

LAW REPORT.

Manchester.—Appeal against a Conviction for selling milk adulterated with 4 per cent. water.—Reversal of Judgment.

SPECIAL REPORT TAKEN BY REQUEST OF THE CORPORATION OF MANCHESTER AND THE PROPRIETORS OF THE ANALYST.

At the Sessions Court, Minshull Street, Manchester, on the 6th of October, 1883, before H. Wyndham West, Esq., Recorder, the case of Wardle *v.* Edwards was heard. We reprint the following verbatim report from the shorthand notes of Messrs. Snell & Son, 36, Chancery Lane, London, W.C., and 64, Fountain Street, Manchester. Mr. Gully, Q.C., appeared for the respondents, the Mayor and Corporation of Manchester; Mr. Cottingham, for the Appellant the previous defendant Wardle; and Mr. Sutton, for the Justices.

Mr. Gully: This, Sir, is an appeal by Richard Wardle, who is a farmer in Derbyshire, against a conviction obtained against him at Petty Sessions in Manchester for selling adulterated milk. The Respondents are Mr. John Edwards, who is an Inspector in the employment of the Corporation, and the Justices convicting, Mr. Lister and Mr. Furness. My friends Mr. Cottingham and Mr. Ferguson appear for the appellant, and Mr. Hopkinson and I appear for the Corporation, and Mr. Sutton only represents the Magistrates. The notices have been properly given to us, and I do not put my friend to any trouble upon that; and as the burthen lies upon me to prove the case over again in this Court I will state shortly what the circumstances are. I believe that the real question in dispute between us is one upon the merits. I do not say that it is not open to my friend to raise any point he can, but the substantial one between us no doubt is whether the milk was adulterated or not. That at first sight seems rather a curious point to come upon appeal before you after it has been decided in the Court below; and in this particular case it does raise considerations of some general importance. It seems that on the 23rd April last Mr. Wardle, the Appellant, sent into Manchester a consignment of milk in several cans to a milk salesman named Halewood. Mr. Halewood had, it seems, been dealing with Mr. Wardle since October, 1882, and he had had on previous occasions to complain of the quality of the milk which had been sent to him, notably I think in January of this year; and he had, shortly before the 23rd April, complained to the Inspector that he was getting milk which he believed to be adulterated. The consequence was the Inspector went up with him to the railway station; and, when the milk came in, he went up in the manner required by the Act of Parliament and obtained from the consignment certain samples of the milk. Two samples were taken by him which have been numbered respectively 203 and 204, one from one churn and the other from another churn. The usual formalities required by the statute were complied with by the Inspector—that is to say, he took each sample and divided it into two parts, one of which he sealed up and kept, the other one he handed over in proper form to the Public Analyst of Manchester, Mr. Estcourt, to be analysed. I mean rather to say that he divided that, and gave half of it to Mr. Estcourt and kept the other half himself. The half which was taken on the first division—that is, the half of the whole of each sample—was handed over to the appellant for him to deal with as he thought fit. I think you will find that he had an analysis made of that milk himself; and I shall probably think it right to call before you on their subpoenas the two chemists who did make the analysis at the request of the defendant himself of the sample which he furnished to them. I think it will be found that they bear out the view taken by the Respondents on this appeal—that this milk was adulterated. The milk was analysed, and I had better read the two analyses which will explain themselves; and perhaps, if the originals are in Court, they might be handed up to the Recorder now. They are addressed to Mr. Rook, the Inspector.

"I, the undersigned, Public Analyst for the City of Manchester, do hereby certify that I received on the 24th April, 1883, from Inspector Edwards a sample of milk marked 203 for analysis (which then weighed —) and have analysed the same, and declare the result of my analysis to be as follows;—I am of opinion that the said sample contained the percentages of foreign ingredients following, namely, 4 per cent. of added water. No change had taken place in the constitution of the sample which would interfere in any way with the analysis." That is a clause which under the statute is required to be put into the certificate; and I think you will find that it is a very material clause to be put in with reference to this particular case. "As witness my hand, 25th April, 1883."

Then the other certificate is in precisely similar form I think—4 per cent. of added water. That is the certificate which was handed in; and it would be convenient that I should allude at the same time to what the actual result of the analysis was. Probably you, Sir, will know more of the chemistry of milk than I do, and it would be certainly be difficult to know less. It will be necessary that I should call your attention to what is material in this matter as far as I understand it. It seems that the main constituent of milk is water itself; and that therefore you cannot test whether milk has been adulterated by water simply by trying whether you can discover water in it, because something like 88 per cent. of milk is water. The residue consists of solids which are divided into two different descriptions of solids: solids which are not fat, and solids which are fat. Without going into the details of the matter, which I am afraid I could not describe very clearly, it is sufficient to say that the way of testing it is this: First of all you evaporate the water; and then, by a process which will be described, you get rid of the fat, and so ascertain what the residue is of solids which are not fat; and the question of whether the milk has been adulterated or not is settled by ascertaining whether the due proportion of solids which are not fat exist in the sample. If the due proportion does not exist it shows that the milk in its original state of purity has been tampered with, that is that some of it has been taken away, and in lieu thereof water—to take

