—

Diameter of Pipe.	Length.	Fall.	Rate of Fall, 1 in	Discharge.				
				Actual Discharge.	Du Buât, 1783.	Young, 1802.	Young, 1808.	Leslie, as proposed.
In.	Ft.	Ft. in.		Cubic feet per minute.	Cubic feet per minute.	Cubic feet per minute.	Cubic feet per minute.	Cubic feet per minute.
$2\frac{1}{2}$	270	0 $3\frac{1}{8}$	1,037	1·017	1·220	1·423	1·581	1·165
$2\frac{1}{2}$	270	2 $5\frac{5}{8}$	109	4·615	4·420	4·382	4·731	4·355
$2\frac{1}{2}$	100	2 $5\frac{1}{8}$	41·1	7·058	7·920	6·933	7·046	7·165
$2\frac{1}{2}$	100	0 $11\frac{3}{16}$	107	4·138	4·476	4·297	4·236	4·287

Mr PHIPPS said, that in offering a few remarks upon the Paper before the Institution, he would first allude to the experiments on the flow of water through pipes, on the large practical scale of those derived from the trials on the pipes of the Edinburgh Waterworks. To these he attached great importance, not so much on account of the higher interest which direct experiments possessed, when made to illustrate any given facts, but because it was to such larger applications of principles to practice, as in the cases above referred to, that the practical Engineer would rather look for his guidance.

From a careful examination of the experiments, especially the larger cases of supply from the pipes of the Edinburgh Water Company, he was led to concur in the main principle contended for in the Paper, that for velocities of efflux which differed to a great degree, no rule could be correct, which did not recognize some varying power of the velocity, for showing the amount of friction due to those different velocities. Whether such varying powers of the velocities, had generally been used by the Engineers of this country, it would be for the leading Hydraulic Engineers to say; but he presumed, that in general practice, such had not been the case, and this might be accounted for, to a certain extent, by the fact of the velocity of flow in pipes being usually circumscribed by such limits, as, generally, to prevent any great variation in the velocity. In all instances when the water was to be pumped, this certainly was the case, as under such circumstances, the velocity could not be allowed to vary much beyond 3 feet to 5 feet per second, without a large sacrifice on the score of economy.

Although recognizing the necessity of introducing into the formulæ for the flow of water, some modifying quantities, to meet the case of great departure from a medium velocity, either greater or smaller, it must not be supposed, that this idea had now been broached for the first time. The question was freely discussed in the works of D'Aubuisson and Weisbach;—the first of these Authors suggested, that the law of the increase of friction was, as the square of the velocity, plus a certain addition of the velocity itself, (which resembled somewhat, the mode proposed by Mr. Leslie);—whereas, Weisbach proposed a law of increase, compounded of the square, plus the square root of the cube of the velocity. The determination of which of these different systems would, on the whole, prove to be the best, would require a long and patient research, and could only be successfully prosecuted, by a good theoretical and practical Hydraulician. It did, however, appear from an inspection of the figures, that the method proposed by Mr. Leslie, approached nearer the truth, than the simple unmodified formula generally used by Engineers.

Mr. Phipps could not, however, agree with Mr. Leslie, in fixing a given gradient of about 1 in 100, as the point from which, whether above, or below, it was necessary to begin to apply the modifying power of the velocity; for surely this gradient must vary with the different diameters of pipes which were used. On the same gradient, two pipes, whose diameters were as 1 to 4, would maintain velocities as 1 to 2, and as the velocity was the source of the varying resistance of friction, would it not be more natural, to apply the correctional quantities on each side of equal velocities?

While on the subject of the varying powers of the velocities, representing the amount of resistance of friction, he conceived it would be interesting to compare the friction in pipes, with that known in vessels by the term, 'skin resistance,' as it appeared to e an analogous case.

He found, by comparing the experiments on the mains of the Edinburgh Water Company, with the experiments of Colonel Beaufoy on the resistance of bodies moved through water, that there was a marked identity of the diminution of the law of increase from that of the squares, as the higher speeds were attained;—and another fact, which seemed worth attention, was, that the resistance, per square foot, of the side of a ship, was very nearly only one-half of the resistance on a superficial foot of the inside of a pipe, at the same velocity. Whether this had any reference to the mass of water around the ship, as compared to the contents of a pipe, might perhaps be worth attention. The calculations he had made upon the mains of the Edinburgh Water Company, were confirmatory, in general, of the views propounded by Mr. Leslie.

The Crawley pipe, which was 44,400 feet long, 15 inches in diameter, with a head of 226 feet, at an inclination of 1 in 196, yielded 255 cubic feet per minute.

Now he found, that the actual velocity per second, was = 3·46 ft.
The velocity, calculated by the formula

$$\sqrt{\frac{2500 \times dh}{l + 50\,d}} = 3{\cdot}93 \text{ feet.}$$

The actual resistance on a square foot . .	= ·0983lb.
Calculated by the law of the square, starting from the next example as zero	= ·0722,,

It was clear in this case, that, as the real resistance per square foot, was greater than was given by the law of the squares, the discharge would not be effected with the above pressure, or equal head.

The Colinton pipe, of 16 inches diameter, and 29,580 feet long, with a head of 420 feet, yielded 571 cubic feet per minute; at an inclination of 1 in 70·5.

The actual velocity per second . . .	= 6·8 feet.
The velocity, calculated by the preceding formula	= 6·9 ,,
The actual resistance on a square foot . .	= ·289 lb.
The calculated ditto, by the law of the squares	= ·289 ,,

This example was taken, as showing where the two modes of computation agreed, the velocities being equal.

From Torduff Cistern to Castle Hill, the length of the pipe was 25,765 feet, under a head of 230 feet; the diameter was 16 inches, and the yield was 440 cubic feet per minute, at an inclination of 1 in 112.

The actual velocity per second .	= 5·28 feet.
The velocity, calculated by formula .	= 5·43 ,,
The actual resistance per square foot .	= 0·181 lb.
The calculated ditto, by the law of the squares	= 0·168 ,,

From Clubbie Dean to Torduff Cistern, the length of pipe, of 16 inches diameter, was 3,815 feet, under a head of 184 feet; and at an inclination of 1 in 20·7, the yield was 1,215 cubic feet per minute.

