(M)

MR. WALSH ON THE MEASUREMENT OF EXCHANGE-VALUE[1]

[THE ground on which I ventured to criticise Mr. Pierson's attack on index-numbers, namely, the not to be ignored connection of the subject with Probabilities, is also the main ground of my differences with Mr. Correa Walsh. They are expressed in the following paper, which appeared in the ECONOMIC JOURNAL, 1901, under the title, " Mr. Walsh on the Measurement of General Exchange-Value." Mr. Walsh does not accept my view, and has replied with vigour in a brochure entitled "The Problem of Estimation," of which an account is given in the *Journal of the Statistical Society* for 1921, in a review bearing the well-known initials G. U. Y.

A rejoinder to Mr. Walsh's replies is published in two parts, one in the ECONOMIC JOURNAL, September 1923, the other in the *Journal of the Royal Statistical Society,* July 1923. *Cp.* above, p. 198.]

The capacity of taking boundless trouble, which is a characteristic of solid talent, distinguishes the work of Mr. Walsh. Whether he searches the writings of others or elaborates his original ideas, the thorough student and close thinker is manifest on every page.

The literature of the subject has never been examined so fully. Every devious path in the field where index-numbers flourish has been traversed in order to form an unrivalled collection of methods for measuring changes in the value of money. Many of the specimens here exhibited are probably new even to specialists. Or if the form was known, its origin and evolution were unknown. Who ever heard, for instance, of Carli and of Dutot as authorities on the subject? The bibliography would alone be sufficient to impart a lasting value to this work.

But Mr. Walsh is much more than a collector of specimens,

[1] *The Measurement of General Exchange-Value,* by Correa Moylan Walsh. New York: Macmillan & Co., 1901.

The powers of a systematic botanist are also his. He classifies the material which he has collected. For example, it is doubtless a great improvement in logical arrangement to distinguish index-numbers in which, as usual, a single system of weights is used for the relative prices, from those typified by Lehr's and Drobisch's methods in which "double weighting" is practised. Again, among methods of weighting each article according to the expenditure thereon, there is a distinction between those which in effect compare the money value of the same set of articles at different times and those typified by Mr. Palgrave's method. I give the essence, as I conceive it, rather than the wording of some passages in the author's learned and logical Appendix C.

Mr. Walsh has not contented himself with classifying the specimens which he has collected. He has also attempted to penetrate to the structure and function of an index-number by a new microscopical analysis. Having observed the properties of the different kinds, by skilfully crossing the "arithmetic" with the "geometric" type he has produced a new variety which may claim to excel in certain respects the existing species.

Limits of space prevent me from tracing these general characteristics through the contents of Mr. Walsh's volume. In truth, it might be feared that my reader's patience would give out if I attempted to reproduce in anything like their original, almost Kantian, elaborateness discussions to which the term "exhaustive," with all its suggestions, is particularly applicable. I will, therefore, select a few points which seem to be of special and permanent interest. Some solid and salient stepping stones may thus be afforded for traversing the flood of dialectic.

Mr. Walsh begins by defining different senses of value. He is specially happy in distinguishing cost value from other species. He complains not without justice, although great names fall under his condemnation, of those who have confounded the different *quæsita*. He well remarks that, if a measure pertaining to cost value is to be constructed, we should not confine our calculations to the consideration of wages, but include profits.[1] His own investigation is confined to "general exchange value," which seems to have a certain parallelism with "final utility," as appears from its relation to Lehr's method :—

"In this method [Lehr's] its author has made an effort to do what appears to be accomplished in the method here presented. He has tried to measure the variation in the average price of

[1] *Cp.* Section on the "Labour Standard" in the Memorandum attached to the third Report of the British Association Committee (above, p. 203).

mass-units, in all the classes, that have the same exchange-value over both the periods together—to which equivalent mass-units he has given the not inappropriate name of pleasure-units" (p. 386).