the case of adulteration with water—has been put in. Water of course would not contain any of these solid matters; therefore there being mixed with the milk a certain quantity of water not containing these solid matters it would diminish the percentage of solid matters over the whole body. What they want to get at is whether or not the proper proportion of solids which are not fat is present; and that is not an absolute constant but a nearly constant quantity in milk. If that proper proportion does not exist in the milk there is an excess of water and it shows that there has been adulteration by water. The result obtained in this case was this as regards sample 203—Mr. Estcourt found there were 8·67 parts of solids not fat, and 2·54 of fat, the rest of the 100 parts consisting of water which was evaporated away. The total of solids was 11·21 and that would leave for water 88·79. Then sample 204 was a sample taken from a different churn. In that Mr. Estcourt found 8·62 of solids not fat, and 2·81 of fat, making total solids 11·43, and leaving of water 88·57. Those were the figures which Mr. Estcourt in his own laboratory ascertained as the analyses of these two samples of milk; and it was upon those figures that he made his certificate that 4 per cent. of water had been added. I may say at once that the basis upon which Mr. Estcourt made that report, and came to that conclusion that there was an addition of water, was that in his opinion there should have been at least 9 per cent. of solids which are not fat. That I think will be found to be not only Mr. Estcourt's opinion, but the opinion universally acted upon by Public Analysts in this country. The abstraction of anything from that figure of 9 indicates the substitution of some other matter, in this case water, and that therefore there had been an adulteration to the extent indicated by the difference between 9 and 8·6, and the amount of that difference in solids indicates by a process of arithmetic about which probably there would be no dispute, an adulteration to the extent of 4 per cent. of water. That was the mode in which the certificate was arrived at. I should tell you the course which matters took in this case shortly, so far as it is material. The case came on for hearing and some evidence of the usual kind was given. Mr. Estcourt's certificate was put in, and that was all that was necessary, according to the Act of Parliament. No doubt Mr. Estcourt was in Court, but whether he was called to give evidence or not I do not know. At any rate his certificate was put in which is sufficient evidence under the Statute until disproved. Upon the other hand the appellant, Mr. Wardle was called, and one of his men, who denied that any water had in fact been put in. Then Mr. Wardle applied to the magistrates as he was entitled to do under sec. 22 of the Sale of Food and Drugs' Act, 1875, to have the sample analysed by the authorities at Somerset House. The appellant having required the justices to have an analysis made by the Commissioners of Inland Revenue, at the hearing, which I think was on the 9th of May, the justices made an order to that effect; and a part of the original sample was sent up to the chemical officers at Somerset House to make an analysis, and that analysis was made on the 16th May I believe. At any rate it is dated the 22nd; and although this certificate is in itself no part of my case, I propose to read it because I think it is only fair to the other side that it should be read; and no doubt you will hear more of it in the course of the case, because it is necessary that I should call some evidence in respect of the processes adopted to shew the relative value of the certificate and analysis made by Mr. Estcourt and that made by Dr. Bell in London. Dr. Bell is a gentleman of great eminence in his profession as an analytical chemist and for reasons which will appear in the course of the case, I shall venture to impugn his certificate, which is as follows:—"Somerset House. The sample of milk referred to in the annexed letter marked 203 was received here on the 10th inst. The bottle was securely sealed. We hereby certify that we have analysed the milk and declare the results of our analysis to be as follows:—Non-fatty solids 8·20 per cent.; fat 2·80; water 89·00; ash 81 per cent." The latter I understand would be included in the non-fatty solids. "After making an addition for natural loss arising from the decomposition of the milk through keeping,"—that is a most material precaution—"the proportion of non-fatty solids is not lower than is found in genuine milks. The percentages of fat and ash are equal to those found in genuine milks. From a consideration of these results we are unable to affirm that water has been added to the milk. As witness our hands this 22nd May, 1883." This is signed by Dr. Bell and two of his assistants, Mr. Bannister and Mr. Lewin. The next is in form the same, but I will read the details of the analysis. "Non-fatty solids 8·02, fat 3·01, water 88·97, total 100. Ash 75." In the same way he says there, "After making an addition for natural loss arising from decomposition of the milk through keeping, the proportion of non-fatty solids is not lower than is found in genuine milks." As I said before, in reading the previous certificate, that is a very material point. Before I say anything more about the certificates I will shortly state what the result was. Dr. Bell explained his certificate and his process, and his reasons for arriving at the certificate before the magistrates below, as also did Mr. Bannister and Mr. Lewin. Mr. Estcourt was tendered for cross-examination, his certificate being put

in; but the appellant not desiring to cross-examine him he was not put into the witness-box. Other chemists were called to criticise the analysis of Dr. Bell, and in support of the analysis of Mr. Estcourt, and the magistrates in the end came to the conclusion that inasmuch as there were chemists who positively testified that they had analysed the milk while it was fresh and found it adulterated, they could not disregard that or disbelieve it upon an analysis made a considerable time subsequently when the milk was in a state of decomposition by Dr. Bell, especially as Dr. Bell was unable to affirm that the milk had not been adulterated; and taking that view the magistrates then convicted the Defendant. That is the history of the case. Now I will just say a few words upon the discrepancies which do exist, merely adding this before I deal with that question: that parts of the original sample taken by the officer, Mr. Edwards, and handed back by him to Mr. Wardle the appellant were handed by Mr. Wardle to two other chemists, who are also Public Analysts, I think—Mr. Wilkinson and Mr. Hehner. They analysed it before decomposition had set in; and they, not knowing what Mr. Estcourt's results were, and being in fact employed by Mr. Wardle for the purpose of analysing this milk, came to a conclusion entirely confirming Mr. Estcourt and differing from Dr. Bell. The appellant was cross-examined about Mr. Wilkinson's analysis in the Court below, and he admitted that much. I shall be in a position to call before you both Mr. Wilkinson and Mr. Hehner to show what I have just stated. There is a discrepancy as you will observe between these analyses—I will leave out for the moment the question of fat and the total solids, and confine myself to the material point, the solids which are not fat. As regards the sample 203, there is a difference between Dr. Bell and Mr. Estcourt. Dr. Bell, before he made any addition or calculation with respect to the effect of decomposition or any allowance therefor, says that he found in fact in the sample which was before him in May, 21 days or more after the milk had been seized, 8·20 per cent of solids not fat. Mr. Estcourt says he found 8·67. Now assuming that the processes were precisely the same, and assuming that there was no allowance to be made in respect of the decomposition of the milk having taken place, that of course would do more than confirm Mr. Estcourt's view; and it would be difficult to understand how Dr. Bell could say that that did not indicate adulteration. Dr. Bell's own evidence is that by his own process he found solids not fat only to the extent of 8·20; and I think from what Dr. Bell said in the Court below that he would be prepared to admit that that must have been adulterated. Then Dr. Bell gets over that difficulty—I am only using the phrase as meaning a scientific explanation—he explains the difficulty in this way: he says he was experimenting upon a sample that was decomposed, and that the effect of decomposition is to get rid of a certain quantity of solid matters which are in this fluid milk when it is fresh; and therefore in order to calculate what the milk was when it was fresh, or to borrow a phrase which I understand chemists sometimes use, in order to build up the fresh milk again you have to add something which your experience or your science teaches you is the proper thing to add in order to turn it into fresh milk again. Dr. Bell says I have done that; I have added something to the 8·20 in order to bring it up to what was the true fresh milk at the time when it really was fresh, that is, in order to bring up that milk which I am now analysing in a decomposed state to the point at which it was when fresh, I add .4 that is to say 4/10ths per cent. for that loss in 21 days and by that means bring it up to 8·60. Now the process which Dr. Bell uses is a process somewhat different from that used by all these gentlemen who are Public Analysts, and by the Society of Public Analysts, which treats these questions as questions of very great importance; and by Dr. Bell's process I believe 8·67 corresponds as nearly as we can put it to 9·00 of solids not fat by the process used by the Public Analysts. Therefore, if as I understand 8·67 or 8·60 by Dr. Bell's process was the result as regards solids not fat, that would represent substantially 9·00 of solids not fat according to the process of the Public Analysts; and if the milk did contain 9·00 per cent. of solids not fat Mr. Estcourt would not condemn the milk. But let me go back for a moment. In order to get at that 8·60 and to bring the milk into an innocent condition Dr. Bell has added a figure for which we say there is no scientific basis whatever—he has added to the 8·20 4/10ths per cent. of solid matter; and he says that he puts in that as representing the loss. It is not a thing that he actually finds in the milk at all. He says that he does not find that solid matter and that he has no evidence of it in the milk at all; it is merely a calculation; it is really a guess at what the solid matter was in that milk. I will tell you why I use the word "guess." I use it advisedly. There is undoubtedly a loss from decomposition which takes place. At what particular moment that decomposition will set in in particular milks, or how long it has been going on in a particular sample at the expiration of 21 days it is impossible to tell. In some cases it sets in much earlier than in other cases; in some cases it sets in