The actual velocity per second .	=15·3 feet.
The velocity, calculated by formula .	=12·6 ,,
The actual resistance per square foot .	= 0·98 lb.
The calculated ditto, by the law of the squares	= 1·48 ,,

It was clear, that if 0·98 lb. would give a velocity of 15·3 feet per second, 1·48 lb. would give more than 12·6 feet per second; and, therefore, that the law of the squares gave too low a result, at this rapid inclination.

BEAUFOY'S EXPERIMENTS.

(X) Constant { 0·0031597 lb. per foot per second. / 0·009025 lb. per nautical mile.

Nautical miles per hour	2	3	4	5	6	7	8
Feet per second .	3·38	5·07	6·76	8·46	10·15	11·84	13·53
	lb.	lb.	lb.	lb.	lb.	lb.	lb.
Pound per square foot per experiment	0·0432	0·0884	0·1444	0·2086	0·279	0·354	0·4316
If increased by the squares . . .	0·0361	0·08125	0·1444	0·2256	0·3249	0·4422	0·5776

In the above, 4 nautical miles per hour = 6·76 feet per second, were taken as the starting point, or zero, as being nearly the same velocity as the experiment on the Colinton Pipe, viz., 6·8 feet per second.

Mr. RAWLINSON expressed his disbelief in the results of the experiments of the older Hydraulic Engineers, "excepting they were confined to pipes, filled by a single head and having a parallel bore." His own professional experience induced the conviction of modified formulæ being necessary, under certain circumstances, such, for instance, as the accession of lateral feed pipes, by which results had been obtained, greatly at variance with received opinions. These conditions,—several heads, or inlets,—he repeated, required to be taken into consideration, in providing for practical works, and introduced new elements affecting the cost of works. "Pipes had capacity, as the squares of their diameters, and delivered water, as the square root of the head, if the feed would allow a full bore at the discharge."

Mr. PHIPPS inquired of Mr. Hawksley, whether it was to be understood, that his modification of the formula was applicable to all cases? He asked, because it did not appear to him, to have any provision for cases differing in velocity, and, therefore, it was presumed to be taken for the mean velocity.

Mr. HAWKSLEY was not aware of ever having stated, and he certainly never intended it to be understood, that the formula was applicable to all cases. It was applicable, however, to all cases, which fell within the ordinary practical application of Hydraulic Science; but it was not applicable for those extreme cases of minute diameter and sluggish velocity, in which, what ought to be really termed the friction of water, required to be taken into account. What was usually denominated friction, by writers on Hydraulic Science, was not friction at all, but was a resistance of impact, which varied as the square of the velocity, and that was the great resistance experienced in the conduct of water, and was that given in the formula. In addition to this, there was another resistance ,which only varied as the simple velocity,—the resistance of adhesion, or viscidity, as it had been called, but in reality, of friction proper. This brought him to an important experiment on the subject of resistance of water through pipes, or sewers, and he would endeavour to explain familiarly, that which was practically well known, viz., that the resistance was, within certain narrow limits, independent of the surface of the material over which the water flowed. This arose, from the fact of the first particles of water moistening the surface, and forming, as it were, a tube of water, through which the main body of the fluid rolled along, with only such an amount of friction, as was due to the impact of the globular particles upon each other, and upon those of the fluid adhering to the internal surface of the pipe. If, therefore, the tube was of brick, or vitrified clay, or of any other porous material, or even of glass, the water would always adhere to its surface, and so, by providing, in all cases, an annulus for the cylinder of water to move through, afford similar discharges of water. It was always assumed, as a matter of course, that water running through a pipe, encountered some resistance, which was to be provided for; but not in different degrees for a fire-clay pipe, a pot-pipe, an iron pipe, or, as in the case of the trial works of the Metropolitan Commissioners, for a glass pipe; as there was, in all cases, the annulus intervening, between the external shell and the moving fluid. As to the total resistance, it must be borne in mind, that all fluids were composed of particles, arranged with respect to each other as a series of globules, which, when in motion, had to travel over similar globules, losing somewhat of their initial velocity, by their impact upon the particles retained near the sides of the pipes, and also by rubbing against them. Consequently, there was first, the resistance to impact, which was as the square of the velocity, and secondly, the friction of the particles rolling over each other, and with that compound action Du Buat, and after him, in a more complete form, Dr. Young had dealt; the latter Author had indeed introduced the real friction into the equation, under its proper term. Dr. Young had dealt with the two conditions of resistance, the one varying as the square of the velocity, and the other, as the velocity itself; but as regarded all those diameters of pipes which occurred in practice, inasmuch as the friction proper, only occurred in a small degree at the immediate margin of the pipe, and was only as the velocity itself, it became, in fact, almost inappreciable in ordinary cases, and therefore, all Hydraulicians had agreed to omit that quantity from the practical equation. The result was, that on applying this equation, as modified for rivers, or ordinary cases of pipes in use in water-works, the formula would give so correct a result, that in practical applications no sensible errors could be detected.

Mr. PHIPPS begged to inquire, to what velocity the formula was particularly applicable? Mr. Leslie had not appeared to find it agree, at extreme velocities. In the experiments on the large pipes at Edinburgh, the formula did not appear to give the proper quantities, and thus error had been induced. The statement of the friction and impact, was not satisfactory to him, for when all the globules were proceeding in one direction, the impact being also in the same direction, and the whole mass in motion simultaneously, there could not be any reaction, or resistance, except from the particles adhering to the periphery. The precise action of the spherical globules required investigation.

