THE PHYSIOLOGICAL COST OF MUSCULAR WORK:

A REPLY TO OBJECTIONS.

BY

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In articles published in the BRITISH MEDICAL JOURNAL of May 21st, 1921, p. 733, by Leonard Hill and J. A. Campbell, and of July 9th, p. 39, by J. B. Orr and J. P. Kinloch, objections have been raised to the validity of the method by which Waller and De Decker have arrived at a preliminary estimate of the physiological cost of various kinds of industrial work, of which a summary was published in this journal on May 7th.

These objections are not new to us, and have, indeed, already been dealt with by one of us in Dr. Waller's first interim report to the Food (War) Committee of the Royal Society,¹ and a reference to that paper might serve as a sufficient answer to them. That report, which took origin from an agreement that "Waller's method by which determinations of CO₂ alone are made at short frequent intervals, was to be taken on trial," dealt at length with all of the objections now raised *de novo* in their unaltered form without any reference to the fact that they were then, as we believe, completely met and disposed of. But inasmuch as they have not been discussed at all in this journal, and as our further experience of the method has confirmed our forecast of its utility, we are unwilling to let the opportunity pass of making a direct reply to the objections. A final and in itself sufficient reason for doing so consists in our belief, confirmed now by more than two years' experience, that by this method, as by no other, further knowledge is to be obtained regarding a primary factor in industrial life—namely, a knowledge of the physiological cost of various kinds of industrial work measured during its actual performance in the workshop and at the ship's side, in the factory, and on the road. If our belief is wrong, we have merely wasted a couple of years of our time, and all that matters is that other people's time should not be similarly wasted. But if we are right, it is important that many other people should add their endeavours to ours in the application of our method of observation.

Briefly stated, the method consists in measuring and recording the ordinate of CO₂ discharge during the working day by taking its percentage in small samples (10 to 20 litres) collected at convenient intervals during the working day with least possible interruption of the work in progress. Obviously there is nothing new in such a method; the only novelty consists in its practical application and in the subsequent translation of the CO₂ data into their equivalent calories, so as to establish comparison between our results and those of other observers for such forms of exercise to which more elaborate methods can be applied. And we take walking at moderate speed on a horizontal path as the form of exercise that most readily affords comparative data by different methods.

The objections, as stated by Hill and Campbell, and by Orr and Kinloch, are three in number:

FIRST OBJECTION: *The period (half a minute) during which our samples of expired air are taken has been too short.*

Obviously we should have preferred longer periods if such had been practicable, since on short periods the error of volume by turning the tap on and off at different phases of respiration can be very serious, and at the outset we thought it would hardly be possible to make use of half-minute samples. But after trial, and especially in cases where ventilation (during work) has high values (30 and 40 litres a minute), we found that the half-minute samples were sufficient and necessary; we learned from our first observations on dock labourers that during a three minutes rest the ordinate fell to nearly half its work value.² For normal ventilation we always take longer samples—that is, for one or two minutes or 100 seconds.

Short samples do not unduly tax the patience of the worker nor cause him to lose time. Interruptions of work for four minutes would usually be quite impracticable; nor

would it be convenient to fit him with a large bag and face-piece to collect 80 or 100 litres of his expired air. We have to use a small (20 to 30 litre) bag with a mouth-piece and a nose clip that do not require laborious adjustment. We did as a matter of fact expressly state that the error of volume inherent in short sampling might be of a serious order, but by repeated trial we became convinced that the error is less serious than we had anticipated before trial. Moreover, the serious error in any single reading is reduced to insignificance when the final result is averaged from many readings. Obviously a first sample collected from an unaccustomed subject, if found to be of markedly deficient volume, is to be rejected; it means that the subject has failed to breathe naturally into the bag.