But Mr. Walsh's exchange-value is more objective (9). The properties of general exchange-value are set forth in a series of propositions, which may deserve the epithet "expletive," in so far as they are mostly self-evident yet render our instructive knowledge fuller and clearer. Among original points may be noticed the distinction between the exchange-value of a thing (*e.g.*, money) in relation to all *other* things, and in relation to all things *including itself* (13). When first the reader learns that exchange-value is considered as objective, he may be disposed to expect that it is an affair only of ratios abstracted from the quantities produced and consumed. Insensibly, however, as we ascend the gentle steps formed by the series of more or less "expletive" propositions, there is borne in on us the need of weighting. We dimly descry a unit, sometimes called an "economic individual" (102, 301), an "exchange-value quantum" (302); we are directed to contemplate "mass-units ideally constructed" (285), "considered as equal, not as weights or capacities, but as exchange-values" (284), in relation to which it is sought to determine the value of money at different times (and places). The data for this determination are prices and quantities of commodity; the problem is properly to combine these data. Two main questions arise:—What importance or "weight" is to be assigned to each of the given prices which enters into the combination? and What should be the method of combination? These questions are first considered separately as far as possible, and then in their necessary connection. I will not follow the preliminary separate inquiries through the windings of Mr. Walsh's exhaustive discussion. Suffice it to notice that materials are not to be included in our index-number along with finished goods (78, 96), apparently for a reason usually given, that the factors of production are counted in the products. Nor is it the quantity of each exchangeable thing that is actually exchanged for money (85), but rather, as I understand, the quantity that is used, which concerns us. As to the method of combining the data we are practically restricted to the three classic Means, the Arithmetic, Harmonic, and Geometric. The author compares the properties of these means, showing certain grounds for the preference of the Geometric:—

"If the exchange-value of money in [B] rises by more than 100 per cent. the compensatory fall of the exchange-value of money in [A] should be to below zero according to the arithmetic method of averaging, which therefore is inapplicable in this case [where [A] and [B] are two equally important classes of things.] And if the exchange-value of money in [A] falls to less than half, the exchange-value of money in [B] should rise from below zero, according to the harmonic method of averaging, which therefore is inapplicable here. But in the use of the geometric compensation there are no such impossible cases " (249).

This passage illustrates certain properties of the compared means, to which the author attaches importance. In the simple case of two extremes, between which a Mean is taken, the distance of the Arithmetic Mean from one extreme, per cent. of the Arithmetic Mean, is equal to the distance of the Arithmetic Mean from the other extreme, per cent. of the Arithmetic Mean. The distance of one extreme from the Harmonic Mean, per cent. of that one extreme, is equal to the distance of the other extreme from the Harmonic Mean, per cent. of that extreme. The distance of one extreme from the Geometric Mean, per cent. of that extreme, is equal to the distance of the Geometric Mean from the other extreme, per cent. of the Geometric Mean. This last proposition cannot be extended from the case of *two* to that of many variables, from the geometric *mean*, in Mr. Walsh's very peculiar phraseology, to the geometric *average*. To the same class of properties, true of the "mean," but not the average, belongs the following, which Mr. Walsh considers important :—

$$\text{If } a_1 a_2 = b_1 b_2, \text{ then } \sqrt{\frac{a_2}{a_1} \times \frac{b_2}{b_1}} = \frac{a_2 + b_2}{a_1 + b_1}.$$

Confining myself to the general and concrete case of plural data, I hasten on to the latter stages in which the question of weights and means, at first separated, are considered in their real connection. We have now to consider penultimately the two simplified cases in which either (1) the sums of money expended on each commodity remain constant at the two periods (or places) compared, or (2) the quantities of each commodity are thus constant; and finally (3) the general concrete case in which both expenditure and quantities vary. In the first case I think most people would be disposed to answer off-hand that the sums supposed constant form the proper weights for an arithmetic combination. The author, however, seems to rightly judge that the ideal of comparing the money-values of the same number of exchange units or "economic individuals" would not be realised by this procedure; for a reason which he thus assigns with respect to the proposal of taking the arithmetic mean of the sums when supposed different :—

"If it happens that the exchange-value of money has fallen or prices in general have risen, greater influence upon the result would be given to the weighting of the second period. . . . Or in a comparison between two countries greater influence would be given to the weighting of the country with the higher level of prices. But it is plain that *the one period or the one country is as important in our comparison between them as the other, and the weighting in the averaging of their weights should really be even*" (105).

