The *Dublin Review* remarks:—

"The secret of his success has been that, in addition to chemical analysis, he has used the microscope in his inquiries, and his merit not only consists in the able manner in which he has employed the instrument, but in his being the first to use it practically and to such an extent for this purpose."

In an article on the same work, written in January, 1855, the *Lancet* remarks:—

"It is now unnecessary to say how completely Dr. Hassall dispelled the delusion as to the circumscription of science, and how he demonstrated that the microscope, wielded by the skilful naturalist and chemist, was able to unravel and to analyse the component structures of substances that bid defiance to the blow-pipe and the test tube alone. It is the great and original merit of Dr. Hassall to have applied the microscope to important uses in inquiries of this nature, and to have shown by its uses, not only many things previously considered impossible to show, but many things not previously suspected to exist."

Lastly, the *Times* writes:—

"The microscope seems to have been the more effective instrument in the work."

We have now shown that the assault made upon Dr. Hassall by Dr. Letheby was uncalled for and unmerited, and that nothing has been stated in the whole of the correspondence now before the public to detract in the slightest degree from the high reputation which Dr. Hassall has justly earned for himself by the eminent share which he has had in the prosecution of these inquiries. And we confidently believe that so far from his former friends dropping from him, as Dr. Letheby anticipates, in disgust, he will find that the injustice which has been directed against him will not only confirm them in their attachment, but will be the means of attracting new ones to his side.

APPENDIX.

STATEMENTS

Made by Dr. Letheby, with the Replies.

STATEMENTS.	ANSWERS.
1. Dr. Letheby states, that he made "EVERY ONE" of the important analyses.	1. It is proved by Dr. Letheby's own accounts that the analyses actually made by him amount to 165, while he has himself limited his own claim to 205 samples. That is, Dr. Letheby made 165 analyses, many partial only, as against 2,481, the number made by Dr. Hassall, and many of which were both microscopical and chemical. (See Report by the Rev. R. S. Daniell and Mr. Bolton., p. 31.)
2. Dr. Letheby states, "in the month of June, that is, *directly after the Commission began its labours*, I was requested by Dr. Hassall to give him assistance, &c.; in fact, on the 14th of that month he forwarded specimens of cocoa ash for analysis."	2. The Commission commenced its labours in the latter part of 1850. The first Report appeared in the beginning of January, 1851. At the period named by Dr. Letheby upwards of 800 analyses and examinations had been published by Dr. Hassall before any reference was made to Dr. Letheby. Again: *the analyses of snuff-ashes and opium, which constituted nearly two-thirds of the entire number of analyses made by Dr. Letheby, were made during the latter part of* 1853.
3. Dr. Letheby states, "In many cases, indeed I may say, in most, he just got far enough with the inquiry to become embarrassed, and then his products were sent to me for completion or his results for correction."	3. This is certainly a most incorrect statement, and not according to the facts of the case.
4. Dr. Letheby writes, "Latterly, *as he* (Dr. Hassall) *most truly says*, I have been constantly engaged with him."	4. This statement is equally incorrect. No such admission was ever made by Dr. Hassall.

5. Dr. Letheby states that Dr. Hassall published certain analyses by himself in his Review of Dr. Golding Bird's Urinary Deposits, "with a mere apology in the way of acknowledgment."

6. Dr. Letheby states, that "the entire Chemistry," of Dr. Hassall's Paper on Indigo in the Human Urine, published in the Transactions of the Royal Society, "is a verbatim copy of my notes to him on the subject."

7. Dr. Letheby states that his note on Indigo was reprinted without the merest shadow of acknowledgement.

8. Dr. Letheby writes, "Dr. Hassall states, that up to the last few days we have been in friendly communication, and that no word of dissatisfaction has escaped me. *This is not strictly true,* &c." And again Dr. Letheby states in another letter, "Since that time our correspondence has only been of a business character."

9. Dr. Letheby states, "I wrote a strong letter of censure to him;" and again "I wrote to him (Dr. Hassall) in the severest terms."

10. Dr. Letheby states that the Analytical Sanatory Commission "was composed of three persons,—the present writer (himself) as analytical chemist, Dr. Hassall the microscopist, and Mr. Miller the artist."

"11. Dr. Letheby states, in evidence before the Committee of the House of Commons, "For nineteen years before that I was engaged by the late Dr. Pereira in making investigations for his great work on *Materia Medica.*"

5. The analyses made were 5 in number; they were of things sent by Dr. Hassall. Dr. Letheby's name is attached to one of them, and the following acknowledgment is made with reference to the others: "It should be stated that in the above analyses of the urates, we have received great and valuable assistance from Dr. Letheby." This, then, is what Dr. Letheby calls "a mere apology in the way of acknowledgment."

6. It has been shown that this statement is incorrect. (See Dr. Hassall's reply, p. 54.)

7. The acknowledgment actually made was as follows: "For, as suggested by my friend Dr. Letheby, to whom *I am much indebted for the aid afforded in these analyses,*" &c. Besides this, Dr. Letheby's name is specially mentioned in the paper in four other places.

8. See conclusion of Dr. Hassall's third letter to the *Times.* The paragraph there quoted was written several months after the completion of the *Lancet* Reports, and the publication of Dr. Hassall's work, entitled "Food and its Adulterations." It ends "with kind regards," &c.

9. This is certainly most untrue. The letter in question is still in existence, and it does not contain *a syllable of censure.*

10. It has been proved that this statement is likewise utterly incorrect. See p. 53.

"11. The work of Dr. Pereira does not contain any acknowledgment whatever of services rendered by Dr. Letheby in the production of that work."

SUMMARY OF ACCOUNTS OF ANALYSES

Performed by Dr. Letheby at the request of Dr. Hassall, for the Sanatory Commission of the Lancet.

Dr. Hassall has placed before us certain letters, accounts, and charges in the handwriting of Dr. Letheby.

These were sent in at different periods during the last four years, and they specify the nature and number of the analyses performed by him during that period at the request of Dr. Hassall for the *Lancet* Sanatory Commission. They have been submitted to a careful examination by us, and we now report upon them to the following effect: —

From these documents it appears that in 1851 twenty-one analyses were made by Dr. Letheby in all: 10 of these were ashes of cocoa, one being a quantitative analysis and the rest merely qualitative, whilst the remaining 11 samples consisted of breads and flours which were analysed for alum only.

That in 1852 twelve analyses were made by Dr. Letheby; of which 8 were of samples of ale, 3 of vinegar, and 1 of cayenne.

That in 1853 one hundred analyses were made by Dr. Letheby; of which 8 were of samples of tobacco, 12 of Manilla cheroots for opium, 43 of snuff ashes, and 37 of powdered opium.

That in 1854 the number of analyses performed by Dr. Letheby was thirty-two; of which 16 were of powdered opium, 10 of blue sugar confectionary for the blue pigment only, 2 of snuff, and 4 of samples of gin.

The accounts rendered by Dr. Letheby, being so clear and in his own handwriting, furnish indisputable and conclusive evidence of the actual number of analyses performed by him, some of them being partial analyses only. Thus it is distinctly shown that the entire number of chemical analyses performed by Dr. Letheby, viz. 165, bears but a very small proportion to the number of the analyses, as well microscopical as chemical, performed by Dr. Hassall, viz. 2481.

Of these samples 63 were of tobacco and snuff, and 53 of opium; thus leaving 48 samples of all other kinds; but it is to be especially noted that the analyses of these 53 samples of opium are not recorded in Dr. Hassall's work on "Food and its Adulterations."

As witness our hands this first day of August, One thousand eight hundred and fifty-five.

Raymond S. Daniell, M. A., Oxon.
George Bolton.

9, Queen Street, Brompton.

In this statement of the number of analyses made by Dr. Letheby in each year, the dates given in his own account are followed.

MEMORANDUM OF AGREEMENT

Between Mr. Wakley and Dr. Hassall.

Mr. Wakley agrees that Dr. Arthur Hassall shall have the sole right to republish for his own benefit, and in his own name, in a form separate from the *Lancet*, all reports and articles under the title of the Analytical Sanatory Commission projected by Mr. Wakley, and being accounts of analyses of food and drugs written by Dr. Hassall, as well those which have heretofore been as those which may be furnished by him to and published in the *Lancet*, before the twenty-fifth day of December, One thousand eight hundred and fifty-four.

First Title as to the Reports on Food.

Reports from January 1st, 1851. to December 31st, 1854, inclusive, revised and extended, of the Analytical Sanatory Commission of the *Lancet* on "Food and its Adulterations;" being Records of the Results of some Thousands of Microscopical and Chemical Analyses of the Solids and Fluids consumed by all Classes of the Public. By Arthur Hill Hassall, M. D., Chief Analyst of the Commission, Physician to the Royal Free Hospital.

Second Title.

Reports from January 1st, 1851, to December 31st, 1854, inclusive, revised and extended, on Food and its Adulterations, originally published in the *Lancet;* being Records of the Results of some Thousands of Microscopical and Chemical Analyses of the Solids and Fluids consumed by all Classes of the Public. By Arthur Hill Hassall, M. D., Chief Analyst of the Commission, Physician to the Royal Free Hospital.