very rapidly, in others more slowly ; and it is impossible to tell with any accuracy what the amount of decomposition is. I shall show you by evidence of very experienced gentlemen, which I think you will attach considerable importance to, that they have tested milk which was in a state of decomposition and compared the analysis of that with the analysis of a part of the same sample taken when fresh, and they have found that no loss has taken place ; while at other times with other samples they found that there had been a considerable loss. What does that lead to ? It leads to the result that an analysis of milk in a state of decomposition is utterly unreliable. It may or may not be correct ; but it is utterly unreliable ; and according to a number of Public Analysts, who I shall call before you upon this short point, they will tell you that they would never think of testing, or judging by an analysis made of a sample of milk after decomposition had set in, an analysis of the same milk made by a competent person when the milk was fresh. You may add a figure, it may possibly be the right figure ; but you have no means of ascertaining whether it is the right figure. Even if you could assume that there was an average, if you could ascertain by any experience what the average was, which is really all that you could do, it would be no means of testing whether an analysis taken of fresh milk at the time it was fresh is right or not. You cannot test it by what the average of loss three weeks afterwards is. It may be that in that particular case there has been no loss at all ; it may be a case in which the loss has been double the average. You have therefore no means of testing it ; and to say that you have tested it by an average is to pass no judgment at all, as it seems to me, in a case in which analysts have analysed the milk when fresh. Therefore this is a case which is of considerable importance : because the Public Analysts, Mr. Estcourt among them, are in the habit of testing milk properly at the time when it ought to be tested, namely, when it is fresh, which is the only time when you can take really reliable tests ; and if the results arrived at, not merely by Mr. Estcourt, but in this case by independent Public Analysts acting really under the instructions of the appellant himself, from analyses made of the milk before decomposition had set in show that there has been adulteration, and shew results which I think Dr. Bell himself would have to admit prove that there was adulteration, are they to be set aside by a test taken by a chemist, however eminent, some three weeks afterwards when decomposition had set in, because he says not "I will affirm that it is impossible that Mr. Estcourt's results are correct," but "I am unable to affirm from my examination of this decomposed milk that there has been adulteration?" The matter is one of very considerable importance, because although 4 per cent. of water is not in itself a very large quantity, it represents on the consumption of Manchester, I am told, something like £10,000 a year—that is to say that if all the milk sold in Manchester were adulterated to that extent it would represent £10,000 a year on the consumption of Manchester. It is impossible to say that this 4 per cent. is the limit of the adulteration in this particular case. What these gentlemen say is, that it has been adulterated to the extent of at least 4 per cent.

The Recorder : What do you say was the object of the Legislature in making this *quasi* Court of Appeal at Somerset House ?

Mr. Gully : If you ask me what the object was, I can only answer that I believe the object was the same as that with which many clauses are put into Acts of Parliament. There are many conflicting interests in Parliament, and some County Member, having in view the interests of the farmers, has some theory about the matter and gets this clause inserted, thinking it will protect his constituents, and the Minister in charge of the Bill does not object, thinking it will do no harm. Nobody sees how many days it will take before the milk can reach the hands of the chemists at Somerset House, but when the Act is put in force it is found that there is very serious difficulty. I say that the evidence from Somerset House is to be taken for what it is worth. Of course, if it were not open to any comment which ought to have weight with the Magistrates, and they found a conflict between two chemists, they would say that in the face of that conflict of testimony they ought not to convict. I quite agree ; but at the same time it is to be taken as a piece of evidence which is to be considered and dealt with like other evidence ; and if it can be shewn, as we did shew to the satisfaction of the Court below, that this discrepancy is accounted for in a way which is not only consistent with the analysis of Mr. Estcourt being a correct one, but consistent also with Dr. Bell's analysis when you take out what is unreliable in it, then it is to be dealt with like you would deal with any other evidence given by any other witness, and it should be set aside if you do not think it is valuable as compared with the evidence of the other side.

Mr. Cottingham : I think now is the most convenient time to draw your attention, Sir, to one of the grounds of appeal in order to raise a question of law upon it which you have just at this moment touched. It may shorten the case if I read the third ground of appeal, "That the said Court of summary jurisdiction having exercised the discretion vested in them by section 22 of the Food and Drugs Act (38

and 39 Vict.) 1875, and obtained a certificate from the chemical officers' department at Somerset House, were bound by the contents of such certificate and ought not to have convicted me." That is our third ground of appeal. It has evidently struck the mind of the Court that the legislature must have had some object in view in introducing this clause. Before I go into the object of the clause, it is quite apparent that the clause for whatever purpose introduced would become a dead letter if my friend Mr. Gully's argument were to prevail as to the length of time between the taking of the sample and the sending of the sample to Somerset House, because this 22nd section assumes that a prosecution has been commenced, the samples have been analysed by Public Analysts and that the defendant is upon his trial; and either he or the prosecution may request the justices to send another sample to Somerset House for the purpose of analysis. That being so it is evident that the Act of Parliament assumes that a considerable interval will elapse, and an interval too which is entirely at the option of the prosecutors; because they may delay the prosecution for a considerable length of time. This provision would become a dead letter.

The Recorder: Not quite. What struck me when Mr. Gully was addressing the Court was that it may be that this appeal under the circumstances is not conclusive. It may be that the legislature intended that protection should be thrown over the dealer in milk against some grossly ignorant conviction upon some grossly ignorant analysis; and if the local analyst had said there was 10 per cent. of water, or 20 per cent. of water, or 30 per cent. of water, and the Somerset House analysts said that is all nonsense, there is nothing of the sort, that would of course be conclusive, not in law, but in the mind of the justices. However, let us look at the exact words. What you say, as I understand, is that the appeal to the Government analyst is conclusive?