To avoid the difficulty thus indicated, the following formula is proposed in the case of constant sums being expended on each commodity. Let a_1, a_2; β_1, β_2; . . ., be the prices at the first and second epoch respectively, and x_1, x_2; y_1, y_2; . . . the corresponding quantities of commodity; the required index-number is $\frac{x_1\sqrt{a_1a_2} + y_1\sqrt{\beta_1\beta_2} + \cdots}{x_2\sqrt{a_1a_2} + y_2\sqrt{\beta_1\beta_2} + \cdots}$; or, as by hypothesis $x_1a_1 = x_2a_2$, this may be written, $\frac{a_2\sqrt{x_1x_2} + \beta_2\sqrt{y_1y_2} + \cdots}{a_1\sqrt{x_1x_2} + \beta_1\sqrt{y_1y_2} + \cdots}$ (310). The transition is easy from this formula, "Scrope's emended method," as Mr. Walsh calls it, to Scrope's method pure and simple, which is proper to the second abstract case, in which the *quantities* of each commodity are constant, say $x, y \cdots$ We have only to substitute in the last written formula, x for $\sqrt{x_1x_2}$, and so on (360). These prolusions lead up to the general concrete case in which neither the sums nor the quantities remain constant. Guarding against the difficulties encountered in the simpler cases, the author proposes this "universal formula" :—

$$\frac{x_2a_2 + y_2\beta_2 + \cdots}{x_1a_1 + y_1\beta_1 + \cdots} \times \frac{x_1\sqrt{a_1a_2} + y_1\sqrt{\beta_1\beta_2} + \cdots}{x_2\sqrt{a_1a_2} + y_2\sqrt{\beta_1\beta_2} + \cdots}.$$

This form is shown to have a certain theoretical advantage over other species of index-number, in particular those which, as affected with "double weighting," most challenge comparison with it, namely Drobisch's and Lehr's methods. The universal formula satisfies *some* of the criteria which Mr. Walsh has laid down. It does not, however, in general, satisfy what he has called Professor Westergaard's test that (*e.g.*) "prices measured from 1860 to 1870 and from 1870 to 1880 ought to show the same variation from 1860 to 1880 as would be shown by comparing the prices of 1880 directly with those of 1860" (205). One may imagine a world in which the universal formula, and even "Scrope's emended method," would completely satisfy Professor Westergaard's test and all other tests. "But in the world as it is, we have not reached the absolutely true method" (402).

What now is the worth of this result and of the investigations which lead up to it? The answer to this question will vary with the critic's preconceived opinion on some very debatable first principles. I, for one, find myself at variance with Mr. Walsh on certain fundamental issues, for the discussion of which I have thought an independent article more appropriate than a review.