First Title as to the Reports on Drugs.

Reports, revised and extended, of the Analytical Sanatory Commission of the *Lancet*, on Drugs, Chemicals, and Pharmaceutical Preparations, with their Impurities, Adulterations, and Falsifications; being Records of the Results of some Thousands of Microscopical and Chemical Analyses. By Arthur Hill Hassall, M. D., Chief Analyst of the Commission, Physician to the Royal Free Hospital.

Second Title.

Reports, revised and extended, on Drugs, Chemicals, and Pharmaceutical Preparations, with their Impurities, Adulterations, and Falsifications; being Records of the Results of some Thousands of Microscopical and Chemical Analyses, originally published in the *Lancet*. By Arthur Hill Hassall, M.D., Chief Analyst of the Commission, Physician to the Royal Free Hospital.

Not any such Report on Food to be republished by Dr. Hassall until after the expiration of one year subsequent to its first publication in the *Lancet*, unless with the consent in writing of Mr. Wakley, his executors, administrators, or assigns.

The republication of the Reports on Drugs not to include any report that has been published in the *Lancet* at a less period than two years from the time of its first publication in that journal, unless with the consent in writing of Mr. Wakley, his executors, administrators, or assigns.

It is also agreed that if Dr. Hassall, or his assigns, republish the Reports on Food aforesaid, he or they shall, on or before the 25th day of December, 1855, purchase all the engravings used in such reports at one half of the original cost of such engravings, and of the drawings from which they were made, this half amounting, to this date, 3rd September, 1853, to the sum of 29*l.* 12*s.* 3*d.*, the half cost of all subsequent drawings and engravings to be ascertained and computed from the accounts of the artist and engraver.

It is further agreed between the parties subscribing to this memorandum, that if Dr. Hassall, his executors, administrators, or assigns, shall not have republished the reports and articles, consent in writing, when necessary, having been given, or have purchased the drawings and engravings on or before the 25th day of December, 1855, then Mr. Wakley, his executors, administrators, or assigns, shall be at liberty to republish the said reports and articles in a separate form, as the same originally appeared in the *Lancet*. And further, in case the said Dr. Hassall, his executors, administrators, or assigns, shall, on or before the 25th day of December, 1855, have republished the said reports and articles in a separate form, then it shall be lawful for the said Mr. Wakley, his executors, administrators, or assigns, to republish the said reports and articles as the same originally appeared in the *Lancet*, also in a separate form, at any time after the expiration of five years from the date of the republication by Dr. Hassall; the right, however, of Mr. Wakley so to publish in a separate form is not in any manner to limit or restrict Dr. Hassall's right of republication when, and as often, as he pleases.

In the event of a republication by Mr. Wakley, his executors, administrators, or assigns, of any portion of the *Lancet* at any time, he reserves the right to the use of the engravings for such purpose; but the part or parts of the *Lancet* so to be republished are not to consist of any collection separate and apart from the other contents of the *Lancet* (except as herein-before provided) of the reports and articles, the right to the republication of which is hereby vested in Dr. Hassall, his executors, administrators, or assigns.

Provided also that nothing herein contained shall prevent the republication by Mr. Wakley of any occasional report or article as aforesaid.

But this shall not authorize Mr. Wakley to republish such reports or articles at stated periods, or in a collected form, except as herein-before mentioned, viz., five years after their first publication by Dr. Hassall, his executors, administrators, or assigns.

THOMAS WAKLEY.
ARTHUR H. HASSALL.

Witness, WILLIAM HENRY STRANGE.

Dated 12th Dec. 1853.

An accident by fire having occurred at Messrs. Savill's since this agreement was first drawn up, by which it is understood that some of the engravings have been destroyed, it is further agreed that Dr. Hassall shall only be required to purchase at the rate before stated such engravings as Mr. Wakley shall be enabled to produce.* It is further agreed that Dr. Hassall shall purchase all the drawings and engravings used to illustrate the reports on drugs and their adulterations on the same terms on which he is to purchase those on food and its adulterations. It is likewise agreed that should Dr. Hassall have occasion to have new engravings prepared in consequence of the accident beforesaid, that Mr. Wakley shall be entitled to the use of such engravings should he republish the reports as before described, on terms to be mutually agreed upon, or if necessary by arbitration in the usual manner; it being understood that the engravings are to be returned to Dr. Hassall, his executors, administrators, and assigns.

THE following are the more important parts of the leading article, which appeared in the *Times* for July 24th, 1855, and which gave rise to the correspondence printed below: —

"Some thirty years ago, the British public was frightened by the cry of 'Death in the pot;' but we might now, it seems, re-echo the alarm with greater force than ever. Death is not only in the pot, it is everywhere; not only in our food and our drink, but in the very medicines which should cure our diseases. The matter is now under investigation before a Parliamentary Committee, and it has been shown by evidence of the most convincing kind that of the articles of daily use and first necessity a very great portion is subject to foul and systematic adulteration.

"But how, the reader may ask, has the discovery at this particular

* It being found on examination that the whole of the wood engravings were destroyed, Dr. Hassall had to be at the entire expense of their re-execution.

period been made or certified? Partly through material improvements effected in the means of detection, but mainly by the skill and perseverance of Dr. Hassall, who, by devoting to this subject the energies of a scientific mind, and pursuing it with that steady zeal which its importance justified, has thus become a public benefactor of no common order. If gratitude is due to those who discover antidotes to disease, or invent appliances for relieving pain, the same obligation must undoubtedly be admitted to the man whose researches, by detecting the hidden seeds of sickness, must directly tend to prolong life and increase its comforts.

"The facts of the case, though already more or less known to the public, are really startling when presented in so large a mass and on such formal authority. 'In *nearly all* articles,' said Dr. Hassall before the Committee, 'whether food, drink, or drugs, my opinion is that adulteration prevails,' and 'many of the substances,' he added, employed in the adulterating process were not only injurious to health, but even poisonous. Anatto, arrowroot, anchovies, bread, coffee, chicory, currypowder, and gin are some of the articles which were enumerated in alphabetical order as visibly and incontestably subjected to this treatment; while 'sulphate of copper, verdigris, Brunswick greens, whitelead, and sulphate of lime,' were specified as among the substances used for the purposes of adulteration. The investigations which led to these agreeable results were conducted in the most practical and businesslike manner. Samples of the articles to be analysed were purchased to the number of twenty or forty specimens from different classes of traders, the powers of the microscope, as well as other tests, were then brought to bear upon them, and a report was drawn up embodying the general conclusions. The microscope seems to have been the more effective instrument in the work. Less than five years ago, for instance, it would, we are told, have been impossible to detect the presence of chicory in coffee. In fact, the opinion of three distinguished chymists was actually quoted in the House of Commons to that effect, whereas by the use of the microscope the differences of structure in these two substances, as in many other cases, can be promptly discerned. Out of thirty-four samples of coffee purchased at the outset of the investigation, chicory was discovered in thirty-one, chicory itself being also adulterated with all manner of compounds. There is no falling back either upon tea or chocolate, for these seem rather worse used than coffee. Tea is adulterated, not only here, but still more in China; while as to chocolate, the processes employed in corrupting the manufacture were described as 'diabolical.' 'It is often,' said a witness, 'mixed with brick-dust to the amount of ten per cent., ochre twelve per cent., and peroxide of iron twenty-two per cent., animal fats of the worst description, and rancid tallow.'

* * * * *

"And now for the practical question,—how are we to put a stop to such proceedings? Let no readers suppose that they are exempted by their position from the evils under discussion, for Dr. Hassall expressly deposed that he 'had not observed any great difference in the samples procured from wealthy or the poorer neighbourhoods.' However rich, indeed, a man may be, he cannot grow his own tea or his own drugs. For beer, bread, preserves, pickles, &c., it is possible, now

and then, to rely upon home manufacture; but even for some of the materials used in these very processes we must be dependent on the ordinary trade supply, while in an infinite variety of cases we must take either what is offered for sale or nothing. No doubt, great security may be obtained by a judicious selection of shops, but these means of choice are not within everybody's reach, and besides, no shops, however humble, ought to deal in adulterated articles. Poor customers demand even more protection than rich. Of course, very considerable and immediate benefit will result from this exposure itself; in fact, Dr. Hassall observed that a marked difference was already noticeable in the shop windows with regard to such articles as preserved fruit and vegetables, formerly coloured with deleterious compounds; for housekeepers will please to understand that pickles can be excellent without being bright green, just as anchovy sauce can be perfect without being red. Still, it will be very desirable that more direct measures should be adopted to remove so serious an evil, and it does not seem that any sufficient agency for this purpose exists. Perhaps some machinery of inspection, attended with a publication of the results, would be found most effective, but for the present we suspect the public will find the best safeguard in their own vigilance, aided by the light which this seasonable inquiry cannot fail to throw over every part of the case. We only trust that such services as Dr. Hassall has rendered in the matter will not be soon forgotten. It is through researches like his that what would be otherwise mere suspicion, even if it were not treated as fable, becomes producible as fact, and that truth is at length put before our eyes so palpably as to forbid either indifference or doubt."