The numbers quoted by Hill and Campbell for their unaccustomed subject, "J. A. C.," on April 1st (Table I)—namely, 1.3, 2.1, 4.1 c.cm. CO₂ per second (resting values) are evidently wrong; unfortunately the volume of ventilation and percentage of CO₂ are not stated, but, as is shown by their low probable caloric value as calculated from oxygen, it is clear that they took an incomplete sample. On April 4th, on the now accustomed subject, they get the more reasonable-looking figures—3.8, 3.7, 3.3 c.cm. CO₂ per second. Nevertheless, they bring their previous trio of absurd numbers in evidence of the unreliability of our method. We cannot admit the validity of such criticism.

The figures given by Hill and Campbell in Table II are a little more satisfactory. They show, on careful inspection, what, indeed, might be taken for granted *a priori*—that half-minute samples vary more than do four-minute samples; but they afford no means of comparing the two sets of figures to a common scale. We have been at some pains to do this, on the assumption that their heat figures were taken on a normal subject with a skin area of 1.75 square metres. These figures show far more clearly than do the original figures the actual extent to which the CO₂ figures diverge from the caloric figures of Hill and Campbell.

Data given by Hill and Campbell.				Kilo-calories per hour as calculated by us from the data.		
	Hour of Work.	Heat cost in milli-calories per cm ² .	CO ₂ in c.cm. per sec.	Theirs (from the heat data).	Ours (from the CO ₂ data).	Difference.
April 25th. Half-minute samples	2nd	5.8	17.7	365	354	-11
	3rd	6.3	19.2	397	384	-13
	4th	5.4	16.4	340	328	-12
Four-minute samples	2nd	4.6	12.7	290	254	-36
	3rd	—	11.0	—	220	—
	4th	4.2	11.2	264	224	-40
May 4th. Half-minute samples	2nd	3.3	8.3	208	165	-42
	4th	4.8	12.7	302	254	-48
Four-minute samples	2nd	5.1	13.6	321	272	-49
	4th	4.9	13.2	309	264	-45

The differences in the individual results of the two methods of calculation are here considerable, but hardly as great as might have been anticipated from the data as given by Hill and Campbell; they do not give the area of J. A. C., and we have therefore had to assume it of an average value of 1.75 M². The authors' heat figures, which are given in millicalories per square centimetre, represent single observations taken chiefly in the second and fourth hours of work, and the range of error in single observations of the kind is considerable, even in the hands of the most skilled observer. The observations are too few and too unsystematic to allow of averaging.

SECOND OBJECTION: *We (W. and De D.) have failed to place sufficient emphasis on the influence of food.*

As a matter of fact, we took no account at all of the small variations of metabolism due to the influence of food, but regarded them as negligible in comparison with the increased metabolism occasioned by muscular work. From our own observations, as well as from those of many

previous observers, we reckoned that the post-prandial increase of CO_2 might amount to one-fifth of the resting value, whereas the increase by moderate muscular work amounts to at least five times the resting value. We considered that it would be very hazardous to make any attempt to detect, far less take account of, the varying influence of food. Our neglect of this factor was, moreover, justified by the conclusion arrived at by Benedict and Murschhausen³ to the effect that "the increment (of energy discharge) due to the work of forward progression was constant, irrespective of whether the subject was with or without food." And we were concerned with measurements of CO_2 output of labourers under their normal conditions of life.

THIRD OBJECTION: *We (W. and De D.) have neglected the significance of the respiratory quotient (R.Q.) in determining the caloric equivalent of the volume of CO_2 exhaled.*

This objection, which was dealt with at length in Dr. Waller's first report cited above, is, of course, fundamental and theoretically the most important of the three, since it strikes at the root of the short method by CO_2 alone, and claims for the long method that it is the sole and only means of ascertaining the cost of work in terms of calories per unit of time and body surface.

We demur to this exclusive claim made on behalf of the longer method represented as valid, as against the shorter method represented as invalid. In our opinion, the apparent accuracy of the long method must be reduced by the notorious difficulties of reading and calculation, while the apparent inaccuracy of the short method is reduced by the facility with which relatively large numbers of observations can be made and worked out on the spot.