I cannot accept a view of the subject according to which it is significant to seek an exact measure of the change in the value of money in the case where only *two* relative prices are given. This paucity of data would indeed be innocuous if we had as clear and objective a perception of the units of exchange-value as of the units of mass and motion, or the degrees of the thermometer. On that supposition we might even speak with Mr. Walsh of obtaining an expression for the "general exchange-value" of money, or any one thing, "at each period separately" (76, *cp.* Appendix A). A series of such expressions for successive years would no doubt satisfy Professor Westergaard's criterion above mentioned and all other tests. But I can form no idea of such a general exchange-value, except the somewhat indefinite notion of the relation between an amount of money and the quantity of utility which it will procure. I have not the courage to speak with Professor Irving Fisher of a *util* as an hedonic unit, I do not insist on the term utility, but only on the fact that our perceptions of the value of money in relation to such a unit as is desiderated are vague and indefinite. Suppose that one large class of commodities, say those following the law of decreasing returns, were to rise in price each by the same or nearly the same percentage, while all other articles in use, also forming a large class, were to fall together; that in such a case the exchange-value of money has varied by so much would appear to me a somewhat indefinite proposition—its subject deficient in logical clearness, and its predicate in numerical precision. On such a supposition the objections which have been urged by a distinguished economist against index-numbers,[1] that the results are widely different according as different species of averages are employed, would seem to me a fatal objection. The wide differences which may exist in such a case between different means are indeed of a piece with the enormous discrepancies which might be expected between the estimates

[1] Economic Journal, Vol. VI. p. 130.

of equally competent judges as to the change in the value of money in respect to some such unit as it postulated. For example, if the drop in one large class, including necessaries, was great, while the rise in the remaining class was small, it would probably seem to all that money had fallen in value; it might seem to only a few that it had fallen to half its original value; but between these limits there might be no unanimity. With all his logical precision, Mr. Walsh does not seem to have removed what Mill calls "the necessary indefiniteness of the idea of general exchange-value." Mr. Walsh admits that "we have not yet reached the absolutely true method." I am disposed to think that we never will reach an exactly true method on his lines, until we are able to handle and weigh final utility, or what he calls "esteem-value," as we do material commodities.

What should we think of a book which purported to instruct the Civil Service Commissioners who superintend our public examinations as to the principles by which their judgment should be decided in cases where there might be only two marks for each candidate, say one in literature and one in science? Should we expect that any skilful blend of arithmetic and geometric mean would bring out a true figure, representing the real relation between the merits of the candidates? That large part of Mr. Walsh's analysis which is devoted to the case of *two* data appears to me to be equally foredoomed to failure. I should not expect much useful suggestion from any formula which holds good only for the artificially simplified case of dual data, and not for the concrete reality of plural data.

Doubtless a certain interest is excited by this attempt to feel after a conception of general exchange-value. Perhaps posterity will regard these tentatives as we regard the exercise of thought by which appropriate conceptions in mathematical physics have been won. Or, to compare small things with great, the better parallel might be found in the disquisitions by which the ancient philosophers made familiar, if they did not make quite definite, many abstract terms which are still in use. Meanwhile our author has a less pleasant feature of resemblance to the Greek sages, namely a proud confidence in dialectic, to the neglect of more positive science. I refer to his treatment of the Calculus of Probabilities. He regards it as irrelevant (38,) and takes Cournot to task for applying it to the problem in hand (38, 66, 69). This omission of Probabilities appears to me serious. Even granting [1] that the *primary* problem is to measure the value of money in some

[1] Without prejudice to the claims of the "Labour" or "Real Cost" standard; which we may agree to postpone as not ripe for discussion.

such unit as Mr. Walsh desiderates, still by rejecting the Calculus of Probabilities he has not only thrown away an instrument necessary for the performance of that measurement, but also has lost sight of an important *secondary* aspect of the problem.