Mr. HAWKSLEY said, a familiar instance was, the action of drops of rain upon the window of a railway carriage in motion. According to the law of gravity, these drops should descend, but they did not do so, although the carriage was travelling at a high velocity, and with a great amount of tremour, unless one drop fell on another, and then they acquired an increased amount of weight, and fell. The adhesion to the glass was produced by the action of the law of aggregation; the power of attraction, up to a certain point, exceeding that of gravitation. A drop of water would adhere to the finger, and in the same manner, the particles of water adhered to the interior of a pipe; therefore, they were not all travelling together, but certain particles in contact with the material of the pipe, were retained, and the inner particles propelled over them, each particle, or filament of particles, moving more rapidly as it was further removed from the resisting surface of fluid, retained by the attractive force of the material, of which the pipe was formed. The manifest effect of this intervention of Nature, was to give to pipes, constructed of whatever material, the same power of resisting fluid motion. It was, therefore, matter of indifference, in an engineering point of view, whether the resistance of the fluid moving along a pipe, was derived from an earthenware, or a brick, or a metallic, or a vitrified surface: in all practical cases, it would be the same. A knowledge of this simple fact, would have saved the Commissioners of Sewers some thousands of pounds, expended by them in useless experiments, instituted for the purpose of obtaining impossible results,—would have prevented the loss of years of valuable time, occupied in unprofitable debate,—and would have spared the Government the ridicule and contempt, with which the absurd theories and lamentable failures of the General Board of Health, had overwhelmed them.

Mr. BAZALGETTE had occasion, some years ago, to gauge the quantity of water discharged by the rivers around London, and had used as tests, floats for the surface, the flow over weirs and sluices, and also the current meter, and had repeated the same experiments in sewers; and in all cases, when compared with the calculated result by the formula, he had been surprised at the coincidence. The profession was much indebted to Mr. Hawksley, for his formula, for determining the sizes of sewers for town drainage, the accuracy of which had been thoroughly tested, and proved to be worthy of the confidence accorded to it. Mr. Wicksteed, in his Report on the Drainage of Croydon, had not only made some remarks entirely confirmatory of the utility and accuracy of the formula, used by Engineers, for determining the velocities of flow through sewers and pipes, but had anticipated many of the opinions elicited during the present discussion.

The passage alluded to, was the following:—"Previous to 1835, I had relied upon the well-known accuracy of the late Mr. Smeaton, and had used his rules and data, for calculating the velocity and discharge of water through pipes. In the summer of that year, having a very good opportunity of testing these rules and data, I found them remarkably accurate. The experiments I was enabled to try, were upon two miles of 9-inch pipe, a portion of which had been laid for several years, and the remainder had been laid recently. There were several bends on the line of main, which communicated between the East London Waterworks Reservoir, at the top of Stamford Hill, and their works at Lea Bridge, the difference of elevation between the two, being from 90 to 100 feet. The results obtained were extremely satisfactory, exhibiting a discharge somewhat greater than the data given by Mr. Smeaton, whose experiments were tried upon smaller-sized pipes.

"Subsequently, I had an opportunity of trying numerous experiments, upon the discharge of the mains of the Kent Waterworks Company, between Deptford and Woolwich. The length of main was seven, or eight miles; the diameter of the main commenced with 18 inches and reduced to 6; and the difference in elevation was about 220 feet. The result of numerous trials showed the accuracy of Mr. Smeaton's rules; but, owing, probably, to the various bends in the mains, the discharge was not quite so great as in the experiment at Stamford Hill, where the main was shorter in length, had fewer bends, and was of the same diameter throughout.

"In constructing my tables, therefore, for general use, I have added about 10 per cent. to those of Mr. Smeaton, considering, practically, that it would be better to have a pipe, or sewer one-tenth too large, than one-tenth too small. When, therefore, I find other Engineers calculate upon a result, 10 per cent. greater than my Tables, I do not think it impossible, that they may be accurate, but, practically, I consider mine the safest rule."

It had been argued, that the formulæ in question were not applicable to a ramification of sewers, because the branches discharging into a main sewer, increased the velocity in it, by their greater fall, and therefore, the formula gave smaller results than were absolutely correct. Now Mr. Bazalgette found in practice, that whilst some branches, with perfect junctions and rapid velocities, accelerated the flow in the main sewer, other branches, with defective junctions, or a sluggish flow, retarded the velocity of the main stream, and that it was, therefore, unsafe to calculate upon any permanent increase of velocity, from such varying and conflicting conditions. The only increase of velocity, which could, under such circumstances, be fairly estimated, was that due to an increase of hyraulic depth.

Mr. BEARDMORE said, he had frequently made experiments, on a large scale, with the view of testing the general consistency of the formula used by Engineers, both for the discharge through pipes and for the flow in rivers and artificial channels, by observations of velocities and the corresponding surface slopes, and *vice versâ;* and also for the discharge by large sluices and weirs. The first case he had before remarked upon, and he would now only observe, that in testing experimental discharges by formulæ, the most perfect accuracy was necessary in the experimenter, as small practical defects of construction, or errors in observations of the levels, or of the duration of the experiments, were liable to cause serious discrepancies. He had noticed recently, the case of a pipe, 42 inches in diameter, and about 2 miles in length, for conveying water by gravitation; the fall in the whole length, being about 10 feet. Perpendicular air-pipes, of about 1 foot 6 inches diameter, were placed at short intervals along the main, so that the water-surface was freely self-adjusted; and on examining the surface, he was surprised to find, that it was in a constant state of oscillation, amounting, at times, to as much as 4 inches. The Engineer of the East London Water-works, would probably be able to give the results of his experiments on these pipes, as the case was peculiarly adapted to test the formulæ in use, and to discover their defects, if any existed, at low velocities.

The second case, of open river channels, was the converse of the former, because there, Nature adjusted the entire surface of the water, which travelled at velocities generated by the surface slope, and modified by the friction of the channel. Under favourable circumstances, the experiments he had made, for testing the surface slope and corresponding sectional velocities, agreed strikingly with the established formulæ, referred to in the Paper, and adopted by himself in his professional employment. There were circumstances' however, which frequently puzzled, and obscured experiments, in a manner that would surprise an inexperienced observer; such as undulations caused by wind, the discharge of locks of water at either end of the reach, which was being experimented upon, (in the case of a canalized river,) &c. In fact, in ordinary rivers, the section, the surface slope, and the velocity, (any two of which would, by calculation, give the third,) were perpetually varying. As a matter of practice, however, gauging was by far the most certain system for obtaining the actual velocities.