As stated by Dr. Waller two years ago, in his first communication on the matter to the Physiological Society in December, 1918, both methods have their proper uses—as a "low-power method" and as a "high-power method" respectively—the former being best adapted for use in the factory and on the road for the necessary preliminary survey on an extensive field, the latter for the closer determination of particular points in the laboratory. We have, of course, considered and studied very carefully the range of error incurred by disregarding the fluctuations of the caloric value of CO_2 which accompany possible fluctuations of the respiratory quotient during observation, and the adoption of a fixed factor for the calculation of the caloric value of observed CO_2 .

We used in the first instance a factor calculated upon a mean R.Q. = 0.85 and a temperature difference of 14° between air as expired and as analysed, which gave the following simple equivalence:

(1) 1 c.cm. CO_2 per second = 20 kilocalories per hour. We still use this equation for summary comparison of data on the two scales, but we now prefer to use as a simpler and more generally intelligible figure the equivalence:

(2) 1 c.cm. CO_2 = 5 calories, whereby the R.Q. is taken = 1 and the corresponding value of CO_2 rounded from the tabular value 5.047, as originally given by Zuntz (*Pflüger's Archiv*, vol. lxxviii, 1897, p. 201).

Orr and Kinloch give the following figures to show the extent of error that may occur when caloric values are calculated from CO_2 alone. Their subject was marching at a rate of 100 yards per minute.

Data and Calculations by Orr and Kinloch.

1919.	CO_2 per min.	O_2 per min.	R.Q.	Calories calculated from O_2 and R.Q.	Calories calculated from CO_2 . R.Q. assumed at 0.85.
Jan. 8 ...	743	989	0.75	4.69	(4.36)
Jan. 9 ...	865	960	0.90	4.73	(5.07)
Difference...	...	...	...	0.04	0.71

This is an extreme case, selected by Orr and Kinloch to show the extent of error incurred by the short method as compared with the accuracy afforded by the long method. Our critics ought to have taken as their illustration a group of several observations as homogeneous

as might be, and have then worked out the comparative figures afforded, with and without the O_2 readings taken into account, when, if we are not mistaken, they would have realized that the short method data do not compare unfavourably with long method data, but rather the contrary. Taking, for example, as a fair group of cases their own data for the subject M. marching at the speed of 100 yards per minute, as given in Table L on page 45 of their report on the Energy Expenditure of the Infantry Recruit in Training, we have the following figures; the total number of observations (7) is too small to allow of strict statistical inference; it is, however, the largest group number we can find in that publication.

Data by Cathcart and Orr (*loc. cit.*, p. 45).

O_2 .	CO_2 .	R.Q.	Calories per Hour.	
			Calculated from CO_2 and O_2 .	Calculated from CO_2 alone.
1,305	1,057	81	376.8	352.3
1,322	1,004	76	376.8	334.7
1,131	995	88	332.4	331.7
1,053	947	90	310.8	315.7
1,137	1,012	89	334.8	337.3
1,104	949	86	322.8	316.3
1,178	989	84	342.6	329.3
Arithmetic mean ...	...	...	342.4	331.0
Standard deviation ...	...	...	23.62	11.71
Probable error ...	...	...	15.9	7.9
Coefficient of variation ...	...	...	6.900	3.540

The results in calories per hour by the long method are here given as calculated by Cathcart and Orr in accordance with the R.Q.; those by the short method have been reckoned by us on the assumptions that only CO_2 figures were available and that 1 c.cm. CO_2 per second was equivalent to 20 calories per hour. As will be seen, the arithmetic mean obtained by the short method is only 11.4 ± 6.72 calories below that by the long method—that is, the difference is not significant. Further—and this certainly gives us some surprise—the probable error and the coefficient of variation for this particular group of observations comes out with oxygen about twice as high as without. The measurement of oxygen has evidently introduced an additional error of observation.

But this can be an accident, and all that we may claim from the two columns of figures is that the short method requires to be taken into serious consideration.