First, according to the view here submitted, the estimate of the relation between money and the unknown unit based upon one or two price variations is very vague—the discrepancy between equally authoritative estimates might perhaps be as likely as not to amount to twenty-five per cent. in accordance with the suppositions made just now. But by the Theory of Probabilities, as observations are multiplied, the enormous "probable error" incident to the individual observations becomes diminished in the average. The rope is much stronger than its component strands. I would not deny that there is some philosophical difficulty in thus obtaining a definite measurement of a quantity, the degrees of which are not capable of being perceived distinctly. Rather, I would say with Professor Marshall,[1] that an absolutely perfect standard is "unthinkable." But here, as in wider spheres of conduct, although speculative difficulties cannot be perfectly resolved, we may obtain sufficient guidance for action. One useful direction is that "weighting" is of less importance than at first sight appears. Even with reference to what I am willing to regard as the primary *quæsitum*, it is safe to say with Mr. Bowley that "no great importance need be attached to the special choice of weight."[2] It is well to imitate the judicious compromise and happy ambiguity of Sir Robert Giffen in the second Report of the British Association Committee (1898):—"Practically, the Committee would recommend the use of a weighted index-number of some kind, as, on the whole, commanding more confidence. But they feel bound to point out that the scientific evidence is in favour of the kind of index-number used by Professor Jevons—provided there is a large number of articles—as not insufficient for the purpose in hand. . . . A *weighted* index-number, in one aspect, is almost an unnecessary precaution to secure accuracy, though, on the whole, the Committee recommend it."

I do not retract the opinion which has been expressed above that the index-number elaborated by Mr. Walsh[3]—the one applicable to the general case of varying quantities and prices—has a certain theoretical advantage over its predecessors. But I doubt whether the advantage of this method over the simpler method sanctioned by the Committee of the British Association

[1] *Contemporary Review*, 1887.

[2] *Elements of Statistics*, p. 113; *cp.* ch. ix. 1.

[3] Above, p. 373.

is so great as to compensate the trouble of applying the more complicated method. This doubt is confirmed by the following consideration. It seems to be admitted by high authorities—and Mr. Walsh would apparently agree [1]—that the most exact solution of the concrete problem is obtained by a series of index-numbers taken at short intervals of time. Now the interval of time between any two adjacent index-numbers being small, we are entitled to assume that the change in price and also in quantity during any such interval is small. Accordingly let us substitute in Mr. Walsh's above-written formula for $a_2, \beta_2, a_1 + \Delta a_1, \beta_1 + \Delta\beta_1$, and similarly for $x_2, y_2, x_1 + \Delta x_1, y_1 + \Delta y_1$, where $\Delta a, \Delta\beta, \Delta x_1, \Delta y_1$ are small (relative to a_1, β_1, x_1, y_1 respectively), in such wise that the second and higher powers of the quantities $\frac{\Delta a_1}{a_1}, \frac{\Delta x_1}{x_1}$, etc., are small fractions. Then, expanding in powers of Δa_1, etc., Δx_1, etc., we find that Mr. Walsh's "universal" formula differs from the index-number recommended by the British Association Committee only by quantities of the *second* order. It may be added that the elegant formula which, as above mentioned, Mr. Walsh introduces as "Scrope's emended method" differs from the index-number of the British Association Committee only by quantities of the *third* order.

Mr. Walsh seems to have exaggerated the need of weighting. He gives the *Economist's* index-number as an example of the discrepancy resulting from different weights (83).

"In the comparison given by Mr. Palgrave of the Economic series of 'unweighted' index-numbers and the index-numbers calculated upon the same prices, we find the following contrasts :—

1880	87	89
1881	81	93
1882	83	87
1884	79	88

Here the calculated movements of general prices go in exactly opposite directions in every sequence of years. Between the first and the second years, for instance, the *Economist* figure falls 7 per cent., and the 'corrected' figure rises 4½ per cent.—a difference of 12 per cent. Divergences of this sort are to be seen in every case where in a series of periods the same price has been treated in both ways for comparison."

But in a matter of this sort we should look to the average

[1] P. 113, referring to the Report of the British Association for 1887, our first Memorandum, above, H.

character of experience rather than at exceptional instances. The rudimentary index-number of the *Economist* appears less typical than Mr. Sauerbeck's index-number or that compiled by the Aldrich Report,[1] each of which gives almost identically the same result whether unweighted or weighted. We should contemplate in the statistics compiled by the Bureau of Economic Research,[2] the curves which represent the weighted or unweighted index-numbers hugging each other closely through the long course of years. We should take into account too the *a priori* reasons for expecting this sort of correspondence, reasons which derive some confirmation from their verification in the like matter of wage statistics. See the "example of the smallness of the change introduced by difference in systems of weighting" in Mr. Bowley's *Elements of Statistics* (p. 114 *et sqq.*, cp. *ibid.*, p. 219, "On the unimportance of weights," *et sqq.*).