The third case, sluices and weirs, was less difficult of observation and calculation; and here, the discrepancies from formulæ were, consequently, less than in the two former cases, errors of observation being less marked. But generally, experience showed, that, even in this case, the surface levels were extremely susceptible, and frequently produced results not expected from the formula; caused, in the case of weirs, by the extreme difficulty of recording the actual state of the surface curve of the water, between its state of rest, and the point where it passed over the outer edge of the waste-board.

Mr. HAWKSLEY, in answer to questions, stated, that his attempts to arrive at some definite information, as to the results of the experiments instituted by the 'Trial Works Committee,' upon the flow of water through tubes of various materials, had been entirely frustrated by the absence of the Report and of the reductions made by Mr. Stoneham; neither of which could be found among the Archives of the present Metropolitan Commissioners of Sewers. This was unfortunate, as it would have been interesting to have compared the results of these comparatively recent trials, however imperfectly made, with those of Du Buat, Dr. Young, and other well-known experimenters, who had tried lead, iron, and even glass pipes, and had arrived at the conclusion, that the discharge was irrespective of the nature of the material of which the pipes were composed.

Mr. Hawksley had also made many experiments, both with pipes and sewers, and invariably with the uniform result, that there was no practical difference in the discharge from conduits of similar figure, of whatever material they were constructed.

He found, that small rivers generally discharged less than was shown by the formulæ: this could easily be accounted for. It arose from the channel not being parallel,—the bed not being uniform,—the presence of impediments on the sides and bottom, causing curls in the water,—and from weeds and other natural obstacles existing in all water-courses. It was well understood, that these uncertain elements could not be introduced into calculations, therefore, an allowance of 5 per cent. was generally made, in applying the formula to small rivers.

The impediments enumerated did not affect large rivers, in the same ratio; therefore, the formula given by Mr. Leslie, might be received as adapted to them.

In applying any formula, the channel could only be presumed to be uniform between given points; and for want of knowledge how to make due allowances, many alleged inaccuracies had been attributed to the formula, which were, in reality, due to the ignorance of those who used it.

Mr. HAYWOOD said, that in connexion with, or about the time of, these remarkable experiments of the 'Trial Works Committee,' it was given in evidence by Mr. John Roe, that glazed earthenware tubes gave an increased velocity of 15 per cent. over that in brick sewers, at the same declivity; now it was currently stated, that out of the experiments made, all the inconvenient results had been suppressed. He had a written statement from Mr. Buckwell, (a manufacturer of a material called 'Granitic Breccia Stone,') to the effect, that he supplied pipes of that material, for the experiments of the Committee, and that the results obtained through his tubes, which were very smooth and straight, were, that 25 per cent. more water passed in a given time and under similar conditions, than through glazed stoneware pipes; and he did not think himself well used, in not having had the results of these experiments published. Now at the period of those experiments, stoneware pipes, and especially glazed stoneware pipes of Lambeth manufacture, were being much used, and it might not have suited, to show that such a result was obtained through unglazed pipes. It would have been desirable to have had at the Meeting, Mr. Roe, or Mr. Lovick, and others who conducted those experiments, as they might have been able to afford some information upon these points. Mr. Haywood's own experiments and observations convinced him, that the nature of the material used was unimportant, if the conduits were of adequate area and constructed with practical skill.

Mr. J. R. McCLEAN said, the remark in 'Weisbach's Mechanics,' that "Experiments with $2\frac{1}{2}$ and $4\frac{1}{2}$ inch wide common wooden pipes, have given the Author the co-efficient of resistance, 1·75 times as great as for metallic pipes;"—must be received with qualification; inasmuch as the position was not supported by details of the experiments, the results of which had probably been influenced by peculiar circumstances, and they were at variance with all others, including those upon water flowing through wooden troughs.

He directed attention to a Table, published by Mr. J. Thomson

Table.

FRICTION OF WATER IN PIPES.

Table, calculated from a Formula of Julius Weisbach, to show, for Pipes 100 Feet in Length, the relation between—

1st. The Velocity of the Water, in feet per second.—2nd. The Internal Diameter of the Pipe.—3rd. The Head to overcome the Friction, in feet.—4th. The number of Cubic Feet of Water delivered per minute; so that, when any two of these four quantities are given, the remaining two can be found.

This Table also gives, by the first and second columns, the relation between the velocity and the head required to produce the velocity, calculated by the laws of falling bodies, independently of friction, contraction by the vein, or other retarding causes.

The Formula of Weisbach, used for this Table, is given in his Ingenieur-und-Maschinen-Mechanik, vol. i. p. 434, and when reduced to English measure, is as follows:

$$H = \left\{0{\cdot}0144 + \frac{0{\cdot}01716}{\sqrt{V}}\right\}\frac{L}{D} \times \frac{V^2}{64{\cdot}4};$$

where H = head to overcome the friction, in English feet; L = length of pipe, in English feet; D = internal diameter of pipe, in English feet; and V = velocity of water, in English feet per second.

By James Thomson, A.M., C.E., and George Fuller, C.E., Belfast.

INTERNAL DIAMETER OF THE PIPES IN INCHES.