Mr. Cottingham: I say that it is conclusive and binding upon the justices.

The Recorder: Now let us look at the words of the Act of Parliament.

Mr. Cottingham: Section 22. The marginal note is "Power to justices to have articles of food and drugs analysed." Then the section itself is "The justices before whom any complaint," &c. So that in point of fact you are sitting here to-day, months after the samples have been first taken, and the milk would now be in a state of putrefaction, yet we might ask you now to send a sample to Somerset House for examination. Then what becomes of my friend's argument as to the inutility and the imperfection attending Dr. Bell's analysts only a few days after the sample was taken when there was only incipient decomposition. You might now order samples of this milk to be forwarded to Somerset House; therefore the law must have contemplated that an indefinite interval should elapse between the sale of the milk and the examination of the samples at Somerset House. It is true that the section does not say in express terms whether it should be binding or not. The only provision is that the justices may inflict the costs of the examination by the officials at Somerset House upon either of the parties. Now, my friend has said that some county member may have introduced this clause.

The Recorder: I am bound to suppose that the legislature has done nothing that is not absolute wisdom.

Mr. Cottingham: Allow me to draw your attention, Sir, to the reason why this clause was introduced. This Act of Parliament of 1875 underwent considerable discussion in Committee. There was a report made upon it by a Committee; but during the examination several attempts were made to establish a standard such as my friend is contending for now, and after various attempts had been made it was found impossible. I have the Blue Book here; and after examining Dr. Voeleker, Dr. Bell, and all the most eminent chemists in England, and some foreign chemists also, the committee found that the adoption of any standard was impracticable; but in substitution for a standard they introduced this 22nd section as a sort of appeal to the Government analysts, who are supposed to be and necessarily are perfectly independent in the matter, and who were intervened in a case of difficulty such as this. The committee with great wisdom and in accordance with the great weight of the scientific evidence adduced before them were unable to and would not adopt a standard.

Mr. Gully: I am quite prepared to cite from Hansard on the other side of the question. I have Hansard, and my friend has the Blue Book; but I quite agree that you must decide upon the words of the Act itself.

The Recorder: I quite agree with you, Mr. Gully, as to the interpretation of the section. It seems to me perfectly clear what the section is. The section is that there is to be a reference to the chemical officers of that particular department at Somerset House. They must make an analysis and give a certificate to such justices of the result of the analysis. It is not to bind the justices; but it must clearly be one of the matters which the justices are to take into consideration in coming to their decision.

Mr. Gully: Take this case for illustration—that a man had admitted that he put the water in afterwards?

The Recorder : Yes, there is no doubt about it.

Mr. Cottingham : This is the evidence given by a very eminent analytical chemist, Dr. Stevenson Macadam. He was asked "Do you not think that being a Government department it would be better than almost any other court of appeal could be ?" This is his answer, "I think if we had the processes thoroughly worked out, and authenticated processes submitted for working out the Act, Somerset House might be a court of appeal; for, so far as the range of analytical work is concerned, they certainly are competent to do it; but I am still in a little difficulty as to whether they are the proper parties to frame processes for the analytical examination of all the articles under the Food and Drink Act."

The Recorder : I can only look to what the legislature enacted ; I cannot decide the case upon the opinions which were given to the legislature before they formed their judgment.

Mr. Cottingham : I want the Court to take notice of this: that attempts were made during the progress of this bill through committee to establish a standard; and the attempt to establish any standard as to non-fatty solids from which to draw a conclusion as to adulteration of milk entirely failed ; and in substitution for that standard they have introduced this 22nd section in order to allow the analysts of the Government to intervene. However, you hold that it is not binding ?

The Recorder : I have no doubt about it, looking at the Act of Parliament. There is no necessity for sending it on to the justices if the sending the sample to Somerset House were intended to be conclusive.

Mr. Cottingham : You see that no time is fixed for the sending of the sample to Somerset House ; and any interval of time may be supposed to elapse from the taking of the first sample to the time of the sample being sent there ; therefore the legislature assumes that the milk would be in a different state when it reached the Government analyst to that which it was in when it was tendered to the public analyst for examination.

Mr. Gully : The section is not only about milk, but about any food or drug. Milk is perhaps the only article that would so decompose.

The Recorder : If you want a decision I clearly hold that the third ground of appeal is not good in law.

Mr. JOHN EDWARDS, SWORN.—Examined by Mr. Hopkinson :

Q. I think you are one of the Nuisance Inspectors of the City of Manchester ? A. I am.

Q. And you deal with questions under the Food and Drugs Act ? A. Yes.

Q. Do you remember in the early part of this year receiving complaints from Anthony Halewood, a milk seller of this city ? A. Yes.

Q. In consequence of those complaints, did you on Monday, the 23rd April, go to the Central Station ? A. Yes.

Q. Did you, about 8.20 in the morning, take samples of milk from two cans of milk which came by the train ? A. I shall have to look at my book to tell the time. Twenty minutes past 8 in the morning.

Q. Did you take samples from two cans arriving by train ? A. Yes.

The Recorder : Does not this case come to a point at which there is no dispute as to all this ? It is merely a question of whether the analysis is correct or not.

Mr. Cottingham : I will shorten it as much as I possibly can. No doubt what you suggest, Sir, is most important ; but I must hear what this gentleman has got to say.

Mr. Hopkinson : Did you, before taking the samples, mix the milk ? A. I mixed the milk well up.

Q. What did you do with the milk cans ? A. I got a " dozen " cans, and poured from the large railway can a quantity into the dozen cans, then dashed it back again. That was repeated twice over.

Q. And after that what did you do ? A. Then I took a sample from each of the two cans.

Q. What did you do with the samples ? A. I divided them into two parts, and sealed and labelled each one.

Q. What did you do with the parts ? A. One I delivered to Mr. Esteourt, the Public Analyst, and the other I kept at the office until Mr. Wardle came for it.

Q. Did you produce those samples that you sent to Mr. Esteourt, at the hearing before the magistrate ? A. Yes.

Q. And those were afterwards sent to London, I think? A. They were. I believe they were handed in by me to the Court.

The Recorder. I shall assume that all this was done correctly, and leave Mr. Cottingham to point out any inaccuracies that may have been fallen into by this witness.