Doubtless divergences of the sort, to which our author points triumphantly, "are to be seen in every case" if you look out for them; just as extraordinary sequences are to be seen in games of chance if you look out for them long enough. Mr. Walsh, indeed, has not been very happy in his selection of a specious exception. By a pardonable oversight it has escaped his attention that the index-numbers which he contrasts are not as he supposes "calculated upon the same prices." The unweighted index-number is taken from Mr. Palgrave's Table 26,[3] in which the prices of *cotton-wool, cotton-yarn, cotton-cloth*, play a part. The weighted index-number is taken from Mr. Palgrave's Table 27, from which these three prices are excluded. For the purpose in hand it would have been proper to exclude those three cotton prices, as is done in the Memorandum attached to the Second Report of the British Association Committee. I reproduce the result so far as relevant here.

	1880.	1881.	1882.	1883.
Mr. Palgrave's Weighted Mean for 19 articles	89	93	87	88
The simple Arithmetic Mean for the same articles	93·5	86	89	85·5
Excess of Arithmetic over Weighted Mean	4·5	− 7	+ 2	− 2·5

[1] See ECONOMIC JOURNAL, Vol. VI. p. 136. [2] *Ibid.*, Vol. X. p. 600.
[3] Third Report of the Royal Commission on Depression of Trade and Industry. [*C.*—4797], 1886; pp. 343—353 (cp. *Brit. Ass.*, 1888, p. 203).

It is still true that "the calculated movements of general prices go in exactly opposite directions in every sequence of years," that is three times.[1] But as the distance to which they go is inconsiderable in comparison with the "probable error" to be expected, it would be requiring too much that they should always go in the same direction. The figures in the table from which an extract is given had been noticed in the Memorandum referred to as exceptional, not on account of their divergence but on account of their agreement. "The annexed comparison," it was there remarked, "does not present the appearance of pure chance. The discrepancies are rather *less* in magnitude than the theory regards." This "faultily faultless" character of the index-number is *pro tanto* corrected by Mr. Walsh when he points out some little discrepancies in the matter of the sequences.

Had he bestowed more attention on the theory of averages, our author would have asserted with less confidence that "in no other case [except the case in which all prices vary alike] do we want to seek any determination 'irrespective of the quantities of commodities.'"[2] There is a *secondary* form of the problem with respect to which weighting has even less importance than under the first aspect. I may introduce this variety by a problem which has been likened to the problem now before us, the determination of the sun's motion relatively to the sidereal system. Referring to this sort of problem Mr. Walsh has some just remarks on the relative motion of the single body and the system (68, *cp.* 38). He may be right in suggesting that the use of Probabilities in the analogous monetary problem has sometimes been connected with a confusion between cost-value and the kind of value which he has set himself to measure. Yet I do not feel sure that the function of the Calculus is adequately recognised in the following passage :—

"When we have chosen which method we shall adopt, and what shall be our standard [whether we shall consider motion of a body relatively to *all other* things, or to *all* things including itself],

[1] Out of fifteen sequences or changes from year to year shown by the complete table *eleven* are in the same direction for both weighted and unweighted index-numbers; *four* are in opposite directions, viz. 1873–1874 and the three sequences selected by Mr. Walsh, 1880–1881, 1881–1882, 1882–1883.