The subjoined letters comprise the whole of the correspondence which ensued on the publication of the above article. The editor of the *Lancet,* in republishing these letters in his Journal, omitted Dr. Hassall's second and principal letter, giving only one or two short passages, excusing himself on the plea that he could not find space for it. When it is remembered that Dr. Hassall contributed the whole of the Reports of the recent Sanatory Commission for a period of four years, that scarcely a number of that Journal appeared without its containing some of his writings, and that those writings brought both profit and reputation to the *Lancet,* it might have been thought that Mr. Wakley would not have denied Dr. Hassall the right of making known to the readers of that Journal what he had to say in his own defence, and that he would have made room for the letter in question. The injustice of

giving part only of the correspondence is, however, so obvious, that further remark on such a proceeding is unnecessary.

At the request of his friends, Dr. Hassall has submitted this correspondence, as also the *Literary Gazette* for 4th August, 1855, to the opinion of counsel, and that opinion is, that the articles in question contain matter of an actionable character.

LETTER I.

ADULTERATION OF FOOD.

To the Editor of the "Times," July 26th, 1855.

Sir,—A leading article in the *Times* of Wednesday, on the evidence given before Mr. Scholefield's Committee on the Adulteration of Food, contains expressions which are likely to raise the credit of one person at the expense of many, — I allude to the eulogium which the writer addresses to Dr. Hassall. It is, however, but right to say that, although Dr. Hassall is undoubtedly entitled to much praise for the manner in which he has performed his part in exposing the frauds practised in food and drink, yet others have been employed with him in the same work who are entitled to an equal share of public esteem; for example, it was Mr. Wakley who originated the idea of a Sanatory Commission, and it was he who planned the arrangements necessary to put it into operation. He it was who paid all the expenses of the inquiry, and who was at the cost of publishing the results in the *Lancet,* and who also bore the risk which was attendant thereon. A poor artist of the name of Miller made the microscopic examinations and drawings, without which the work of the commission would have been very incomplete Dr. Letheby, of the London Hospital, conducted all the important chymical analyses; and Mr. Postgate, of Birmingham, was really the agent of public agitation, whereby this inquiry of Mr. Scholefield's has been instituted.

I think, Sir, you will admit that, in justice to all these gentlemen, the preceding facts should be recorded, and that you will see the force of my motto, —

PALMAM QUI MERUIT FERAT.

LETTER II.

FOOD AND ITS ADULTERATIONS.

To the Editor of the "Times," July 27th, 1855.

Sir, — Your correspondent in his letter of to-day has somewhat contravened the spirit of his excellent motto, *Palmam qui meruit ferat.*

This letter contains certain inaccuracies, which, as they relate to a subject now of public importance, and concern closely myself, I beg to be permitted to rectify.

I will notice the several points of the letter in the order in which they occur.

The Analytical Commission of the *Lancet* took its origin under these circumstances: —

Early in 1850 I read a paper on the subject of coffee and its adulterations, in which I recorded the results of the application of the microscope to the detection of adulteration, before the Botanical Society of London. A very favourable notice of this paper appeared in nearly all the daily journals, including the *Times*. I forwarded the original article itself to the *Lancet*, and proceeded to follow up the subject by preparing another report on sugar, intended for the same society. After a time Mr. Wakley, with whom I had not previously held any communication on the subject, wrote to me, and asked me whether I thought a series of articles could be so prepared as to admit, without very great risk, the publication of the names and addresses of the parties of whom the goods analysed were purchased? I replied that they could, due caution being exercised. I then engaged to furnish the articles, Mr. Wakley devising the title under which they were to appear, and undertaking to publish the names — a very bold proceeding, and one entitling him to the highest praise.

In conformity with this engagement, I supplied the reports for a period of four years. I employed my own agents in the purchase of the samples, accompanying them on all occasions myself, for greater security. I made all the necessary investigations, microscopical and chymical, unaided, except latterly in some few instances, where extra chymical assistance was required, or where a second opinion on a chymical question was requisite. I determined the order of the several reports, and I wrote them all, with a single exception, up to the expiration of the four years, when, acting under a written agreement with Mr. Wakley, I published them in my own name under the title of *Food and its Adulterations*.

The manner in which these inquiries were conducted may be judged of by the fact that, although some thousands of analyses were made and names published, in one instance only was an action brought, and this was abandoned at an early period. In republishing, with additions, the reports in my own name, I incurred the whole expense; and also the responsibility of the renewed publication of the names.

In these inquiries no one was associated with me, but during the latter part of the period I was authorised to employ, to a certain extent only, chymical assistance when necessary, and in the exercise of this discretion I employed my friend Dr. Letheby, whose services I had much pleasure in acknowledging in my work.

Such is a brief sketch of the origin and history of the Sanatory Commission of the *Lancet*.

I will now, in as few words as possible, point out the inaccuracies contained in your correspondent's letter. He writes: "A poor artist, of the name of Miller, made the microscopic examinations and drawings."

Mr. Miller, who had been in my exclusive occupation for many years of his life, and who was specially educated as a microscopic artist by myself, made, in my own house, and under my own immediate instructions, the necessary drawings, but he did not, as stated, make the microscopic examinations, for which, indeed, he was not competent; neither is it correct to say that Dr. Letheby "conducted all the important chymical analyses."

Having made these material corrections, may I ask, after having assigned the chymical investigations to Dr. Letheby, and the microscopical examinations and drawings to Mr. Miller, what part either of the work or of the merit your correspondent reserves for me, which would justify the eulogium which, at the same time, he is pleased to confer on me?

I have the honour to remain,
Your very obedient servant,
ARTHUR H. HASSALL, M. D.,
Member of the Royal College of Physicians.

Bennett Street, St. James Street, July 26.

LETTER III.

To the Editor of the "Times," July 30.

Sir, — As my name has been very emphatically mentioned by Dr. Hassall in his letter in the *Times* of to-day, it is but right that I should offer an explanation of the part which I have performed in the work of the Lancet commission.

Dr. Hassall states that "in these inquiries no one was associated with me, but during the latter part of the period I was authorised to employ, to a certain extent only, chymical assistance when necessary, and in the exercise of this discretion I employed my friend Dr. Letheby, whose services I had much pleasure in acknowledging in my work." Again, Dr. Hassall further remarks, "Neither is it correct to say that Dr. Letheby conducted all the important chymical analyses."

Both of these statements contain grave inaccuracies, for as early as the month of June, 1851 — that is, directly after the commission began its labours* — I was requested by Dr. Hassall to give him assistance in his investigations; in fact, on the 14th of that month he forwarded specimens of cocoa-ash for analysis, for he could not say exactly what the nature of the colouring matter was. Before the year was out I had examined all the samples of bread, flour, and salt which were necessary for his reports on this subject.

From that time until the present I have been in constant commu-

* This is incorrect. The commission commenced its labours during the latter part of 1850, and the first Report appeared in January.

nication with him—directing him in the conduct of all the unimportant chymical analyses, and I myself making every one of the difficult and important ones. In truth, he invariably wrote to me when he began a new inquiry, and asked for instructions how he was to proceed with the chymical part of the investigation. In many cases, indeed I may say in most, he got just far enough with the inquiry to become embarrassed, and then his products were sent to me for completion, or his results for correction.* Latterly, *as he most truly says*, I have been constantly engaged with him; and, to speak the truth, I have made hundreds of analyses for him.†

In conclusion, I will put it to any practical chymist whether it was possible for any man to proceed with an important research who would ask such questions as the following?—

"Will you kindly give me the process for the quantitative analysis of iron? Is the Prussian blue which subsides on the addition of ferrocyanide of potassium soluble in excess of the precipitant?"

Again, after having received instructions from me respecting the examination of porter, he says:—

"I am getting on very well with the porters, but one difficulty which presents itself to me is the best method of detecting sulphate of iron in porter. It would not be thrown down, I believe, either by the acetate of lead or the sulphuretted hydrogen used, in which case, if present in the porter, it ought to be found in the sugar obtained by the evaporation of the fluid portion of the porter freed from acetate of lead. Is this so? A line to steer me right in the matter would greatly oblige."

I have many such questions as these. They are in Dr. Hassall's own handwriting; and I regret he has made it necessary that I should allude to them.

I am informed by Mr. Scholefield that my evidence will be required by the parliamentary committee, and I shall then take the opportunity of saying what I really have done in this matter.

I remain, Sir, yours obediently,

H. Letheby, M.B.,

Professor of Chymistry and Toxicology in the Medical College of the London Hospital.

London Hospital, July 27.

* This statement is entirely incorrect.

† Such an observation as the above was never made by Dr. Hassall. Dr. Letheby here attributes to Dr. Hassall an admission in his own favour, for which there is not the smallest authority.

LETTER IV.

To the Editor of the "Times," July 30.