Cross-examined by Mr. Cottingham:

Q. This milk was contained in two large cans, which they call churns? A. Yes.

Q. The milk was not in charge of the Defendant, Mr. Wardle, or in charge of anyone on his behalf other than the Railway Company? A. No one else but the Railway Company.

Q. How did you get access to the cans? A. As soon as the train arrived the cans were pointed out to me as being the cans belonging to Mr. Wardle.

Q. Of course you opened them. Was the lid locked, or what? A. No, Sir, they were not locked.

Q. So that you opened the cans without difficulty? A. Yes.

The Recorder: Which contention are you going to rely upon: are you going to say that this milk had had water put into it after it left the appellant's premises, or are you going to say that the milk, as examined by this man, was pure?

Mr. Cottingham: I say there is no evidence of impurity at all. That is, of course, the evidence on the analysis; but I want to ascertain how the sample was taken. A question will arise as to the milk he took—he took only morning's milk, as you will see, and that is what I am coming to.

The Recorder: Of course both grounds of defence are open to you: but if you are going first to shew that this water had been put in previous to the analysis, that will, of course, render futile any attempt to shew that the milk was pure.

Mr. Cottingham: I am not going to assert that at all; it may or may not be; I make no point of it. I want to shew the Court how the milk came to Manchester, and how this man got to it. I have only a question or two more to ask upon this:

Q. You opened the can without difficulty, and you took out the milk. About what quantity did each of those churns contain? Mr. Wardle: 17 imperial gallons.

Q. About how much of the milk did you pour out for the purpose of mixing it and pour back again? A. I filled the dozen-quart can, and poured it back again twice.

Q. You did not see Mr. Wardle, and you did not see any person on his behalf? A. No.

Q. You took it as an Inspector? A. I did.

Q. You did not pay for it? A. No.

Mr. Cottingham: I ask that because my friend, Mr. Gully, opened that there was a sale.

Mr. Gully: Then I will withdraw it. I do not know whether my friend admits that this was milk which was being delivered to Halewood under a subsisting contract to supply the milk?

Mr. Cottingham: This was milk that was being delivered to Halewood under a contract. We admit that the whole of the two cans formed one consignment under a general contract.

Mr. ANTHONY HALEWOOD, sworn.—Examined by Mr. Gully:

Q. Had you a contract with Mr. Wardle to supply you with milk? A. Yes.

Q. Had that been going on for a long time? A. Since October, 1882.

Q. Was he to send you all his milk, or so much a day? A. The produce of his farm—all his milk.

Q. Had you found it of good quality or not? A. No.

Cross-examined by Mr. Cottingham:

Q. You say you had a contract for this milk. At what price? A. In summer it was 1s. 1d. before October.

Q. What in the other parts of the year? A. 10d. per gallon for the winter months.

Q. Those gallons are not imperial measure? A. Imperial measure.

Mr. CHARLES ESTCOURT, sworn.—Examined by Mr. Gully:

Q. Are you a Fellow of the Chemical Society, and Public Analyst for the City of Manchester, and also for Oldham? A. Yes.

Q. Were those two samples, Nos. 203 and 204, handed to you by the first witness? A. They were, on the 24th April.

Q. I want to take it shortly. Were they made up in the usual way in which samples are made up? A. They were.

Q. Did you analyse them? A. I did.

Q. On what day? A. On the same day.

Q. On the 24th April? A. Yes.

Q. How soon after you got them? A. Each was put into operation immediately. I received them on the 24th, and analysed them on the 24th.

Q. And you made out your certificates on the 25th, and those are they which have been put in and read? A. That is so.

Q. Would there be any change in the milk on the 24th which would affect in any way the analysis? A. None; they were fresh.

The Recorder: They were fresh when you analysed them? A. They were fresh.

Mr. Gully: Have you got the details of your analysis? A. I have.

Q. Just give them shortly to the learned Recorder. A. Sample 203—solids, not fat, 8·87.

The Recorder: That is non-fatty solids? A. Yes, non-fatty solids—fat 2·54. Sample 204—non-fatty solids 8·62.

Mr. Gully: Finish with the first sample—total solids 11·21? A. Yes, 11·21 total solids. No. 204—8·62 solids not fat, and 2·81 fat.

Q. That makes 11·43? A. 11·43 total solids.

Q. Of course the balance 88·57 would be water? A. Would be water. 11·43 is the total solids.

Q. And we may leave the water out of consideration altogether? A. Yes.

Q. Whatever is not solids not fat, or fat, is water? A. That is so.

Q. Is the fat a matter that it is material to consider in this analysis? A. It is not.

Q. That may be left out? A. That may be left out in calculating the amount of water.

Q. That may affect the richness of the milk, the amount of cream, or fat, I suppose? A. Quite so.

Q. But it is by estimating the amount of solids not fat, that you ascertain whether there has been adulteration by water? A. Yes.

The Recorder: You ascertain it by the quantity? A. By the quantity of solids not fat; that is how we ascertain adulteration with water.

Q. That would not apply to anything else? A. No; skimming would be ascertained by the amount of fatty solids.

Mr. Gully: The solids not fat consist principally, I believe, of casein and albumen? A. Casein, milk sugar, and mineral matter.

The Recorder: What is casein? A. Curd, which, with fat, makes cheese—the cheesy matter.

Mr. Gully: In testing whether there has been adulteration, you have first to get rid of the water? A. Evaporate the water away at the temperature of boiling water as nearly as possible.

Q. I think you had better describe what you do. You first of all evaporate the water. How do you do that? A. You weigh the quantity of milk, place it in a small vessel, which is placed upon a water-bath with steam impinging upon the bottom of the vessel. That evaporates the water away.

Q. How long do you subject it to that? A. Three hours.

Q. By that time the water is evaporated? A. By that time it does not sensibly lose weight—it is dry by the time.

Q. What is left is not affected by the steaming? A. That is so. I had better explain that, you may go on drying it for any length of time, it will continually lose a very small quantity. The method pursued by Public Analysts is to weigh it at the end of three hours.

The Recorder: Is this method impugned?

Mr. Cottingham: Certainly. We say that this method is entirely obsolete, and founded upon a false basis.

Mr. Gully: "Obsolete" means by the other side?

Mr. Cottingham: It is obsolete in the opinion of all practical scientific men, Dr. Voelcker among others.

Mr. Gully: When you have evaporated the water, you proceed to get rid of the fat? A. Yes; we use any fat solvent, and that is poured upon the milk solids which are left in the vessel, and it is heated on the bath; and then the liquid portion is decanted off.