[2] Page 222, note. Referring to the present writer's Memorandum attached to the Report of the British Association Committee, 1887, p. 280; where the commentator strangely supposes that the case contemplated is that "in which all prices vary alike." The context of the section referred to and the parallel section in the third Memorandum (Report of the British Association, 1889, p. 156) make it clear that the sought common effect of changes in the supply of money is not supposed to be given free from disturbances special to particular commodities (*cp.* below, p. 380 *et seq.*).

there is of course no occasion for employing in our measurements the law of probabilities—as was asserted also in this connection by Cournot. We do not say it is more probable that all the other things have remained stationary than that this one has stood still and they moved; or it is more probable that all things have together remained stationary, wherefore both this and the others have moved relatively to the whole. But having adopted our point of view we simply measure as best we can what we see happening before us. And our point of view itself in these matters we adopt not by any use of the law of probabilities, but because the myriad inter-relations which do not change, or which do not change on the average, make more impression on us than the particular ones which do change" (69, 70).

However this may be, it does not invalidate the proposition which I am concerned to maintain: that without knowing the centre of gravity, or "weighted mean" of a system of bodies, we may know by the theory of averages that one single body is advancing through the cluster. Leaving the problem of the stars, which involves some technicalities, let me take a humbler terrestrial illustration. The annexed pairs of figures were thus obtained: As I walked along Piccadilly one day I noted the number of omnibuses* which met me (viz. 7) and the number which passed me (viz. 3) out of the first *ten* which came up to me, whether they were moving in the one direction or the other; and so on for successive decades (the observations not being all made on the same day, nor at the same hour). Here are some of the observations:—

7, 3; 8, 2; 8, 2; 5, 5; 7, 3; 8, 2; 7, 3; 6, 4;
7, 3; 6, 4; 7, 3; 7, 3; 6, 4; 8, 2; 8, 2; 7, 3;
8, 2; 4, 6; 7, 3; 7, 3; 8, 2; 6, 4; 9, 1; 8, 2.

From these and other observations in *pari materia*, I find that on an average of the omnibuses observed, about 70 per cent. met and 30 per cent. passed the observer. If, as there is reason to suppose [1] (at the hours when the observations were made), the

* The vehicles were drawn by horses in those days. The experiment recently repeated with respect to motor-buses gave a different result.

[1] This presumption is confirmed by the following statistics in which the first member of each pair (*e.g.*, 6 in the first pair) denotes the number of omnibuses moving eastward, and the second number (*e.g.*, 4 in the second pair) denotes the number moving westward, out of every ten omnibuses, which, sitting at the window of a club in Piccadilly, I observed passing in either direction:—

6, 4; 5, 5; 4, 6; 5, 5; 6, 4; 6, 4; 5, 5; 3, 7;
5, 5; 6, 4; 3, 7; 5, 5; 5, 5; 7, 3; 5, 5; 5, 5.

It may be noticed that on the basis of the calculation in the text the observer same number of omnibuses are moving in both directions with the same average velocity, say, V; an easy calculation shows that the velocity of the pedestrian, supposed uniform, $= (0{\cdot}7 - 0{\cdot}3)V$, $= 0{\cdot}4V$. That is the absolute velocity, so to speak, referring, say, to some fixed point in the street. Accordingly the velocity of the pedestrian relative to the vehicles which are moving in an opposite direction to his is $1{\cdot}4\ V$: and relative to the vehicles which are moving in the same direction, $\cdot 6\ V$. If, then, the pedestrian could observe his own velocity relative to a great number of vehicles taken at random from the whole series—say all that at a given instant were in Piccadilly—the distance by which he would be found to gain upon the average omnibus in a unit of time would be about $(1{\cdot}4 - {\cdot}6)V = {\cdot}8\ V$. This datum might possibly have been obtained by observation, if the observer had attended to the relative velocities of the vehicles in his neighbourhood, not merely to the numbers which met him and passed him, as he walked.