Velocity of Water, in Feet per Second.	Head to produce this Velocity, in Feet.	3		3½		4		4½		5		6		7		8		9	
		Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.
2·0	·062	·659	5·89	·565	8·02	·494	10·4	·439	13·2	·395	16·3	·329	23·5	·282	32·0	·247	41·9	·220	53·0
2·2	·075	·780	6·48	·669	8·82	·585	11·5	·520	14·6	·468	18·0	·390	25·9	·334	35·3	·293	46·1	·260	58·3
2·4	·090	·911	7·07	·781	9·62	·683	12·5	·607	15·9	·547	19·6	·456	28·2	·390	38·5	·342	50·2	·304	63·6
2·6	·105	1·05	7·65	·901	10·4	·788	13·6	·701	17·2	·631	21·3	·526	30·6	·450	41·7	·394	54·4	·350	68·9
2·8	·122	1·20	8·24	1·03	11·2	·900	14·6	·800	18·5	·720	22·9	·600	32·9	·514	44·9	·450	58·6	·400	74·2
3·0	·140	1·35	8·83	1·16	12·0	1·02	15·7	·905	19·8	·815	24·5	·679	35·3	·582	48·1	·509	62·8	·453	79·5
3·2	·160	1·52	9·42	1·31	12·8	1·14	16·7	1·02	21·2	·915	26·2	·763	37·7	·654	51·3	·572	67·0	·508	84·8
3·4	·180	1·70	10·0	1·46	13·6	1·27	17·8	1·13	22·5	1·02	27·8	·851	40·0	·729	54·5	·638	71·2	·567	90·1
3·6	·202	1·89	10·6	1·62	14·4	1·41	18·8	1·26	23·8	1·13	29·4	·943	42·4	·808	57·7	·707	75·4	·629	95·4
3·8	·225	2·08	11·2	1·78	15·2	1·56	19·9	1·39	25·2	1·25	31·0	1·04	44·7	·892	60·9	·780	79·6	·693	101
4·0	·250	2·28	11·8	1·96	16·0	1·71	20·9	1·52	26·5	1·37	32·7	1·14	47·1	·979	64·1	·856	83·7	·761	106
4·2	·275	2·49	12·3	2·14	16·8	1·87	22·0	1·66	27·8	1·50	34·3	1·25	49·5	1·07	67·3	·935	87·9	·832	111
4·4	·302	2·71	12·9	2·33	17·6	2·03	23·0	1·81	29·1	1·63	36·0	1·35	51·8	1·16	70·5	1·02	92·1	·905	116
4·6	·330	2·94	13·5	2·52	18·4	2·21	24·0	1·96	30·4	1·76	37·6	1·47	54·1	1·26	73·7	1·10	96·3	·981	122
4·8	·360	3·18	14·1	2·72	19·2	2·38	25·1	2·12	31·8	1·91	39·2	1·59	56·5	1·36	76·9	1·19	100	1·06	127
5·0	·390	3·43	14·7	2·94	20·0	2·57	26·2	2·28	33·1	2·05	40·9	1·71	58·9	1·47	80·2	1·28	105	1·14	132
5·2	·422	3·68	15·3	3·15	20·8	2·76	27·2	2·45	34·4	2·21	42·5	1·84	61·2	1·58	83·3	1·38	109	1·23	138
5·4	·455	3·94	15·9	3·38	21·6	2·96	28·2	2·63	35·8	2·37	44·2	1.97	63·6	1·69	86·6	1·48	113	1·31	143
5·6	·490	4·22	16·5	3·61	22·4	3·16	29·3	2·81	37·1	2·53	45·8	2·11	65·9	1·81	89·8	1·58	117	1·40	148
5·8	·525	4·50	17·1	3·85	23·2	3·37	30·3	3·00	38·4	2·70	47·4	2·25	68·3	1·93	93·0	1·68	121	1·50	154
6·0	·562	4·78	17·7	4·10	24·0	3·59	31·4	3·19	39·7	2·87	49·1	2·39	70·7	2·05	96·2	1·79	125	1·59	159
6·2	·600	5·08	18·2	4·36	24·8	3·81	32·4	3·39	41·0	3·05	50·7	2·54	73·0	2·18	99·4	1·90	130	1·69	164
6·4	·640	5·39	18·8	4·62	25·6	4·04	33·5	3·59	42·4	3·23	52·3	2·69	75·4	2·31	102	2·02	134	1·79	169
6·6	·680	5·70	19·4	4·89	26·4	4·28	34·5	3·80	43·7	3·42	54·0	2·85	77·7	2·44	106	2·14	138	1·90	175
6·8	·722	6·02	20·0	5·16	27·3	4·52	35·6	4·01	45·0	3·61	55·6	3·01	80·1	2.58	109	2·26	142	2·01	180
7·0	·765	6·35	20·6	5·45	28·0	4·77	36·6	4·24	46·4	3·81	57·2	3·18	82·4	2·72	112	2·38	146	2·12	185

Velocity of Water, in Feet per Second.	10		11		12		13		14		15		16		17	
	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.
2·0	·198	65·4	·180	79·2	·165	94·2	·152	110	·141	128	·132	147	·123	167	·116	189
2·2	·234	72·0	·213	87·1	·195	103	·180	121	·167	141	·156	162	·146	184	·138	208
2·4	·273	78·5	·248	95·0	·228	113	·210	133	·195	154	·182	176	·171	201	·161	227
2·6	·315	85·1	·287	103	·263	122	·242	144	·225	167	·210	191	·197	218	·185	246
2·8	·360	91·6	·327	111	·300	132	·277	156	·257	179	·240	206	·225	234	·212	265
3·0	·407	98·2	·370	119	·339	141	·313	166	·291	192	·271	221	·255	251	·240	284
3·2	·458	105	·416	127	·381	151	·352	177	·327	205	·305	235	·286	268	·269	302
3·4	·510	111	·464	134	·425	160	·393	188	·365	218	·340	250	·319	284	·300	321
3·6	·566	118	·514	142	·472	169	·435	199	·404	231	·377	265	·354	301	·333	340
3·8	·624	124	·567	150	·520	179	·480	210	·446	243	·416	280	·390	318	·367	359
4·0	·685	131	·623	158	·571	188	·527	221	·489	256	·457	294	·428	335	·403	378
4·2	·748	137	·680	166	·624	198	·576	232	·534	269	·499	309	·468	352	·440	397
4·4	·814	144	·740	174	·679	207	·626	243	·582	282	·543	324	·509	368	·479	416
4·6	·883	150	·803	182	·736	217	·679	254	·631	295	·589	339	·552	385	·519	435
4·8	·954	157	·867	190	·795	226	·734	265	·682	308	·636	353	·596	402	·561	454
5·0	1·03	163	·935	198	·857	235	·791	276	·734	321	·685	368	·642	419	·605	473
5·2	1·10	170	1·00	206	·920	245	·850	287	·789	333	·736	383	·690	435	·650	492
5·4	1·18	177	1·07	214	·986	254	·910	298	·845	346	·789	397	·740	452	·696	511
5·6	1·26	183	1·15	222	1·05	264	·973	309	·903	359	·843	412	·791	469	·744	529
5·8	1·35	190	1·22	229	1·12	273	1·04	321	·964	372	·899	427	·843	486	·793	548
6·0	1·43	196	1·30	237	1·19	283	1·10	332	1·02	385	·957	442	·897	502	·844	567
6·2	1·52	203	1·38	245	1·27	292	1·17	343	1·09	397	1·01	456	·953	519	·897	586
6·4	1·61	209	1·47	253	1·35	301	1·24	354	1·15	410	1·08	471	1·01	536	·951	605
6·6	1·71	216	1·55	261	1·42	311	1·31	365	1·22	423	1·14	486	1·07	553	1·01	624
6·8	1·81	222	1·64	269	1·50	320	1·39	376	1·29	436	1·20	500	1·13	569	1·06	643
7·0	1·90	229	1·73	277	1·59	330	1·46	387	1·36	449	1·27	515	1·19	586	1·12	662