Q. And you pour on ether, I believe? A. Petroleum ether; ordinary ether will do, or benzoline. that is poured upon it, and then decanted off, and it gradually takes away all the fat.

Q. You mean that you strain it away? A. You pour it off. The method of analysis differs from the one Mr. Cottingham will bring forward, I suppose. Inasmuch as the material is not detached from the vessel, therefore we only pour it off. If it were detached from the vessel we should have to filter it. We do not powder it.

Q. By means of ether you separate the fat from it? A. Yes, that is so.

Q. Leaving a residuum of non-fatty solid matter? A. Yes.

Q. That you found to be 8·67 in the one case and 8·62 in the other? A. That is so.

The Recorder: Can you tell me how it happens that the analyses were different when these two samples of milk were taken from the same churn, or from a portion of two churns mixed together? A. I am afraid the last witness has not put the case clearly if your Honour understands that to be the case. The milk was not taken from two churns mixed together. Each of those samples was taken from separate churns. Those separate churns, from the method adopted at most farms, are likely to contain milk of different cows.

The Recorder: That has cleared up what I wanted to know.

Mr. Gully: Sample 203 you understood to be a sample from one can? A. I understood the evidence to be so; but I have nothing but the numbered sample.

Q. And sample 204 from another can? A. Yes. There might be a greater difference between the two cans even than there is.

Q. One can might contain the milk from one dozen cows, and another can the milk from another dozen cows? A. Quite so.

Q. Which may account for the small discrepancy? A. Yes; and there might be a larger discrepancy. There frequently is from cans from the same farm.

Q. Your difference is 5 per cent. between the two? A. It is 5/100ths.

Q. .05 I mean? A. Yes.

Q. You have described the method. You are a Member, I believe, of the Society of Public Analysts? A. I am,

Q. Is that a Society to which most of the Public Analysts of counties and boroughs belong? A. It is; and a large number of other analysts also belong to the same Society.

Q. You have great experience in analysis of milk? A. I have—500 samples per annum for the last two years, and 400 samples for the previous five or six years.

Q. Is that including experiments for your own satisfaction? A. Yes.

Q. Is the system you have described that which you follow? A. It is; and it is in general use among Public Analysts.

Q. As the result of your general knowledge and experience, which do you find to be the best and safest method of analysing milk? A. The one originally adopted, and generally adopted by Public Analysts.

Q. Does 8·67 and 8·62 shew adulteration in your opinion? A. It does.

Q. To what extent? A. I calculated it out to the extent of 4 per cent.—that is calculating it to the lowest limit which the Public Analysts found from a dairy of cows, when their method was followed, which is 9 per cent.

Q. I do not understand that? A. The lowest amount of non-fatty solids that has been found on an analysis of genuine milks by the method adopted by Public Analysts is 9 per cent. That is the standard I take.

Q. Any milk shewing a less amount of solids not fat than 9·00 per cent. shews adulteration? A. It does.

Q. To work out how much water that shews is only a matter of arithmetic? A. Quite so.

Q. You say that the difference—the 4 per cent. was water? A. 4 per cent. of water.

Q. You can shew how you work that out if you are asked? A. Yes.

Q. You say that 9 per cent. is the standard, so to call it, that you take? A. It is the minimum. It is rather a limit than a standard; it is the limit below which the Public Analysts did not deem it advisable to permit the addition of water to milk.

Q. What do you find is the actual quantity of solids not fat in pure milk? A. I have made a series of analyses of milk from cows milked in my own presence; 173 cows gave an average of 9·3 milked in my own presence.

Q. They were milked in your own presence, but did you see the milk put away yourself that was to be sampled? A. Yes, I carried it myself.

Q. Is that of importance in your opinion? A. That is very important. I may say that I was assisted in some cases by four inspectors. It would be impossible to see a dairy of cows milked if one person only were to supervise it. If the object is to prevent the introduction of water one person could certainly not do that.

Q. That has all been done under your own personal superintendence, and what did you find the result was there—what was the average of solids not fat? A. 9·8 the average. The fat is also very high—3·68. There was nearly 13 per cent. solids.

Q. In how many different farms were those cows—or was it one dairy? A. They were in 17 different shippings, 6 different farms in Cheshire and near Manchester; the cows were stall-fed and grass-fed and all varieties; some morning's milk and some evening's milk.

Mr. Gully: Did you find that the cows being stall-fed or grass-fed made any difference in the amount of solids not fat? A. I have two samples here—the first of a lot of six shewed 10·52. That is one of the samples of grass-fed and 9·17 is the lowest there. Then I have stall-fed. 9·1 is the lowest and 9·44 is the highest of the stall-fed. This was a large farm near the town—near Manchester.

Q. What do you find those range from? What do you find are the limits of the range altogether? A. From 10·52 down to 9·01.

Q. That is the lowest of 173 cows? A. That is the lowest. There are 95 more cows which were not milked in my presence but in the presence of our inspectors.

Q. You say they were not milked in your presence? A. That is so, but I have analysed those.

Q. And no pure milk ought to contain less than 9·00. A. That is so. I did not find a single dairy less than 9·00.

Q. Have you made many experiments with the same view before, besides your large experience in analysing for cases of this description? A. Yes.

Q. Do you find that pure milk always contains at least 9 per cent.? A. That is my experience and there were very few cases where I found it as low as 9.

Q. Did you make the analyses of 203 and 204 in precisely the same way in which you have made all others? A. Precisely.

Q. Speaking as a scientific man have you any doubt that those were adulterated to the extent of 4 per cent. with water? A. From my experience of the milk of cows I have no doubt whatever.

Q. Have you experimented also upon milk when it has been fresh and pure and upon the same milk afterwards when it has become decomposed? A. I have.

Q. Do you find that you are able to test the one analysis by the other? A. No. I find from my experience that it is impossible. There is no relation between the length of time and the decomposition.

Q. In the first place the setting in of decomposition I believe does diminish the amount of solids not fat? A. It does. The sugar is acted upon principally.

Q. The longer the decomposition goes on and the greater the amount of decomposition the greater the diminution? A. That is so.

Q. Is there any precise rule as to when decomposition sets in or as to what its rate is? A. None whatever. It would depend upon the temperature and the condition of the milk, the conditions as to keeping, upon whether the milk had been watered or was pure—all those affect the rate of decomposition.

The Recorder: And the state of the atmosphere? A. Quite so. If you take a sample of perfectly pure milk, and water it and put a portion of the pure milk by and a portion of the adulterated by, at the end of seven days you will find a difference in the rate of decomposition.