Sir,—Nothing less than a feeling of necessity has induced me to trouble you with this communication. A conflict of claims on the subject to which it relates cannot be otherwise than offensive to good taste. I trust, however, that I shall be pardoned for noticing some very striking inaccuracies in the letter of Dr. Hassall which appeared in your paper of yesterday. Allow me to quote the passages to which I refer. Dr. Hassall says that.—

"The Analytical Commission of the *Lancet* took its origin under these circumstances:—

"Early in 1850 I read a paper on the subject of coffee and its adulterations, in which I recorded the results of the application of the microscope to the detection of adulteration, before the Botanical Society of London . . . . . After a time Mr. Wakley, with whom I had not previously held any communication on the subject, wrote to me, and asked me whether I thought a series of articles could be so prepared as to admit, without very great risk, the publication of the names and addresses of the parties of whom the goods analysed were purchased? I replied that they could, due caution being exercised. I then engaged to furnish the articles, Mr. Wakley devising the title under which they were to appear, and undertaking to publish the names—a very bold proceeding, and one entitling him to the highest praise."

Thus, it will be seen, Dr. Hassall dates the origin of the Analytical Commission from 1850—only five years since, and ascribes, inferentially, the cause of it to a paper written by him in that year on the subject of coffee and its adulterations. Now, what are the facts? The plan of the Analytical Sanatory Commission was put in operation by me so long ago as 1830—just a quarter of a century since; and Dr. O'Shaughnessy, now in India, who has acquired a world-wide reputation, was engaged by me to conduct the investigations. In the following year a paper by him, entitled "Poisoned Confectionary—detection of Gamboge, Lead, Copper, Mercury, and Chromate of Lead in various Articles of Sugar Confectionary," by W. B. O'Shaughnessy, M.D., was published by me in the *Lancet* of May 14. 1831. From that document I quote the following passage:—"I had, as far back as a year since, been requested by the editor of the *Lancet* to undertake a series of analytic investigations into the truth or inaccuracy of various alleged adulterations, with the view that the authenticated information thus obtained might either dissipate needless apprehension, by pointing out the falsity of many alarming statements, or might lead to the efficient protection of the public health, by showing, as far as analysis could teach, what were the admixtures really prejudicial and essential to be prohibited. Different circumstances, unnecessary to particularise here,

combined to delay the commencement of these inquiries until a fortnight since."

The evidence of Dr. O'Shaughnessy, therefore, is conclusive that he undertook for me a series of analytical investigations in the year 1830; and I actually published a report from him in 1831. Soon after that period Dr. O'Shaughnessy obtained an appointment under the Government, and afterwards proceeded to India. The results of Dr. O'Shaughnessy's inquiries were so important that he laid them before the Secretary of State for the Home Department, but nothing at that time was done by the Government with reference to the subject.

In 1836 I again took active measures with a view to the exposure and prevention of adulterations of food, and I obtained the assistance of Mr. T. H. Henry, who was then very young, but who since that time, in consequence of his great scientific acquirements, has obtained a just celebrity as a philosophic chymist, and is now a fellow of the Royal Society. In reply to a letter addressed to that gentleman this morning the following has been received:—

18. Lincoln's-inn-fields, July 27. 1855.

Sir,—In reply to your inquiry, I beg to state that I believe the idea of publishing reports of the analytical examination of articles of food, drugs, &c., together with the names and addresses of the persons from whom the articles were purchased, originated with Mr. Wakley more than twenty years ago. I was myself engaged professionally by Mr. Wakley, by whom all the expenses of my inquiries were paid, in making such investigations from October, 1836, to May, 1837. I have not now a list of the substances examined by me, but it was furnished to Mr. Wakley at the time, with the names and addresses of the various shopkeepers from whom the articles were obtained. On receiving a valuable professional appointment, my engagement with Mr. Wakley was brought to a close before my investigations were completed.

I remain your obedient servant,

T. H. Henry, F.R.S.

To the Secretary of Mr. Wakley.

The absurdity, therefore, of attributing the origin of the Analytical Sanatory Commission to any paper written in 1850 or to any suggestion from Dr. Hassall in that year is, therefore, too palpable to require any further notice; but I cannot refrain from introducing in this place a single sentence from a leading article in the *Lancet*, which accompanied the report of the Analytical Sanatory Commission, published on the 4th of January, 1851:—"We re-enter upon the labour (which we actually commenced in 1831) with a full sense of the responsibilities it involves and a full determination to cope with them, come in what shape they may, satisfactorily and successfully." In the long interval between 1831 and 1836, and between 1837 and 1850, the subject at different periods had occupied much of my attention, and I continued to be more and more impressed with the conviction that the public, and especially the juvenile portion of it, suffered greatly from the practice of adulteration which so generally prevailed. At different times I contemplated devoting a distinct periodical to the subject, with a view to excite such a degree of attention as would ultimately procure the interference of the Legislature. Multifarious occupations prevented my carrying that purpose into effect until 1850; but it will be seen from the following note that my attention was fully directed to the question in the two years preceding that period:—

13. Caroline-street, Bedford-square, July 28. 1855.

Dear Sir,—In reply to your question, I beg to state that in 1848 and 1849 I was requested by you to assist in making the requisite preparations for publishing a periodical which should contain regular reports of the analyses and adulterations of food and other articles of consumption, together with the names and addresses of the persons from whom the examined articles were bought, and at your dictation I wrote at Harefield Park the introduction or preface of the intended periodical. This certainly occurred at least two years before the report of the Analytical Sanatory Commission of January 4. 1851, appeared in the *Lancet*.

I am, dear Sir, yours truly,

George S. Brent.

To Thomas Wakley, Esq.

Dr. Hassall has admitted in his letter that I am the author of the title, "Analytical Sanatory Commission," and I think it will be admitted, after a perusal of the foregoing documents, that it would only have been consistent with candour and truth if he had also acknowledged that the whole scheme of the commission originated with me, and that it was conducted solely at my cost and legal risk. I may here remark that one of the obstacles to the execution of my design at a much earlier period arose from some apprehensions which I entertained, in consequence of the opinions expressed by my legal friends and advisers, that I might be involved in ruinous expenses if I published the names and addresses of the falsifiers of articles of food; but, having felt most strongly that in such publicity would consist the great utility of the undertaking, and that systematic works on adulterations unaccompanied by such announcements had been productive of immense mischief, instead of benefit, by showing how the frauds could be practised and the culprits left without a check or exposure, I considered that the good to be gained was worth the risk, and at length firmly resolved to appeal from the anger of the dishonest traders and undersellers to the men who conducted their business with fairness and integrity.

It will be noticed that in the paragraph quoted from Dr. Hassall's letter it is stated that in consequence of an application from me he had undertaken to furnish the articles; and then he adds:—

"In conformity with this engagement I supplied the reports for a period of four years. I employed my own agents in the purchase of the samples, accompanying them on all occasions myself, for greater security."

What is the inference from these statements? Apparently, there can be but one, viz., that Dr. Hassall was not only an unpaid labourer, but that he actually made the purchases at his own cost; for not a word

is said or implied that he was employed at a salary (a far higher salary, by the way, than was proposed by himself in the first instance), and that every cost and expense, from first to last, throughout the whole conduct of the commission, was sustained by me. Accordingly, in the Doctor's paid accounts I find charges for all the samples of goods purchased — chymicals, books, journeys by Mr. Miller for making purchases, drawings, apparatus, instruments, several sums for chymical analyses made by Dr. Letheby, &c. Besides these expenses, I sustained the law costs and incurred all the undefinable legal risks of the undertaking, as was my duty. In a word, I procured the services of Dr. Hassall at a higher price than he demanded, and I applied those services to the public benefit.* Is it just, then, that he should now endeavour to make it appear that he was in any respect concerned in originating the Analytical Sanatory Commission, or that any portion of the inquiry was conducted at his pecuniary cost? I readily and cheerfully acknowledge the scientific merits of Dr. Hassall, and that his reports afforded me the highest satisfaction; but by the course he has lately pursued he has placed himself in a false position, and it is one which scientific men must deeply regret.

With respect to the success of the Analytical Sanatory Commission, and the importance which the subject of it has now acquired, I attribute much of both to the favourable notices of the reports which so often appeared in the columns of the *Times*.

I remain, Sir, your obedient servant,

THOMAS WAKLEY.

84, King's Road, Brighton, July 28.

LETTER V.

DR. HASSALL AND MR. WAKLEY.

To the Editor of the "Times."

Sir,—The two letters contained in your impression of this day — one from Mr. Wakley and the other from Dr. Letheby — demand, in the interests of simple truth and justice, a few remarks from me by way of explanation.

The long and elaborate letter of Mr. Wakley leaves the matter essentially as I stated it on Friday, although it proceeds on certain erroneous assumptions.

Thus, I have never claimed to be the originator of the Analytical

* The sum received by Dr. Hassall was at first 100*l.* per annum, which was afterwards raised to 150*l.*, at the request of Dr. Hassall. One would suppose from Mr. Wakley's letter that he had paid Dr. Hassall at the rate of 1000*l.* per annum.