Velocity of Water, in Feet per Second.	18		19		20		22		24		26		28		30	
	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.	Head to overcome Friction, in Feet.	Cubic Feet per Minute.
2·0	·110	212	·104	236	·099	262	·090	316	·082	377	·076	442	·070	513	·066	589
2·2	·130	233	·123	260	·117	288	·106	348	·097	414	·090	486	·083	564	·078	648
2·4	·152	254	·144	283	·137	314	·124	380	·114	452	·105	531	·097	616	·091	707
2·6	·175	275	·166	307	·158	340	·143	412	·131	490	·121	575	·112	667	·105	766
2·8	·200	297	·189	331	·180	366	·164	443	·150	528	·138	619	·128	718	·120	824
3·0	·226	318	·214	354	·204	393	·185	475	·170	565	·157	663	·145	770	·136	883
3·2	·254	339	·241	378	·229	419	·208	507	·191	603	·176	708	·163	821	·152	942
3·4	·283	360	·269	401	·255	445	·232	538	·213	641	·196	752	·182	872	·170	1,001
3·6	·314	382	·298	425	·283	471	·257	570	·236	678	·218	796	·202	923	·189	1,060
3·8	·347	403	·328	449	·312	497	·284	601	·260	716	·240	840	·223	974	·208	1,119
4·0	·380	424	·360	472	·342	523	·311	633	·285	754	·263	885	·244	1,026	·228	1,178
4·2	·416	454	·394	496	·374	550	·340	665	·312	791	·288	929	·267	1,077	·249	1,237
4·4	·452	466	·429	519	·407	576	·370	697	·339	829	·313	973	·290	1,129	·271	1,296
4·6	·490	488	·465	543	·441	602	·401	728	·368	867	·339	1,017	·315	1,180	·294	1,355
4·8	·530	509	·502	567	·477	628	·434	760	·397	905	·367	1,062	·341	1,231	·318	1,414
5·0	·571	530	·541	590	·514	654	·467	792	·428	942	·395	1,106	·367	1,283	·343	1,472
5·2	·614	551	·581	614	·552	680	·502	823	·460	980	·425	1,150	·394	1,334	·368	1,531
5·4	·657	572	·623	638	·592	707	·538	855	·493	1,018	·455	1,194	·423	1,385	·394	1,590
5·6	·703	594	·666	661	·632	733	·575	887	·527	1,055	·486	1,239	·452	1,437	·422	1,649
5·8	·749	615	·710	685	·674	759	·613	918	·562	1,093	·519	1,283	·482	1,488	·450	1,708
6·0	·798	636	·755	709	·718	785	·652	950	·598	1,131	·552	1,327	·513	1,539	·478	1,767
6·2	·847	657	·802	732	·762	811	·693	982	·635	1,168	·586	1,371	·544	1,590	·508	1,826
6·4	·898	678	·851	756	·808	838	·735	1,013	·673	1,206	·622	1,416	·577	1,642	·539	1,885
6·6	·950	700	·900	780	·855	864	·778	1,045	·713	1,244	·658	1,460	·611	1,693	·570	1,943
6·8	1·00	721	·951	803	·904	890	·821	1,077	·753	1,282	·695	1,504	·645	1,744	·602	2,003
7·0	1·06	742	1·00	827	·953	916	·867	1,109	·794	1,319	·733	1,548	·681	1,796	·635	2,061

[To face page 310.

and Mr. G. Fuller, calculated from the formula of Weisbach, to show, for pipes 100 feet in length, the relation between,—the velocity of the water, in feet per second,—the internal diameter of the pipe,—the head to overcome the friction, in feet,—and the number of cubic feet of water delivered per minute; so that when any two of the four quantities were given, the remaining two could be found. This Table he had found correct and practically useful. (*Vide* Table.)

Mr. Bidder, V.P., said, the great advantage of scientific education, was to impress upon the mind what had been taught, or done antecedently, and thus to avoid present error, and useless experiment; unfortunately, however, for the country generally, and unluckily for the success of the various sanitary measures that were, from time to time, proposed, Boards of Health and Commissions of Sewers had been, for the most part, composed of men who were entirely ignorant of even the elements of the science of Hydraulics, by which they professed to conduct great practical works; and hence, large sums of money had been squandered, in trial works, which, even if properly conducted, would only have verified the correctness of experiments, tried nearly a century since. But like all men, whether in politics, or science, who commenced with great professions of liberality, they became intolerant in Science, and tried as it were to warp Nature, and make new laws of gravitation, in order to make effects accord with their own preconceived theories; in consequence of which, they found themselves in the position of having circulated, at the public expense, a mass of supposed results, all equally fallacious and untrue.