Mr. Gully: If it has been already adulterated with water it will decompose at a different rate to a portion of the same milk which has not been so treated? A. Yes.

The Recorder: How fast or how soon? A. There is no law which governs it apparently.

Mr. Gully: Have you been able to ascertain any rule about it? A. I have not.

Q. Except that decomposition does tend to diminish the weight of solid matter? A. It does. I may mention that ten years ago it was attempted to estimate the solids in milk decomposed by neutralizing it. A paper was written at the time, but it was proved that it was totally unreliable.

Mr. Cottingham: Whose paper was it? A. Dr. Stevenson's. It is in the Society's proceedings.

Mr. Gully: Your opinion is that tests of decomposed milk are not reliable? A. I could and would pronounce no opinion as to results of any analysis of decomposed milk.

Q. Would you condemn decomposed milk on an analysis? A. I should say I was unable to pronounce an opinion upon it. I do not mean simply turned sour, but decomposed.

Q. I understand that Dr. Bell by his analysis brings out 8·20 and then he adds a certain amount.

A. From a table in his book.

Q. Dr. Bell has written on this subject, therefore you are familiar with his views no doubt. A. I am.

Q. And you heard what he said before? A. I did.

Q. He added 4/10ths to make it up to fresh milk? A. Yes.

Q. Is that in your opinion a process that can be relied upon? A. I fail to see how he can apply that 4/10ths to the milk he analysed. That 4/10ths may be a proper average, it may apply to some particular milk; but there is no possibility of applying it to any special milk.

Q. It may or may not be a proper average upon a number of experiments? A. Yes.

Q. But would those experiments shew a very great range of difference? A. Yes, they would.

Q. Would some shew no alteration? A. Undoubtedly.

Q. And some a very great change? A. Yes, much beyond the allowance there even.

Q. Dr. Bell brings it out in the first instance at 8·20. Does he use the same process that you do? A. I judge not from the book in which he published his process. It is entirely different.

Q. You know what Dr. Bell's process is? A. I do.

Q. Is that the same process as yours? A. It is not.

Q. Would that bring out the same result in figures of solids not fat if you and he were both to analyse precisely the same milk? A. It would not.

Q. If you brought out for example 8·67, what would you expect his analysis to bring out? A. I should judge it would be uncertain.

Q. Then you do not think his as certain a method to begin with as your own? A. I think not. I may be allowed to point out the reason, perhaps?

Q. Yes. A. It is simply because the milk is not dried thoroughly, but is left in a pasty condition in Dr. Bell's method, and then ether is added to that for the purpose of dissolving out the fat. The milk being in a pasty condition when it was done, water would be there, and the effect of ether being added to the water would be to dissolve out some of the sugar. The ether will not of itself dissolve the sugar, and the effect of putting it on when water is present is to decrease the solids not fat, and increase the fat. This will vary according to what may be the pasty condition, and when analysing what may be a pasty condition to one man would not be a pasty condition to another.

Q. Do you find that he brings out more fat than you do in your analysis? A. I have looked through the series given by him, and find that the fats are always high. You will find there is an increase of fat in both cases—Nos. 203 and 204.

Cross-examined by Mr. Cottingham:

Q. Can you tell us about this Society of Public Analysts: how long it has been in existence? A. I think since somewhere about 1874 or 1875.

Q. It came into existence perhaps a little before the passing of the Act of 1875? A. It came into existence immediately after the passing of the Act, which appointed Public Analysts. It could not exist before.

The Recorder: After the passing of the Adulteration Act? A. Undoubtedly.

Mr. Cottingham: Was it in existence at the time that this bill of 1875 was passing through Committee? A. It was.

Q. This minimum standard of 9 per cent. is what is called the Wanklyn standard? A. Public Analysts' limit.

Q. Was not that 9 per cent. standard introduced by Mr. Wanklyn? A. Professor Wanklyn first observed the constancy of milk solids not fat.

Q. When was that 9 per cent. established for the first time? A. It would be at a meeting of Public Analysts.

Q. When? A. I cannot call it to mind: it would be 1874 or 1875.

Q. It is founded I believe upon an average? A. By no means.

Q. Do you understand?

The Recorder: Mr. Cottingham, he perfectly understands. He says it is not founded upon an average; it is a limit.

The Witness: It is not an average. The average would be very much higher than that.

Mr. Cottingham: How do you get at that?

The Recorder: He has explained. I should be sorry to lecture upon such matters, but I think I

understand him. He says that it is not an average, it is a limit; and no good milk would have less than 9, and that the average of the contents of good milk would be somewhat higher.

The Witness : That is so.

Mr. Cottingham : Will you explain to me how this 9 per cent. is arrived at? What is the process by which it is arrived at?

The Recorder : It is arrived at in the way he has explained. I do not say that it is right, but he has explained it; and by a series of experiments he has made, he finds that in no pure milk is there less than 9 per cent. of non-fatty solids.

Mr. Cottingham : I will put it in another way. At all events the mode in which it was arrived at by Wanklyn, and also by you, was by treating a small portion of the milk in the way you have described. You dry it for about three hours? A. That is so.

Q. In these experiments and these analyses did you first weigh the total solids. Then having done that you extracted the fat? A. Yes.

Q. Did you weigh the fat? A. No.

Q. Did you extract the fat by treatment with ether? A. Benzoline, or petroleum ether.

Q. Then what did you do with the residuum not fat? A. The non-fatty solids were dried then.

Q. How were they dried? A. In the water-bath.

Q. How long were they submitted to the drying process? A. Three quarters of an hour.

Q. Did you weigh them? A. I weighed them.

Q. I did not understand you to say that you dried either the fat or the non-fatty solids to a constancy? A. Yes, there were weighings in between, and when they had practically ceased to lose weight.

Q. Never mind practically. Did you dry either the fat or the non-fatty solids down to a point where they could lose no more weight by the process of drying? A. What limit? When they ceased to lose 1/100th of a grain I should say they were practically constant.

Q. Do you mean to affirm as a matter of science that you dried the fatty or non-fatty solids down to a point at which they could not lose any more? A. I should say 1/100th of a grain would be a limit. 1/100 would be the loss probably when you had dried down for three hours.

Mr. Gully : Do you mean there would be only that left to lose? A. That would be the loss in the weighings at intervals of half an hour probably.

Mr. Cottingham : That was the result of your three hours drying of the fat? A. Three hours drying of the total.

Q. You say you extracted the fat and you dried the fat three hours? A. No.

The Recorder : Not fat; the total solids? A. I dried the total solids for three hours and then extracted the fat and dried what was left for three-quarters of an hour.