The best evidence of the validity of the short method has been afforded by its results—by those more especially that are obtained from measured amounts of work on the staircase and on the horizontal path. We insist upon the latter because it is the most universally practicable, and because it affords the readiest possible means of demonstration as well as the necessary bridging measurement from cost of known to cost of unknown work. We do not desire to overload this communication with tables of data, and shall therefore content ourselves with the following detailed account of a single observation made for the purposes of demonstration at the Institut Marey upon two French soldiers walking together at a remarkably uniform speed round a circular horizontal track of three laps to the kilometre (see table and chart, p. 629).

We have found it useful, for reasons that are self-evident, to carry out such observations upon two subjects walking together at the same pace.

Orr and Kinloch insist very properly upon the spurious fluctuations of the respiratory quotient that take place at the beginning and end of a period of work, and on the necessity of disregarding fluctuations of the respiratory quotient caused by pumping out or retention of CO_2 . They go on to say:

"It is obvious that in the method of estimating the cost of work from the CO_2 exhalation, results at the beginning of work or immediately after the work stops are of little value. In the

Institut Marey, April 27th, 1921. Sergeants S. (53 kilos) and T. (73 kilos) walking together round a circular track of 3 laps per kilometre, S. setting the pace; no load. They walked at a regular speed of 100 metres per minute (= 33 miles per hour), and covered 15 laps (5,000 metres) in 50 minutes. Samples of expired air were collected for 30 seconds over the last 50 metres of the 1st, 3rd, 7th, 10th, 11th, 13th, and 15th laps.

	Time of Lap.	Time of Sample.	Sergeant S.			Sergeant T.		
			Volume of Sample in Litres.	CO ₂ per 100.	CO ₂ in c.cm. per second.	Volume of Sample in Litres.	CO ₂ per 100.	CO ₂ in c.cm. per second.
<i>At rest (sitting).</i>								
End of 5th min. ...		60"	9.0	2.5	3.75	12.0	3.0	6.0
10th		60"	9.0	2.5	3.75	9.0	3.0	4.5
15th		60"	9.0	2.5	3.75	9.0	2.5	4.5
20th		60"	9.0	2.5	3.75	12.0	2.5	5.0
30th		60"	9.0	2.5	3.75	9.0	2.5	4.5
<i>Walking.</i>								
At end of Lap 1 ...	3' 40"	30"	11.0	3.5	12.8	16.0	3.0	16.0
" .. 2 ...	3' 35"	—	—	—	—	—	—	—
" .. 3 ...	3' 34"	32"	11.0	4.0	13.8	18.0	3.5	18.0
" .. 4 ...	3' 34"	—	—	—	—	—	—	—
" .. 5 ...	—	30"	—	—	—	15.5	3.5	18.0
" .. 6 ...	—	—	—	—	—	—	—	—
" .. 7 ...	3' 34"	33"	11.5	4.0	15.3	15.0	4.0	20.0
" .. 8 ...	—	—	—	—	—	—	—	—
" .. 9 ...	—	—	—	—	—	—	—	—
" .. 10 ...	3' 32"	30"	12.0	4.0	16.0	15.5	4.0	20.7
" .. 11 ...	—	30"	11.5	4.0	15.3	16.0	4.0	21.4
" .. 12 ...	—	—	—	—	—	—	—	—
" .. 13 ...	3' 33"	30"	12.5	4.0	16.7	17.5	4.0	23.3
" .. 14 ...	—	—	—	—	—	—	—	—
" .. 15 ...	3' 33"	30"	12.0	4.0	16.0	18.0	3.9	23.4
<i>At rest (sitting).</i>								
End of 5th min. ...		60"	9.0	3.0	4.5	10.0	3.5	5.8
10th		60"	9.0	3.0	4.5	9.0	3.0	4.5
15th		60"	—	—	—	8.0	3.0	4.0

	Sergt. S.	Sergt. T.
Average CO ₂ during walking ...	15.13	20.1
Average (before walk) at rest ...	3.75	4.9
Average net cost of walking ...	11.38	15.2

Work of walking ...	88.33 KgM. (hor.)	121.7 KgM. (hor.)
Cost of walking per KgM. hor. ...	0.129 c.cm. CO ₂	0.125 c.cm. CO ₂
That is, at 5 calories per c.cm. CO ₂ ...	0.645 calories	0.625 calories