Sanatory Commission of the *Lancet*. Further, I have not one syllable to retract as regards my statement of the origin and history of that commission. My description is entirely consistent with the not very relevant fact that so far back as 1830 Dr. O'Shaughnessy wrote an article, but without names or addresses, on "Coloured Sugar Confectionary," which was published in the *Lancet* of the following year. In fact, you will find that article specially noticed in my report on the same subject. My account is also equally consistent with the fact that Mr. Henry was engaged by Mr. Wakley in 1836 to make certain analyses in relation to adulteration. It is right to state, however, that the results of these analyses were never made known.*

From 1837 to 1850 the subject, whatever might have been the thoughts or intentions of Mr. Wakley, remained dormant; nothing was done in it until I of myself, and wholly independently, prepared an article, first on coffee and its adulterations, and then on sugar, which was read to the Botanical Society of London, and which was favourably noticed by the press. It was this event which induced Mr. Wakley to communicate with me, and which led to all the subsequent arrangements and proceedings; what these were on both sides, I will, with your permission, describe as briefly and clearly as I possibly can.

Having assured Mr. Wakley, in reply to his own inquiries, that I thought I could so prepare a series of reports on the subject of the adulteration of food as would admit, without any great risk, of the publication of the names and addresses of the persons of whom the goods analysed were purchased, Mr. Wakley determined to publish such a series in the *Lancet* — these to contain the names and addresses of the vendors; he further devised the title under which they were to appear, and he agreed, of course, to pay the expenses of those inquiries. Now, Sir, I did not refer to the pecuniary part of the subject in my first letter further than was necessary, out of motives of delicacy, and because it seemed to me that it was not called for; but Mr. Wakley has now himself removed all reserve on this head. The annual expense of the commission, including my salary, the cost of the original drawings, of the wood engravings, of the purchases of the samples, and Dr. Letheby's fees†, amounted to exactly 245*l.* 5*s.* per annum. This estimate includes the whole cost of the commission, except printing and the law expenses connected with the single abandoned action. As a set-off to these expenses were to be put the increased sale of the journal and advertisements. My salary for part of the time was 100*l.* per annum, and afterwards 150*l.*

* Lastly, not a single number of the "Journal of Adulteration," which Mr. Brent states that he received instruction to prepare, ever appeared. It may then at least be said that in these repeated failures to bring out a series of reports on the subject of adulteration, Mr. Wakley was singularly unfortunate until he met with Dr. Hassall.

† That Dr. Hassall was only authorised to incur a limited expense for chemical assistance, may be inferred from the fact that for the four years during which the reports were published, the sum of 63*l.* 16*s.* was charged to Mr. Wakley for Dr. Letheby's fees. The amount charged Dr. Hassall by Dr. Letheby, for work for the *Lancet*, *exclusive of private analyses* was about 20*l.* more than this, and with which the *Lancet* was not charged.

Such is a statement of the facts relating to the Commission as regards Mr. Wakley. I will now describe them as they relate to myself.

I employed my own agents to purchase the examples. I accompanied them on all their journeys; I made the whole of the microscopical investigations, and the larger part of the chymical analyses; I wrote all the reports, with a single exception; and I afterwards published, in accordance with my written agreement, with my own name attached, the whole of the reports at my own cost. The republication of these involved an expense of 800*l.*, and I had to furnish my publishers with an undertaking to meet all actions which might arise out of the renewed publication of the names and addresses.

I do not, therefore, claim, nor have I ever claimed to be, the originator of the Sanatory Commission of the *Lancet*, although but for me there is every reason to believe that there never would have been any such commission. On the contrary, I have on all occasions given Mr. Wakley the fullest credit for that which is really his due. In proof of this, I would state that I submitted to Mr. Wakley a copy of the introduction to my work, in which the origin and history of the commission is given precisely as I have stated it, both in my former and present letters. With this Mr. Wakley expressed himself at the time as perfectly satisfied.

But what I do claim is, that I was the first to apply on a large scale the microscope to the detection of adulteration; that I was the chief labourer in the work of the Analytical Sanatory Commission; that I was the author of the reports; that, in connexion with these reports, I have incurred a large pecuniary risk; and that I staked my name and reputation upon the successful carrying out of a most arduous and responsible undertaking, and I am truly thankful to be enabled to look back upon the past and to think that I have so far accomplished the task which I set before myself. Had I been less careful or less conscientious I might readily have involved both Mr. Wakley and myself in ruin.

I will now proceed to comment very briefly upon the second letter.

In the titlepage of my work I style myself "chief analyst of the commission." I did so out of deference to Mr. Wakley, and that it might not be supposed that very nearly the whole labour was in the hands of one man; there really was, however, no other analyst connected formally with the commission. It is quite true, as is acknowledged in my published work, that Dr. Letheby made some of the chymical analyses, but made them as much for me as for the commission; that is, Dr. Letheby analyzed such articles only as I thought proper to send him. I was the judge of what to send and when to send.

This fact I have never sought to conceal in any way, and in proof of it I would ask to be allowed to quote the following passage from my work:

"We have to acknowledge the great assistance which we have derived at different times from our friend Dr. Letheby, to whom we have been in the habit of referring frequently on doubtful points. The chymical portions of the later reports contained in this volume, commencing with that on vinegar and its adulterations, have all been revised by Dr. Letheby. Our best thanks are therefore due, and most cheerfully accorded to that gentleman, for the kind and ready aid which he has at all times afforded us."—Page 40.

But the above is not the only acknowledgment of the services of Dr. Letheby; several other references occur in the body of the work, although his name is not attached to all the analyses made by him, because the reports having the form of a commission had to appear as the work of more than one person. It will be conceded as a somewhat remarkable fact, that although the proof sheets of many of the reports* went through Dr. Letheby's hands, and that up to within the last few days I was in the habit of occasional friendly correspondence with him, no word of discontent or dissatisfaction was ever expressed by him to me.

I will now proceed to show from the following statement what Dr. Letheby really did in the way of chymical analysis. Exclusive of an introduction of 40 pages, my work contains 647 pages of letterpress, and it embraces the reports which were published relating to food in the years 1851, 1852, 1853, and 1854. I find that 25 reports appeared in 1851—these embraced 1,054 analyses, microscopical and chymical, and they occupy 368 pages of the work; that 20 reports appeared in 1852, embracing 512 analyses, the last of these reports extending to page 514; that in 1853 seven reports were published, containing 340 analyses, and terminating with the 600th page of the work; lastly, that in 1854 three reports only appeared, containing 291 analyses. The first year Dr. Letheby made 16 analyses, the second 9, the third 59, and the fourth 12 analyses—that is, in all 96 as against 2,287—a large proportion of which analyses were both microscopical and chymical.

Besides the reports contained in my work four other reports were published in the *Lancet* on drugs, and two others were prepared on articles of food, but were not published; these contained 228 analyses, of which 57 were made by Dr. Letheby; of the analyses conducted by Dr. Letheby, the greater part were confined to two articles — viz., snuff and opium, there being 43 samples of the former and 57 of the latter.

These statistics are, I think, sufficient to show what amount of the work of the commission was performed by Dr. Letheby, and to what extent he has a right to participate in the credit. But of the analyses actually performed many were merely confirmatory, while it occasionally happened that the supplied results were inaccurate; this rendered the repetition of some of the analyses necessary, and but for the exercise of this precaution the commission would have been more than once placed in the greatest danger. I have now but little more to add. I would state, however, that *the chief value of my work on adulteration consists in the fact that in it is recorded for the first time the results of the extensive application of the microscope to the subject of adulteration; that it is, therefore, far more a microscopical than a chymical work, and that*

* The reports referred to are the revised reports for Dr. Hassall's work; the original reports Dr. Letheby never saw until after their publication in the *Lancet*.

had I had no share in the chymical portion of it, it would have made very little difference as to the actual amount of credit to which I might be entitled.

I am sorry that this letter should have extended to such a length, but in writing it I have been very desirous to place the facts before you in such a clear light that you might be enabled to judge of the real circumstances of the case. Sorry indeed should I be that you should consider that I had done anything to forfeit the favourable opinion which on a recent occasion you expressed in relation to my labours.

I have the honour to remain, your very obedient servant,

ARTHUR H. HASSALL, M.D.,
Member of the Royal College of Physicians.

Bennett Street, St. James's, July 30.

LETTER VI.

ADULTERATION OF FOOD.

To the Editor of the "Times," August 1st.

Sir, — I really am ashamed of the circumstance which calls for another letter from me on the subject of Dr. Hassall's inconsistencies. In his letter of the 27th instant, he distinctly stated that in the inquiries of the *Lancet* Commission, "no one was associated with him; but, during the latter part of the period, he was authorised to employ, to a certain extent only, chymical assistance when necessary, and, in the exercise of this discretion, he employed his friend Dr. Letheby, whose services he had much pleasure in acknowledging in his work;" from which it must be inferred that my assistance was not had, or needed, until the latter part of the inquiry. Now, however, that Mr. Wakley has exposed the true position of Dr. Hassall, and even stated that he paid "several sums for chymical analyses made by Dr. Letheby," Dr. Hassall admits that, during the first year I made 16 analyses for him, during the second 9, during the third 59, and during the fourth 12; besides making 43 analyses of snuff, and 57 of opium.*

On looking over my laboratory book, I find that during the four years alluded to I made 291 analyses for Dr. Hassall, of which 13 were for adulterations of chicory and cocoa, 20 for bread, flour, and salt, 11 for bitter beer and pale ale (these being all the analyses necessary for the report on this subject†), 7 for vinegar, 25 for tobacco, 45 for snuffs, 63 for opiums, 10 for confectionary, 4 for cod-liver oil, and about 20 for castor oil ‡; the rest were, for the most part, of things which did

* Here again Dr. Letheby attributes an admission to Dr. Hassall which he never made—the 16, 9, 59, and 12 analyses included, Dr. Hassall showed, all the analyses made by Dr. Letheby, except those of the opium, which are not contained in Dr. Hassall's work.