With regard to the flow of water through different sizes and qualities of tubes, he believed, that error had arisen in the minds of those who designed and of those who conducted the experiments, simply from ignorance of what had been done, or misconception of the results. Now the explanation of the resistance of the interior periphery of pipes, of whatever material they were composed, was consistent with the results of actual experiment; and any further expenditure, on trials for demonstrating the supposed advantages of smoothness of internal surface, must be entire waste. It was well known, that the state of the external surface had much influence on the resistance of floating bodies, moving through water, and no doubt this fact had, from analogy, led to the delusion, that the smoothness, or otherwise, of the internal surface of pipes, would exercise an influence on the velocity of the flow of water. It being, then, an admitted fact, that, within certain limits, the surface was immaterial to the flow of water in pipes, it was desirable to seek a satisfactory explanation. Probably it might arise, from the circumstance of there being a comparatively fixed film of water, held by the force of cohesion, against the internal surface, through and between which, the main body of water moved; whereas with a vessel, or other floating body, the surface passed continually into a fresh mass of fluid, bringing the asperities of the moving surface successively into contact with new particles, and thus materially influencing the resistance.

There must always be some abrasion, or friction in the pipes, and he confessed, there were circumstances connected with the flow of water through pipes, which he did not yet understand. Mr. Hawksley had partially explained the separation and cohesion of particles; but there were still difficulties unexplained.

A case analogous to the question, was the increase of resistance to a boat in a canal, as compared with the same boat at sea, or in a deep river. A boat, that, in the Thames, would go seven miles an hour, would not travel, with the same power, more than four miles an hour, in a canal, whose sectional area was not more than four times that of the boat. That indicated a resistance of 4, or 5 to 1, and it was only after consideration, that the partial explanation occurred to him. When a boat acquired a velocity of three miles an hour, the water would rush into the wake to fill up the vacuity, with an impetus equal to one mile per hour, therefore, that speed would be imparted to the entire mass of water, immediately surrounding the boat; and, practically, the boat would have a resistance due to a speed of four miles per hour, the whole resistance of which must be added to giving motion to the entire body of water in the canal. A canal boat was sharp at the bow and tolerably well shaped, and therefore, not a bad object to try experiments with; and he apprehended that, practically, it would be found necessary to give to the water displaced by the boat, a much greater velocity than to the boat itself; and that the resistance was two, or three times in a canal, what it would be in open water.

With regard to the Public Boards, he could not help expressing his astonishment, at the manner in which their proceedings were generally conducted, and his deep concern at the difficulties, which were thrown in the way of the rising generation of Engineers, by the governing classes of the country. For many years past, the 'functionary weed' had flourished to a most mischievous extent; and Engineers found themselves interfered with, restricted, and controlled by officials, who received their appointment, not on account of superior merit, or scientific acquirements, but from the undue pressure of personal, or political influence. There was, first, the Board of Trade with its Railway Inspectors, who, with gentlemanly feelings and an innate perception, that the duties demanded of them were useless and invidious, felt themselves placed in a false position.

Then there were the Harbour Commissioners of the Admiralty, of whose decisions, there was generally entertained an opinion, anything but flattering. In a case which came under his own observation, it had been reported, that there was not any necessity for undertaking the proposed work: nevertheless, the project, on reference to head-quarters, was strenuously supported by men of influence, who succeeded in obtaining an order for its execution.

Lastly, there was the Board of Health, which, he would advisedly say, was the worst of all: it had expended large sums in valueless experiments, on a question already thoroughly understood by scientific men, and by publishing the incorrect results at the public expense, had created general confusion throughout the country.

The facts which had been stated, with respect to the experiments of the 'Trial Works Committee,' of the former Commissioners of Sewers, and the corrupt use that had been made even of those worthless experiments, was a striking instance of the bad effects produced on private enterprise, and on the development of sound practice, by the rapid growth of functioneering influence. During a long period of peace, the cumbrous machinery of Government Departments had been presumed to have been rendered perfect, and was assumed to be so, whilst no demand was made on their active energies, or so long as no exigencies arose; but the late melancholy and disastrous events in the East, had shown their utter inability to fulfil their functions, under any unexpected pressure, or to conduct any practical measure in a business-like manner.

Over the doors of the Government Offices, might truthfully be written, "He that entereth here, leaveth outside all mental activity, all generous impulse." It could not be denied, that the great public works of the country, had met with much obstruction, from each and all of the bodies to which he had alluded, and that many great enterprises of public utility, had been either kept in abeyance, or entirely defeated, by sinister influence, over which the country had no control. He hoped, that the remarks he had thought it his duty to make, would incite every one to assist in abating so enormous an evil, and in clearing away the difficulties, which now beset the path of those who laboured in the arduous field of scientific progress:—a result, not less important to the country at large, than to the rising generation of Civil Engineers.

Mr. Rawlinson deprecated the attacks which had been so markedly directed against the General Board of Health. He had held the office of Engineering Inspector under that body, and, so far as he was personally concerned, he had the satisfaction of feeling, that he had ever striven conscientiously to perform his duty. For the experiments in question, he was not responsible;

but he complained, that the question had been discussed upon the unfair assumption, that those who carried them on, were interested in arriving at erroneous results. Glass pipes were, of course, only used for experiments: but, with reference to the use of glazed pipes for the passage of semi-solid substances, it must be remembered, that other Engineers, besides those appointed by that Board, had extensively employed them for sewers; and there were instances of small sewers of two thousand years old. He must, therefore, protest against so sweeping a denunciation of the acts of the General Board of Health.

Mr. Bidder, V.P., said, in explanation, that the remarks he had made, were not intended to be personal, but were directed against a system, proved to be pernicious, and from which the most serious results must be anticipated. He must add, in reference to what had just been stated, that the Engineers who had used pot-pipes for main sewers, had only done so, under the compulsory pressure of the Board of Health, as by no other means, could they have procured permission to execute the drainage works, which they had undertaken.

Mr. J. Scott Russell had made many practical experiments upon the resistance of fluids, and had published the results for the public benefit: he, therefore, could not understand, why the results of experiments, prosecuted at an expense of some £7,000 of the public money, and carried out by a gentleman, said to have been nominated by no less an authority than Professor Airy, should not have been made public in the blue-books; more especially, as it was generally understood, that the Board of Health had not hesitated to pay a considerable sum, for some Tables of Observations of a similar nature, made by a private individual, and to publish and circulate them extensively.