Institut Marey, April 27th, 1921. Horizontal walk of 5,000 metres at a speed of 100 metres per minute by Sergeants S. (53 kilos) and T. (73 kilos). The heavier man discharges the greater amount of CO₂. Both men are discharging at approximately the same rate per kilo per metre—namely, 0.129 and 0.125 c.cm. (100 metres = 109.3 yards.)

former case CO₂ is accumulating in the body, while in the latter it is washed out. In neither case is there any parallelism between the production of CO₂ in the tissues and the discharge of CO₂ by the mouth."

We are in complete accord with all of these warnings and have acted upon them from the outset of our observations. Spurious fluctuations of the respiratory quotient do not concern us at all, since in our method it is

not taken into account. Effects of pumping and retention of CO₂ at the beginning and end of a spell of work are likewise left out of account by us, since we measure only the CO₂ ordinate when the constant régime of work is established.

A fourth and somewhat indefinite objection has been raised by Hill and Campbell, which, addressed as it is to matter of interpretation rather than to matter of fact, will ultimately require careful discussion after further investigation.

The matter of fact is that by our method we find regularly that the CO₂ ordinate taken at hourly intervals during several hours' work, exhibits a progressive increase towards a maximum (after several hours it exhibits a decline). The increase can of course in some instances be due to increase of work; the terminal decline is certainly due to decrease of work, and is in our view a sign of fatigue pushed to the degree of exhaustion. But in other cases where there has been no such speeding up—for example, in piecework and in military marching where the rate has been approximately constant or even declining, it must have been due to some other cause. We thought it might be due to a gradually developing insufficiency of ventilation entailing a gradually rising carbon dioxide tension throughout the system, or that it might be a sign of decreasing mechanical efficiency—that is, an early sign of muscular expenditure (objective fatigue).

But whatever the explanation may be, the fact itself is unquestionable, and cannot be disposed of by the statement that it has not previously been detected by other observers using other methods. We find the increasing ordinate on all classes of workers—on heavy workers in the docks and in cold storage, on less heavy workers in the casting-room and at the coal-face and on march; in least heavy workers, such as tailors and shoemakers. We can only repeat the tentative explanation given in Dr. Waller's first report:

"Between the two possible causes [of the gradually increasing CO₂ ordinate] (1) increase of work, (2) decrease of efficiency, I am inclined towards the second as the more probable. The root fact in fatigue is a consumption of material, presumably of carbohydrate, in first instance, also of fat and of protein; subjective fatigue is felt when a certain degree of waste has been incurred. Objective fatigue commences long before its subjective aspect is experienced; it begins indeed with the beginning of work, with increased metabolism, of which increased CO₂ is the indicator and measure, and in the progress of the expenditure there is no line of demarcation to be drawn between a state of freshness and a state of fatigue; these terms are statements of the subjective states associated with the filled and unfilled reservoir of fuel. Moreover, the progressive increase takes place with piecework where the work is of steady value."

Hill and Campbell object to our view in the following terms:

"It is well known that a fatigued or uncomfortable subject carries out a given amount of work at a greater cost than when normal, but it does not seem reasonable that the colliers and shoemakers under consideration are so unskilled and of such poor calibre as to show signs of fatigue after a couple of hours' constant work, when this work is of the degree specified."

Our critics do not seem to have formed any clear idea of the conditions under which our observations were carried out; the colliers and shoemakers under our observation were first-rate workmen; we never examined them for only a couple of hours, and they never exhibited any signs of "fatigue" as imagined by Hill and Campbell. They did, in fact, as do all kinds of workers, exhibit this progressive increase of the CO₂ ordinate (with which we observed

in the cases of the colliers a progressive increase of pulse frequency). We may or may not be right in attributing the rise of cost to decreasing efficiency. We are in presence of a new fact, and are not yet satisfied with our own or any other explanation—least of all with that suggested by Hill and Campbell, to the effect that the progressive increase has been due to food. This suggestion is contradicted by our own experience, as well as by

the far more extensive and methodical experience of Benedict and Murchhausen, as quoted above.