† This statement is not correct. The results of the examination of 47 samples are stated in the report on that subject.

‡ This enumeration, which, by reference to Dr. Hassall's last letter in the

not immediately concern the commission; they were of matters which he had received for examination in the way of business, and which he could not investigate. Others were for his review of Dr. Golding Bird, in the 12th volume of the *British and Foreign Medico-Chirurgical Review*, where they are published, with a mere apology in the way of acknowledgment; and others were for a paper of his which was read before the Royal Society, and printed in their transactions. Indeed, I think it but right to say that the whole of the chymistry of that paper is a *verbatim* copy of my notes to him on the subject; and when he sent the proofs to me for correction, I do assure you I was astonished at the boldness of his conduct, and I made such corrections in his statements as I thought just to myself. I have not seen the paper since it has been printed for the members, and am therefore ignorant of the way in which he used my corrections.

In addition to this I have nearly 100 letters of his in my possession, which show that, while my analyses were really all the important and difficult ones of the commission, I furnished him with specific directions for the conduct of all the others. I say, therefore, with Mr. Wakley, that the course which Dr. Hassall has lately pursued has placed him in a false position, and that it is one which scientific men must deeply regret.

One word more, and I shall have done. Dr. Hassall states that up to the last few days we have been in friendly communication, and that no word of dissatisfaction has escaped me. This is not strictly true; for when, at the onset of the testimonial affair, he wrote to me, and asked me to go to his house and have a private meeting with one or two of his friends respecting the getting up of a testimonial to him, I wrote a strong letter of censure to him, and not only declined to have anything to do with the business, but told him what would be the consequences if he persisted in such a foolish step.* My prophetic warning has been realized; his true position has been made clear, and his friends have dropped from him in disgust. Since then we have not been in what can be called friendly communication; for all our correspondence has been confined to matters of business. Dr. Hassall knows, as well as man can know, that I have had but one object in serving him; and that if his conduct had not been open to the censure which I have passed upon it, he would still have had all my assistance in helping him.

I apologise to you for the length of this letter, and promise not again to intrude on your valuable space.

I remain, Sir, yours obediently,

H. LETHEBY, M.B.

London Hospital, July 31.

Times, will be found to be inaccurate in many respects, gives only 205 analyses as having been made, at Dr. Hassall's request, for the Lancet; that is, 205 as against 2,481 analyses, which were to a large extent double analyses, being both microscopical and chymical.

Dr. Hassall's reply, drawn up at our request, to those parts of this letter which relate to the review of Dr. Golding Bird's "Urinary Deposits," and to the paper on Indigo, published in the "Transactions" of the Royal Society, will be found at p. 54. in the Appendix.

* This statement has already been refuted.

LETTER VII.

[Dr. Letheby, not satisfied with attacking Dr. Hassall in the *Times*, continues the correspondence in the *Medical Times* and *Gazette*, in the following letter, which is in many parts nearly verbatim a copy of his last letter to the *Times*. To this letter Dr. Hassall did not attempt to offer any reply, preferring to treat it with silence. Some of its statements are refuted in the appendix.]

THE HASSALL TESTIMONIAL.

To the Editor of the "Medical Times" and "Gazette."

Sir,—In the *Medical Times* of last week you allude to the proposed testimonial to Dr. Hassall as a premature affair, seeing that the services of other persons have been engaged in the same inquiry, and with equal success. I am quite sure that the correspondence which has been published in the *Times* during the last week will show that the testimonial is not only premature, but also, as I think, unmerited; that Mr. Wakley, Mr. Postgate, Mr. Miller, and myself have had no inconsiderable share in the work which Dr. Hassall claims as his own. In fact, as regards my own share in this investigation, it will be manifest, from an examination of the Commission Reports, that Chymistry has done more to expose the character of the frauds practised on the people than the microscope possibly could have done. The former has exhibited the kinds of adulteration which are mischievous to the health of the people; the latter merely those which are frauds on the pocket. What matters it, in a sanatory point of view, that the starches are substituted for each other, and that wheat-flour, or barley-meal, is the stock material for increasing the weight and bulk of substances; but when we come to find that the mineral acids, the filthy earths, and the poisonous metals are resorted to as a means of masking fraud, then, indeed, are we in a condition to show that it is high time to have a parliamentary inquiry into such a matter. To Mr. Wakley is due the honour of having initiated this inquiry; and I wish that I could say as much in favour of Dr. Hassall. A public opinion has, however, been formed on this subject; and, to judge from the leader in the *Times* of the 24th instant, it would appear as if Dr. Hassall is the only man who has the smallest claim to public praise. This, however, has been set right within the last few days; and now I will trespass on the attention of your readers for a short time, while I recount the many instances in which chymistry has been made the means of exposing the frauds in question. As early as the spring of 1851, Dr. Hassall put himself in communication with me on the subject of the chymistry of his inquiry. From that time I have been constantly referred to, and have made nearly 300 analyses, of one sort and another, for him, and have given written and verbal

instructions for all the chymical processes alluded to in his book. In one case, and only in one case, as far as I know, has he acknowledged the source of his information, and that is in the case of an elaborate inquiry into the composition and properties of chicory and coffee, where, at page 109., he modestly says, the report has been made conjointly by Dr. Letheby and ourselves.* He says nothing of my process for discovering alum in bread, iron in cocoa, vitriol in porter, oxide of lead, chromate of lead, and bichromate of potash in snuff; red lead and vermilion in cayenne, strychnia in beer, sulphuric acid (free and combined) in vinegar, poisonous pigments in confectionary, gelatine in isinglass, opium in cigars, and a multitude of other processes, where I have given him the fullest instructions for discovering the percentages of alcohol, acetic acid, gum, sugar, &c., in a number of substances.

All these are published in his book in the most extensive form without one single word of acknowledgment. It is true that in nine places, and in nine only, as far as I can find, he mentions my name—these are at pages 109. 265. 357. 365, 366. 596, 597. 621. and 643., where the acknowledgments are in connection with the subjects of chicory and coffee, cocoa-ash, bread, isinglass, snuff, confectionary, and gin; but the most liberal interpretation would never lead to the conclusion that I had any important connection with the inquiry. Many of the analyses which I made for Dr. Hassall were of things which had no immediate connexion with the commission. For example: I made about twenty-five analyses of urinary deposits, several of which were published in his review of Dr. Golding Bird, in the 12th volume of the *British and Foreign Medico-Chirurgical Review*, and others were copied into a paper which was read before the members of the Royal Society, and published in their *Transactions;* in fact, I think it but right to say, that when I received a proof of that paper for correction, I was so astonished at the manner in which Dr. Hassall had transcribed my note, without other than the merest shadow of acknowledgment, that I thought fit to show, from the character of my corrections, that all the chymistry of that paper was mine. Whether those corrections appeared as I wrote them I have not since had an opportunity of knowing.†

Dr. Hassall will no doubt say, Why not have told me all this before? I have already given him an answer to this question; and now I say that I did not at the time attach any value to such paltry sacrifices; I thought I was doing him a service, and that was enough for me; but when he writes to me and asks me to have a private meeting of a few friends at his house, to talk over the subject of getting up a testimonial to him, I then become annoyed, and see the mischief which my liberality has occasioned. I wrote to him, therefore, in the severest terms, telling him of the impropriety of the step he had taken, and the consequences which would undoubtedly follow. My warning he disregarded, and now his friends are falling from him in disgust. Since that time our correspondence has only been of a business character; and even now I would not have uttered a single word of reproach if it had not been, as

* The facts in reference to this report will be found described at p. 54. in the Appendix.

† See Appendix, p. 56.

Mr. Wakley truly says, that the course which Dr. Hassall has lately pursued has placed him in a false position, and that it is one which scientific men must deeply regret.

I am, &c.

HENRY LETHEBY, M.B.

London Hospital College, August 1st.

LETTER VIII.

[It is difficult to explain why this letter was not inserted by the *Times*.]

To the Editor of the "Times."

Sir,—I am glad to perceive by Dr. Letheby's own admissions contained in his letter of to-day, that his pretensions are now reduced to somewhat nearer their true proportions, although the account last rendered by Dr. Letheby — namely, that he had performed 205 analyses for the "*Lancet*" — is very wide indeed of the actual facts.

This will appear from the conclusive statement* which accompanies this, and from which I extract the two following paragraphs:—

"Of these samples, 63 were of tobacco and snuff, and 53 of opium, thus leaving 49 samples of all other kinds; but it is to be especially noted that the analyses of these 53 samples of opium are not recorded in Dr. Hassall's work on Food and its Adulterations.