The distance which the individual on foot moves relatively to the average omnibus during a unit of time may be treated as a substantive entity, an independent measure of the rate at which the individual is advancing through the crowd of vehicles. Or it may be regarded as an approximation to a perhaps more scientific *quæsitum*, the rate at which the individual is moving towards the *weighted mean* of the system. The simple average might be used for this ancillary purpose by one who had not the means of ascertaining the centre of gravity of the system, or even by one who had not formed a very clear idea of what is meant by a centre of gravity. The approximation may be expected to be very close. For the statistics now under consideration are simply related to the group above cited, representing the proportions of vehicles meeting and passing the pedestrian; and this group appears to possess the characteristic on which indifference of weighting depends, namely, *sporadic dispersion* about a constant mean.

Is it necessary to interpret the parable? The oscillating crowd of public conveyances is comparable to the long list of commodities with ever varying values—the swaying series of the logarithms [1] so taken that the difference between any two of them represents the relative value of two articles of exchange. The

would appear to be moving westward with a velocity equal to an *eightieth* of the average velocity of an omnibus; a result which differs from zero by an amount which is well within the probable error incident to the calculation.

[1] As conceived by Cournot (*Théorie Mathématique des Richesses*, ch. ii.); who very properly in this connection does not mention *weights*.

change in the distance of the pedestrian from the "weighted mean" of the system represents the *primary* monetary *quæsitum;* the change in his average distance from the other bodies in the system represents that unweighted—that is, equally weighted, or more generally randomly weighted—mean of relative prices, which may be used either as subsidiary to the primary investigation, or as an independent *secondary* measure. The position of high collateral dignity is all the more deserved in that the secondary measure enjoys an objective or external character, which cannot—according to my view of the subject—be accorded to the primary *quæsitum.*

The recognition of this sort of absolute standard, or at least of that sporadic dispersion on which it is based, demands a considerable widening of the views and softening of the strictures, which we find in the work before us. First, more attention may be claimed for a species of average, appropriate to the secondary *quæsitum*, the *Median*, which Mr. Walsh has mentioned only to reject. Again, his criticism of those who have sought to include wages with commodities in an index-number seems too harsh. Those certainly are to be condemned who confound the distinct standards, which are based on the amount of commodity which the same sum of money will procure, and the amount of effort and sacrifice which are required to procure the same sum of money. Mr. Walsh is quite justified in describing a mixture of these two species of index-number as an unmeaning "hodge-podge." But there is a secondary point of view in which these distinctions are less important: the view which seems to have been taken by some of the great men who first approached our problem. When Hume imagined every one awaking one morning with an additional coin in his pocket, when Mill improved on the idea by imagining the money in every one's pocket to be increased in a certain ratio, presumably they thought of prices in general without distinction of producers' and consumers' goods. And certainly in an alert state of competition, if such a change as Jevons proposed for the purpose of unifying international coins were carried out, namely that what is now 100 dollars should reckon as 103½, it is very conceivable that this change would rapidly propagate itself through a great variety of transactions, including those between master and servant. And accordingly, though the change in wages in each department might be liable to the same proper disturbance as the finished article (in addition to the common monetary influence), and so far as they are not *independent* observations it would not be much good including them, at the same time there would be no harm in including them in such an unweighted index-number as is now under consideration. I am not contending that wages ought in the existing state of things to be included in any kind of index-number along with finished products. I am only regretting that our author's great learning has not saved him from the common defect of original writers on the subject, an inability to perceive the many-sidedness of the problem, an exclusive devotion to one idea.

There are more things in the monetary cosmos than are dreamt of in his philosophy. Still his philosophy is of a very high order. So subtle dialectic, such logical precision, supplemented by a diligence of literary research that is quite unrivalled, if brought to bear on other economic problems, may be expected to merit a less chequered encomium. That they have not now obtained a more decided success seems due to the peculiarity of a problem which involves the more positive science of Probabilities. But, I repeat, this is an individual opinion on a much debated question. There are those who conceive the problem in a sense more favourable to Mr. Walsh. To me he seems unfortunate in his subject; to others perhaps, only in his critic.