Conclusion.

In conclusion, we wish to repeat and emphasize our opinion that the measurement of primary importance to our further knowledge of the physiological cost of industrial labour consists in the measurement of CO_2 expired during the day's work—recording the CO_2 values observed as ordinates throughout the day.

A fixed factor for an approximate conversion of CO_2 values into calories may be used in order to compare the findings by the short method with those of other observers by the long method. In cases where both methods are applicable we find that the results come out very close together.

In our opinion the most satisfactory and accessible test case is afforded by the cost of walking on a horizontal path at ordinary speeds—80 to 100 metres per minute ($= 3$ to $3\frac{1}{2}$ miles per hour). At 80 metres per minute we find by the short method that a subject "fit for military service" expends approximately $1/10$ c.cm. CO_2 per second to carry 1 kilo of his body 1 metre horizontal—a finding that corresponds with that of other observers—for example, Benedict, of 0.5 calorie per horizontal kilogram metre.

As regards the relative (numerical) accuracy of the two methods, we should be very glad to find an opportunity of carrying out upon the same subjects simultaneous observations in parallel by the short and by the long method. We believe that it would be found that the relative accuracy of the two methods would prove to be very close by reason of the fact that the number of observations to be obtained is much greater by the short than by the long method.

The systematic measurement of the physiological cost of industrial work cannot be accomplished by the isolated effort of private enterprise; it requires the support of team work and the services of many industrious if not highly skilled professional researchers. The long method is impracticable without undue disturbance of the workman; indeed, no recent attempt has been made to apply it to "the determination of the energy output of men and women workers," or if such attempt has been made, the results have not been reported. The purpose of our observations of the last two years has been to show by actual trial at the docks, at the coal-face, and in the workshop that this physiological aspect of the industrial problem can be approached by a suitable method.

P.S., October 1st.—We are glad to learn that there is a prospect of carrying out for the Industrial Fatigue Research Board a parallel series of measurements by the long method and by the short method.

Our justification for taking the figure 0.85 as our standard value for the respiratory quotient consists in the experience of many previous observers (including ourselves), who have found that, apart from its spurious fluctuations, the true value varies very slightly from this mean value—thus, for example, in the series of observations given by Benedict and Murchhausen on p. 50 of their monograph on the *Energy Transformations during Horizontal Walking*, the average value of the respiratory quotient for the series of readings taken on four successive days—December 17th, 18th, 19th, and 20th—upon their subject, A. J. O., comes out at the identical value 0.8475 when the subject is standing at rest (twelve observations) and while he is walking at about 105 steps per minute (sixteen observations).

REFERENCES.

¹ *Proceedings*, 1919, B, vol. 91, p. 166. ² Waller, Calibration of a Dock Labourer by means of his CO_2 Discharge, *Proceedings Physiological Society*, December 14th, 1918. ³ *Energy Transformations during Horizontal Walking*, Carnegie Institution Report, No. 331, pp. 92-93.

THREE new cinematograph films have been produced by the New York State Department of Health, and are lent free of charge within the state; these films are entitled "Mouth hygiene," "Saving the eyes of youth," and "Swat the fly." The Health Department is now co-operating in the production of a film on water supply in farms; two reels are devoted to showing the rural resident how he can install a complete plumbing system in his house, and the difficult parts of the plumber's trade are shown in detail.

GLYCOSURIA OF MALARIAL ORIGIN.*

BY

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I WAS particularly interested to read the account of the two cases published by Dr. A. Castellani and Dr. J. G. Willmore in the *BRITISH MEDICAL JOURNAL* of August 20th, 1921. Recently Sir Charlton Briscoe asked me to investigate a case of glycosuria, and the conclusion was reached that the condition must have been due to malaria. On referring to several textbooks of medicine, no mention of glycosuria of malarial origin could be found. Castronuovo, however, regards malaria as important in the etiology of some cases of diabetes mellitus. (See *Medical Science*, iv, 6, 497, September, 1921.)