"The accounts rendered by Dr. Letheby being so clear and in his own hand writing, furnish indisputable and conclusive evidence of the actual number of analyses performed by him, some of them being partial analyses only.

Thus it is distinctly shown that the entire number of chemical analyses performed by Dr. Letheby, viz., 165, bears but a very small proportion to the number of the analyses, as well microscopical and chemical, performed by Dr. Hassall, viz., 2,585.

I wonder it never struck Dr. Letheby that he had himself furnished me at different times with the means of his own refutation in the shape of accounts in his own handwriting. After the above-mentioned statement, I need not stop to point out where or in what particulars the figures quoted by Dr. Letheby are inaccurate, further than to state that it is evident, on his own showing, that he has mixed up with the account of the work performed through me for the *Lancet*, analyses made for myself, extending over a series of years, on my own private account, and having nothing whatever to do with the *Lancet*. In such a proceeding, I need make no other comment, except to regret that he has been so ill advised.

* Statement of the Rev. R. S. Daniell and Mr. Bolton, drawn up from an examination of Dr. Letheby's accounts, printed in Appendix, p. 31.

The other statements made by Dr. Letheby in his letter are as inaccurate as his figures. I enclose an attested copy of the letter (not for publication) referred to by him, dated January 27. 1855, from which you will perceive that not one syllable is uttered by him of complaint against me for ought I had done up to that time. But I hold in my hand a communication, dated 8th May, 1855, a similar copy of which I send, written in the most friendly way and ending—"I hope that you and Mrs. Hassall are quite well; and, with kind regards, believe me to be yours truly," &c., &c.; thus showing that even to that time* I could not have committed any very serious offence against Dr. Letheby.

Really the want of accuracy displayed in this letter is lamentable. I have now nearly done. Looking back at this correspondence, into which I have been most unwillingly led in my own defence, I have to remark that I have nothing to retract from any of the statements which I have advanced, and nothing in my own conduct in this affair to regret. I now beg to thank you for the perfect justice and impartiality which you have displayed towards all parties in this controversy, and conclude with the old but telling aphorism, "*Magna est Veritas et prævalebit.*"

I have once more the honour to ascribe myself,

Your most obedient servant,

ARTHUR H. HASSALL.

Member of the Royal College of Surgeons.

Bennett Street, St. James Street, 2nd August.

"The Home Companion."

The number of the above periodical for October 1st, 1855, contains the first of a series of articles, by Dr. Letheby, on the subject of the Adulteration of Food. This article is thus headed—

"Food and its Adulteration, by Dr. Letheby;"

and in it the following passage occurs—speaking of the *Lancet* Sanatory Commission, Dr. Letheby remarks, "That Commission was composed of three persons—the present writer (himself), as Analytical Chemist, Dr. Hassall, the Microscopist, and Mr. Miller, the Artist.

Thus, Dr. Letheby adopts the very title of Dr. Hassall's work, places his own name after it as though he were the author, assigns to himself the first place in the Commission, makes Mr. Miller another of the Commission, and then uses the matter of Dr. Hassall's work as though

* That is some months after the publication of the last report which appeared in the *Lancet* on the adulteration of food and of Dr. Hassall's work.

The style of the following quotation, not from a private letter, but from a testimonial from Dr. Letheby, intended for public use, forms rather an instructive and amusing contrast to Dr. Letheby's letters to the *Times*: "I have had the pleasure of being acquainted with you for many years, and I am quite sure that your extensive scientific attainments, your genial disposition, and general kindness of heart, not only qualify you in an eminent degree for the appointment you seek, but likewise render you a very acceptable colleague."

it were his own.* After the charges which Dr. Letheby has made against Dr. Hassall, one would have supposed that he would have scrupulously abstained from any course which would have exposed him to the very imputation he has made against others. The composition which he gives to the *Lancet* Commission is entirely arbitrary, and in direct opposition to all the facts of the case. What the relative positions in the matter of Dr. Hassall, Dr. Letheby, and Miller were, has already been explained. There was, in fact, no Commission whatever formed; but there was a Commissioner, he being Dr. Hassall himself. Dr. Letheby was employed by Dr. Hassall to render occasional chymical assistance, and Mr. Miller was Dr. Hassall's own artist, employed for many years in his service, and educated to his work by Dr. Hassall himself. With reference to this part of the subject, the reader is referred to page 22., and also to the following letter: —

Bayswater, Nov. 14th, 1855.

Sir,—As you are aware, Mr. Henry Miller resided with me some years, until within a few days of his death; therefore I have had full opportunity of becoming acquainted with the nature of his duties with you.

I know that his chief occupation with you was that of Microscopic Artist and Draughtsman, and that his qualifications for that office were principally acquired in your service, he working in your house and under your immediate directions.

The idea of his being one of the Sanatory Commission of the *Lancet* is simply ridiculous, and were he alive, he would, I am sure, be the first to repudiate any such statement.

I can also speak from my own personal knowledge of the great kindness and consideration which you at all times had for him, and that often under very trying circumstances.

I am, Sir, yours obediently,
W. Butcher.

To Dr. A. H. Hassall.

Note by Dr. Hassall.

ON INDIGO IN HUMAN URINE, &c.

In Dr. Letheby's last letter to the *Times* these remarks occur (they are reprinted, with a slight alteration in the words used, in his letter to the *Medical Times and Gazette*):—

"Others (analyses) were for his Review of Dr. Golding Bird, in the 12th volume of the *Medico-Chirurgical Review*, where they are published, with a mere apology in the way of acknowledgment; and others were for a paper of his which was read before the Royal Society and printed in their *Transactions*. Indeed, I think it right to say that the

* The material of this Paper is derived almost entirely from Dr. Hassall's work and Johnstone's *Chemistry of Common Life*.

whole of the chymistry of that paper is a verbatim copy of my notes to him on the subject; and when he sent the proofs to me for correction, I do assure you I was astonished at the boldness of his conduct, and I made such corrections in his statements as I thought just to myself. I have not seen the paper since it was printed for the members, and am therefore ignorant of the way in which he used my corrections."

On turning to my Review of Dr. Golding Bird's *Urinary Deposits*, I find that Dr. Letheby made analyses of five samples of urates, four of these being sent to him by myself, I pointing out to him that the great object of the analyses was to ascertain whether they consisted of urate of ammonia or not. Besides these five samples, he also analysed a specimen of artificial vichy water which I sent him for the purpose.

With reference to the analyses of the urates the following acknowledgment occurs: —

"It should be stated, that in the above analyses of the urates, *we have received great and valuable assistance from Dr. Letheby;*" again, it is specially stated that the analysis of the vichy water is by "Dr. Letheby."

This, then, is what Dr. Letheby calls "a mere apology in the way of acknowledgment," and yet he was employed, not as a friend in the matter, but in his professional capacity as a chymist.

The circumstances with regard to my paper on the frequent presence of indigo in human urine, published in the *Transactions* of the Royal Society, are briefly these: —

This paper showed two things: first, that the occurrence of a blue pigmentary substance in the urine was frequent, and second, that that substance was indigo.

With the investigations by which the frequency of the presence of the blue pigment was established, Dr. Letheby had nothing to do. Again, nearly the whole value of my paper rested on the fact, that the substance discovered was indigo,—and indeed the whole chymistry of the paper consisted in proving that it was indigo.

On meeting with this blue pigment for the first time, and on subjecting it to analysis, I found that it was neither cyanourine nor uroglaucin, and that it agreed in many of its characters with indigo. Wishing to obtain all the evidence on this point I possibly could, I forwarded specimens of the blue pigment, and of the urine which furnished it, to Dr. Letheby for his analysis. After the lapse of many months, I received from him, not only the results of his examination, but a report on the subject, in which he refers to the writings of Scherer and Heller, quoted the analyses of those chemists and also of Mulder and Crum. Now this was what I did not require from Dr. Letheby; I was quite as well able as himself to have consulted the authorities referred to, and of course, in doing so in my paper, there was no reason why I should quote Dr. Letheby in connection therewith.

That I was tolerably well acquainted with the nature of the blue pigment in question will appear from the following acknowledgment by Dr. Letheby, "I have just examined the blue deposit, and without doubt it is, as you say, indigo." Further, that I was not backward in recognising Dr. Letheby's connexion with these inquiries will be evident, when I state that *his name occurs in my paper in four different places;*

once thus, "if so, the relation between hœmatin and indigo is still more remarkable, for, as suggested by my friend Dr. Letheby, to whom *I am much indebted for the aid afforded in these analyses*," &c. This Dr. Letheby calls "the merest shadow of acknowledgment."

Dr. Letheby states that "the whole of the chymistry" of my paper is "a verbatim copy of my notes to him on the subject." This I utterly deny. In describing the process of analysis pursued in order to determine the nature of the blue pigment—that is, in describing the tests for indigo, I do so very nearly in Dr. Letheby's own words, and this, not because there was any particular merit in them, for surely the description of the tests for indigo is a simple matter enough, but because I expressly state the analyses were made by "Dr. Letheby and myself," and I preferred to give the method of examination pursued by him.