Mr. Rawlinson repeated, that he had, personally, nothing whatever to do with the experiments. He knew nothing further concerning them, than had already been mentioned in the discussion. The General Board of Health had, certainly, purchased Mr. Roe's Tables, and had at once published them; but he must maintain, that this was not done for the purpose of misleading, but with the view, however erroneous that judgment might be considered, by the Institution, of throwing all the light they could, on the question at issue.

Mr. Hawksley believed, he could explain the misapprehension which appeared to exist. In 1848, upon the representation of parties connected with sanitary inquiries, the various existing Commissions of Sewers were abolished by the Lord Chancellor, who appointed one single Commissioner to control the whole of the Metropolis. In the same year, there was established a General Board of Health, and the most active member of that body was, with some of his friends, placed upon the Commission of Sewers. The expressed opinion of that gentleman had been, up to that time, that the London sewers were too small: but he then arrived at the diametrically opposite conclusion,—that they were positively too large. He also imagined, that larger quantities of water, than were consistent with the laws of gravitation, would pass through pipes, if made of a particular material. It was at this period, that the 'Trial Works Committee' was formed, for the purpose of instituting a large series of (so-called) experiments, in order, ostensibly, to ascertain whether the previous data, relied on by Engineers, were not altogether erroneous. Documentary evidence still remained to show, that the experiments were undertaken, without the necessary knowledge of the science of Hydraulics, or of the mode of experimenting, or of correctly recording the results, the prime qualification for experimenters, being the knowledge 'how to observe and what to observe.' The natural consequence was, the collection of a number of documents of the most heterogeneous character; many of them being endorsed by the examining Commissioners,—"anomalous,"—"incredible,"—"require to be retried," &c., &c. Application was, however, made to Professor Airy, to appoint some competent calculator, to reduce the results into a tabular form, and to draw up a Report. The gentleman accomplished his ungrateful task, and was very ill paid for the performance; but strange to say, no trace of either the tabulated results, or the Report, existed amidst the mass of documents which the Commission, on leaving office, transferred to their successors, who, he was bound to admit, had most carefully arranged them for reference. The papers were printed by order of the new Board, but were then found so unworthy of credit, and so useless for scientific purposes, that the Commissioners declined to sign and issue such a document, and it was finally suppressed; but not until a few copies had reached private hands, one of which was in his own possession. (Mr. Hawksley then quoted from the book, some of the experiments, which he denounced as unscientific, impossible, worthless, and absurd.) Here it might have been presumed, that the matter would have rested; but the originator of the experiments, by means of his other official position at the Board of Health, which he continued to retain, after the Sewers' Commission, of which he was a member, had been superseded, again revived his theory, as a Government correction of mischievous engineering errors. Evidence was obtained, with the object of showing, that all the formulæ in use among professional men were fallacious; a blue-book, intended to demonstrate this, was published by the General Board of Health, and officially presented to Parliament,—eleven thousand copies were circulated at the public expense, and practical Engineers were met at every turn by this fallacious document. The Engineer must necessarily be wrong; for he was diametrically at issue with a book, published under the authority of Government. The profession, as a body, had a right to remonstrate energetically, against such unjust and unconstitutional interference with skilled enterprise, and the public had still greater cause to complain of the disregard for their interests, and the wasteful expenditure incurred, in the execution of works, based on false and unscientific principles, which, within a short period, it would be requisite to reconstruct.

Mr. Simpson,—President,—said, he had himself undertaken a series of experiments on the flow of water through pipes, and had arranged the results in the following Table:—

	Diameter of Pipe.	Length of Pipe.	Head of Water.	Actual Discharge.	Calculated Discharge.	Per centage of Difference above and below Actual Discharge.
	Inches.	Feet.	Feet.	Cubic feet per minute.	Cubic feet per minute.	
Main from Brixton to Streatham.	12	5,200	38	205	200	− 2½
	12	5,200	40	205	205	..
	12	5,200	16¾	137	133	− 3
	12	5,200	19	137	141	+ 3
	12	5,200	4	68½	65	− 3½
Main from Belvedere Road to Brixton.	19	22,440	41	323	325	+ 0½
	19	22,440	43½	331	338	+ 2
	19	22,440	34	298	304	+ 2
	19	22,440	27½	267	272	+ 1⅞
	19	22,440	24	243	247	+ 1⅝
Main at Liverpool.	12	8,140	27	124½	134	+ 7½
	12	8,140	24¾	119	127	+ 6¾
	12	8,140	18	104	110	+ 5¾
	12	8,140	12	89	89	..
	12	8,140	5½	67	61	− 9
Main at Carlisle.	12	6,600	34½	168	167	− 0½
	12	6,600	46	189	195	+ 3
Main from Ditton to Brixton.	30	54,120	25	521	510	− 2

The theorem by which the Table was calculated, was,—

$V = 50 \sqrt{\frac{d\,h}{l + 50\,d}}$, where V = velocity in feet per *second*,

or making Q = cubic feet of water per *minute*,

$$Q = 2{,}356 \sqrt{\frac{d^5 h}{l + 50\,d}}.$$

This formula, known as Eytelwein's, was the same as that used by Mr. Leslie in his Paper.

The results at which Mr. Simpson had arrived, would be found to verify the formulæ, and to corroborate the experiments of Mr. Leslie, which were extremely valuable, from having been made on so large a scale, and extending over so considerable a period of time.

There could not be the least doubt, that the experiments, made by order of the Commission of Sewers, evinced gross ignorance; since, for example, they did not show the inclination of the surface of the water, flowing through the pipes. The different circumstances of bends, &c., must also be taken into account; and whenever apparently anomalous results were obtained, the greatest care should be exercised, in tracing out the cause from which they proceeded. The notion of the smooth surfaces of glazed pipes, accelerating the flow of water, was an utter fallacy.

The observations, made by old and scientific experimenters, might safely be relied on, and calculations, based upon their formulæ, would give the most satisfactory results.

February 13, 1855,

and

February 20, 1855.

JAMES SIMPSON, President,

in the Chair.

The discussion upon the Paper No. 924—"On the Flow of Water through Pipes and Orifices," by Mr. Leslie, having been renewed, was extended to such a length, as to preclude the reading of any other communication.

[February 27,