A Jewish merchant, aged 32, went to Bombay in 1919. In March, 1920, he was in bed for three weeks with "dysentery and colitis." He then went to Indore for further treatment and to convalesce. Whilst there he developed malaria. Parasites were found in his blood, but for three weeks only did he continue quinine treatment. The ague stopped, but he had a relapse a month later. During the following six months he had a series of relapses, but did not take any more quinine. In March, 1921, he sought professional advice on account of excessive thirst. He thought that he had lost about 7 lb., but did not consider that he had been passing an unusual amount of urine; he never had to rise at night to micturate. He had been much worried by his business. Sugar was found in his urine, and a diagnosis of diabetes mellitus was made. Carbohydrates were reduced; the glycosuria decreased considerably, but was still present when he left India in May, 1921. In June, after arrival in England, sugar was again discovered, but in July Sir Charlton Briscoe found no trace.

The patient stated that he had had a "mastoid abscess and glands" at 14; an operation for removal of adenoids at 16; scarlet fever at 18; and an operation for removal of nasal septum at 27 under a local anaesthetic. The operations were successful; no post-operative complications were noted. His father died at 45 of "creeping paralysis." His mother is alive and well, aged 61. He has one brother and two sisters, all very healthy. His maternal grandfather and maternal grandmother both died from "diabetes" at the age of 72 and 70 respectively.

The patient first saw me on June 28th, 1921. He had just recovered from another relapse of malaria, having had ague on June 23rd, 25th, and 27th at mid-day. I examined samples of blood and urine, and advised him to come into hospital for further investigation.

Blood.—Fairly large number of benign tertian malarial parasites present; haemoglobin 90 per cent.; red blood corpuscles not counted; white blood corpuscles 9,600, polymorphs 73.6 per cent., eosinophils 0.4 per cent., lymphocytes 24.4 per cent., large mononuclears 1.2 per cent., mast cells 0.4 per cent.

Urine.—No albumin, no sugar, no acetone nor aceto-acetic acid; urobilin present in large amount.

He came into hospital on the evening of June 29th; his blood still showed malarial parasites, but they were fewer in number than on the previous day. There had been no further attack of ague. No quinine had been taken before admission; 10 grains were given by mouth later in the evening; 30 grains a day were prescribed for the next three days, followed by 20 grains daily for a fortnight, and 10 grains daily for a further two months. The urine again contained no sugar. A full diet, including sugar, was ordered.

On examination on June 30th, 1921, the patient was well nourished, the skin slightly pigmented, the conjunctivae had a sub-icteric tinge, the glands were nowhere enlarged, and his general condition appeared excellent. Pulse, temperature, and respiration were normal. His weight (in clothes) was 11st. 9 lb. His teeth were in good condition, there was no pyorrhoea; nothing abnormal was to be seen in the mouth or throat. Thyroid, heart, lungs, and abdomen all appeared normal. Blood pressure was 105/85; liver and spleen were not palpable. His pupils reacted to light and accommodation; knee-jerks were present; the plantar reflex was flexor.

A specimen of the twenty-four hours' urine (52 oz.) examined on July 1st contained a very slight trace of sugar, no albumin, no acetone, no aceto-acetic acid, but a trace of urobilin. The sugar did not exceed 0.2 per cent. in concentration. Typical glucosazone crystals were easily obtained. No rotation was observed with the crude polarimeter available. After fermentation with yeast, the urine no longer reduced Benedict's solution, yet no gas had been collected in the inverted tube. Bial's test, and the phloroglucin hydrochloride test were both negative. The reducing substance was therefore, in all probability, glucose. Wassermann reaction, negative.

Sixteen different specimens of urine were tested for sugar in the next five days, all with negative results. The volume of the urine in the twenty-four hours was always normal, the highest measurement recorded being 54 oz. During his week's stay in hospital the following results of tests were obtained.

* This investigation will be included at a future date in a report to the Medical Research Council.