Dr. Letheby refers to the fact that I forwarded the proofs of my paper to him for correction. My having done so furnishes a tolerably clear proof that I had nothing to conceal in the matter. But now a word or two as to the alterations which Dr. Letheby states he made. These consisted merely in the introduction of his own name in two different places. Once it was artfully introduced in the very first paragraph of the paper, in such a way as to give him the credit of having been the discoverer of the presence of indigo in the urine. I unhesitatingly drew my pen through this correction; the other was allowed to remain.

After my paper had been read to the Royal Society, the referees expressed a wish that I should demonstrate to them the reactions by which I came to the conclusion that the substance in question was indigo, and required particularly to be shown its conversion into aniline. I wrote to him on the subject, when he replied, that he had some of the pigment left with which I had furnished him some time previously; that he had succeeded in converting a portion of this into aniline, and that he had some of the material still left. I then asked him to meet the referees with me, and to bring the pigment with him, cautioning him to be sure of the grounds upon which he proceeded. Dr. Hoffman, who knows more about the conversion of indigo into aniline than any other chymist, proceeded to operate on the material brought, and failed in obtaining the smallest evidence of the formation of aniline. This failure placed my paper in great jeopardy. Dr. Letheby could not explain the reason of his own success nor of Dr. Hoffman's failure, both operating on the same material, nor did he manifest the smallest concern, although I of course felt deeply mortified. A fresh appointment was made. I procured more of the colouring matter, dispensed with the presence of Dr. Letheby, and this time succeeded completely; and the result was that my paper was published.

But Dr. Letheby likewise complains of my conduct to him with reference to a conjoint report on chicory. The facts of this case are very briefly as follow:—

A gentleman called upon me and expressed a wish to be furnished with a short report on chicory, contrasting this root with coffee. I stated that I should have no objection to prepare such a report, but I advised that it should have a second name attached to it. Accordingly I communicated with Dr. Letheby on the subject, forwarded samples of the raw and roasted chicory root to him for analysis, wrote the report, and sent it to him for his signature. Dr. Letheby made a few alterations in the report, wrote it out afresh, which there was no necessity whatever for doing, and returned it signed. For this report he was paid the sum which he himself demanded. It was never published in the *Lancet*, but was printed in my work on "Food," where it is described as the joint production of Dr. Letheby and myself. Now, I put it to any impartial man to say whether more than this could be reasonably expected.

I have now furnished evidence enough to show that it is often a very dangerous thing in scientific matters to consult a "friend," even although he is paid and his services are publicly acknowledged. Enough has also been adduced in the pages of this Pamphlet to prove that, when a man writes to a "friend" on scientific matters, he ought to be exceedingly guarded in the wording of his letters, and that he always ought to endorse them, "private and confidential," lest, should he neglect these precautions, they should some day or other turn up against him in the columns of the *Times*, as in the present case. Taking the whole circumstances of Dr. Letheby's conduct into consideration, it is clear that but rarely has a more unprovoked or unscrupulous attack been made than that to which I have been subjected at his hands. In publishing in the columns of the *Times* detached portions of private letters, letters written hurriedly from my laboratory amidst a press of work without due reflection, and this in order to attempt to affix upon me a charge of want of chymical knowledge and also in revealing transactions of a private and professional character, Dr. Letheby has committed a violation of that confidence which ordinarily regulates the intercourse between professional men and gentlemen. Such has been the nature of the conduct pursued, that it would I believe justify the use of very strong language indeed. This, however, I shall not employ, and I now leave the facts to the judgment of the medical profession and the public, who will doubtless form their own opinion of that conduct.

In evidence given before the Committee of the House of Commons on the Adulteration of Food in reply to the second question by the chairman, Dr. Letheby makes the following statement:—"For nineteen years before that time I was engaged by the late Dr. Pereira in making investigations for his great work on *Materia Medica*."

This statement, which would make it appear that Dr. Letheby had a considerable share in the production of Dr. Pereira's work, naturally led me to refer to the work itself in order to see what kind of acknowledgment was made by Dr. Pereira for services extending over such a long series of years. To my surprise I find that Dr. Letheby is not alluded to in the most distant manner, either in the preface to Dr. Pereira's work or in the body of the work, as having rendered any assistance whatever.

In what way are we to reconcile the statement of Dr. Letheby and the absence of any mention of his name by Dr. Pereira?

ARTHUR H. HASSALL.

MEETING AT FREEMASONS' HALL.

The Analytical Sanatory Commission.

At a meeting held at Freemason's Hall, on the 8th day of August, 1855, to take into consideration the correspondence recently published in the *Times* in reference to the above subject,

The Rev. B. C. Sangar, M.A., in the chair,

It was unanimously resolved, on the motion of the Rev. J. Compton, M.A., seconded by George Bolton, Esq.—

"That this meeting, having had placed before it a full report of the whole case, having heard read the whole correspondence, and having examined certain original accounts, letters, and documents, and compared them with that correspondence, is fully satisfied of the perfect accuracy of Dr. Hassall in the statements contained in his published letters, and considers that the statements and allegations calculated to detract from the high merit of his distinguished and scientific labours, have been most satisfactorily refuted by him."

It was also unanimously resolved, on the motion of Dr. Seaton, seconded by John Ellis, Esq.—

"That this meeting views with great regret the very unworthy and disingenuous attempt recently made to detract from the merits of Dr. Hassall in connexion with the subject of the adulteration of food, beverages, and drugs; to question, and even disparage, his enormous labours; and to ungenerously and unkindly deprive him of his well-merited reward. It expresses its strongest condemnation of the course pursued, which it can scarcely consider to have originated in worthy motives. It considers this attempt should operate as but a stimulus to themselves to increased exertions in behalf of the proposed testimonial to Dr. Hassall, which is now more than ever necessary, and to maintain and support by every means in their power the high character and reputation and renowned scientific eminence he has so meritoriously established for himself."

John A. Power, L.M., M.A., Cantab.
Raymond S. Daniell, M.A., Oxon. } Hon. Secs.

Freemasons' Hall, Great Queen Street,
August 8. 1855.

These Resolutions were refused insertion in the *Lancet* even as an advertisement.

FROM THE "TIMES" OF AUGUST 5TH, 1850.

It was in consequence of the following notice that Mr. Wakley communicated with Dr. Hassall, and it was this which led to the establishment of the *Lancet* Commission.

"*Adulteration of Coffee.*—At a meeting of the Botanical Society of London, held at the Society's rooms, 20. Bedford Street, Covent Garden, last Friday evening, Mr. J. Reynolds in the chair, a paper was read by Dr. Arthur Hassall on the Adulteration of Coffee. The author commenced by observing that the investigations he was about to detail originated in a remark made in the House of Commons during the late debate on chicory, to the effect that no means had yet been discovered by which the adulteration of coffee with chicory could be determined. The recollection of the fact that in vegetable charcoal the component parts of the several tissues may be detected by the microscope, led Dr. Hassall to infer that by the same means the less completely charred vessels, cells, &c., forming the tissues of those substances employed in the adulteration of coffee might likewise be discovered—an expectation fully realised. In this way it was ascertained that the substances most frequently used in the adulteration of coffee were chicory, roasted wheat, colouring matter, and occasionally beans and potato flour. The structure of the coffee berry and of the several productions named was then minutely described; and it was shown that chicory might at all times be distinguished with the greatest ease by the size and ready separation of the cells, as well as by the presence of bundles of vessels, of the dotted or interrupted spiral kind. The substance so generally employed to deepen the colour of chicory Dr. Hassall found to consist, in those instances in which he had examined it, of burnt sugar; and he referred to the fact that the rich brown hue of coffee is not peculiar to a decoction of that berry, but that almost all vegetable substances when charred, yield a somewhat similar colour. The author then proceeded to detail in a tabular form the results of 34 examinations of coffee of all prices.

From these it appeared that the whole of the coffees, with two exceptions only, were adulterated; that chicory was present in 31 instances, roasted wheat in 12, colouring matter in 22, beans and potato-flour in 1 only; that in 10 cases the adulteration consisted of but a single article, in 12 of two, and in 10 of three substances; that in many instances the quantity of coffee present was very small, and in others not more than a fifth, fourth, third, half, and so on. Contrasting coffee and chicory, it was observed, that, while the coffee-berry contains a quantity of essential oil visible in small drops in the cells, and on which its fragrance and active properties depend mainly, not a trace of any such oil is to be found in the chicory-root. The properties of coffee are those of a stimulant and tonic, with an agreeable flavour and delicious smell, in all which respects chicory is very greatly inferior. The adulteration of coffee with wheat, bean, and potato-flour, Dr. Hassall considered altogether indefensible, since those substances have not one of the properties of coffee belonging to them; and observed that, if the employ-

ment of chicory was deemed in any respect desirable, it should be sold openly, and not, as at present, surreptitiously, and under the names of Ceylon, Berbice, Costa-Rica, Mocha coffee, &c. The paper contained many other interesting details, the mention of which would extend this notice to too great a length, and was brought to a conclusion with one or two hints addressed to coffee-drinkers—viz., that the coffee should be ground fine, in order to facilitate the liberation of the essential oil contained in the cells of the berry, and that an infusion, and not a decoction, of it should be made."

THE END.

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