Mr. Cottingham : You weighed the total solids. Why did not you weigh the fat? A. It is a matter of arithmetic. If I weighed the fat I should not weigh the solids not fat, because the loss of one leaves the other.

Q. You first weighed the total solids and then you extracted the fat. I want to know why you did not weigh the fat that you extracted? A. I find the other method is accurate.

Q. There is a more modern method of analysis than that which you adopt. Supposing that you had dried both the fat and the non-fat to a constant weight, would not that have given you a different result from the 9 per cent. on the same milk? A. Yes.

Q. That is to say that the same milk treated by the modern method of drying would shew less per cent. of non-fatty solids? A. It is not a modern method.

Mr. Gully : That is your word, Mr. Cottingham.

Mr. Cottingham : We will call it the other system? A. No, a system.

Q. At all events it is not your system? A. It is not and it is not the system adopted by Public Analysts.

Q. You will not be surprised to hear that it is the system of the Government Analysts? A. I understand it is from the book.

Q. You admit, as I understand, that if you proceed by the other mode in vogue with the Government Analysts you would have come to a different result—you would have got less non-fatty solids from the same milk? A. Less solids.

Q. Would you not have got less non-fatty solids by that other method? A. No doubt.

The Recorder : Why? A. If it loses $\frac{1}{100}$ of a grain in half-an-hour it would possibly lose in the

course of drying a certain amount of organic substances which decompose to some extent on heating. That was one object in the method adopted by the Public Analysts—to avoid that, and to have one settled way of doing it.

Q. What would be the difference of non-fatty solids arrived at by the two different processes? What would be the difference in weight? A. That I cannot say. Milks would possibly decompose in a different way on heating strongly and for a long while—they lose water of constitution.

Q. You say you found 4 per cent., or you deduct 4 per cent. of water. Did you apply any test whatsoever except the presence of the minimum 9 per cent.? A. I did not.

Q. No difference in the specific gravity of the milk? A. I did not take the specific gravity.

Q. What was the quantity—how many grammes did you analyse? A. 100 grains was the quantity I used.

Q. Now as this 4 per cent. of water. That is a very small amount of adulteration is it not? Yes, it is.

Q. Supposing a trifling mistake of a few grains in weight to be made, it would make a corresponding difference in the amount of adulteration? A. It would. A mistake of 10 grains would make a difference of 10 per cent.

Q. 10 per cent. of water? A. Yes.

Q. A mistake of 10 grains in the weight would make the difference? A. Yes.

Q. You did weigh, but you did not measure? A. I weighed and delivered it into the basin with a measure. I always weigh. The calculations are all made upon weight.

Q. Did you perform this analysis on duplicate? A. I did.

Q. You know Mr. Otto Hehner? A. Yes, he is here.

Q. He is called on your side? A. Yes.

Q. I hold in my hand numbers of a paper called THE ANALYST. I believe it is published very much under the protection of the Public Analysts? A. It is the Society's Journal. You may take it that it represents the opinions of the Society largely.

Q. Just give me your attention to this. Here is a paper read by Mr. Otto Hehner in April, 1882. I ask you do you agree with it?

Mr. Gully: I will call Mr. Otto Hehner.

Mr. Cottingham: I am cross-examining this gentlemen. This is the paper—"On Some Points in Milk Analysis. By Otto Hehner, F.C.S., F.I.C. Read before the Society of Public Analysts' on 15th March, 1882." (ANALYST, vol. vii., page 60.)

Q. Do you agree with that? A. If you alter the method you alter the limit; then I quite agree.

Q. If you had dried for six hours there would have been the difference between 11·27 and 11·21? A. There is no difference at all there.

Q. The result at the end of three hours was 11·27, and at the end of six hours 11·21? A. A difference of $\frac{1}{10}$. I said there would be a difference of $\frac{1}{10}$ in an hour.

Q. Then he takes 5·0916 grammes treated as above.

Weight of residue after 2 hours .5764 or 11·32 per cent.

8 , .5727 , 11·25 "

4 , .6714 , 11·22 "

5 , .5702 , 11·19 "

6 , .5698 , 11·19 "

So that you see from this the weight is gradually diminishing down to this point.

Mr. Gully: That is just what this witness has told us.

The Witness: What is the last temperature at which he dries?

Mr. Cottingham: He goes on to say "It appears to me that as much more concordant results are obtained when the solids are dried to constant weight than for three hours only, and that as the fat is much more completely, readily and with a less amount of trouble extracted in an extractor such as Soxhlet's,—that is a mode of extracting different from the mode you employ—"it would be well to discard the old plan, and accordingly to lower the limit of solids not fat from 9 to 8·5 per cent." What do you say to that, Mr. Estcourt? Do you agree? A. I agree that you may get anything by a change of process. I can invent another process which shall get you still lower solids or raise them higher.

Q. If you have a process that is correct; if this process is a correct one, it results in obtaining from the same milk 8·5 of non-fatty solids instead of 9? A. And it also results in obtaining from the genuine milk 8·5 also.

Q. Then what becomes of your standard of 9 per cent. as the minimum for non-fatty solids in genuine milk? A. If that method is adopted then the standard would have to be lowered.

Q. Can you support your standard after that? If where you adopt another method you get by treating the same milk only 8·5 of non-fatty solids instead of 9 per cent. by your method, upon which 9 per cent. you found the 4 per cent. of adulteration, how can you support your standard? A. I scarcely understand your question, I must confess. I am prepared to say that you will alter the figures by altering the method.

Q. How can you say that your standard of 9 per cent. is an infallible standard? A. Because we do not alter the method?

Q. We say that you do not use the proper method? A. Ah, we do not agree with you there.

Q. You have abandoned 9 per cent. once, you had to lower it? A. No, I have no recollection of any such thing, Do you mean me personally?

Q. It was 9·3 once? A. No. 9·3 was used as a basis for calculating the amount of adulteration by some analysts.

Q. So much for Hehner. Do you know Mr. Bernard Dyer? A. Yes, he is a Member of our Society.

Q. I am now reading from THE ANALYST of April, 1881. Some Analyses of Milk, by Bernard Dyer, F.C.S., F.I.C. (See ANALYST, page 59, vol. vi.) Read before the Society of Public Analysts, on the 16th of March, 1881. Then he goes on and gives the results on the 8th, 9th and 17th of July. He brings out these amounts of solids not fat—9·15, 9·52, 9·36, 8·82, 9·05 and 9·02. There are two of those instances in the same month below your standard.

Mr. Gully: I ask you, Sir, whether we are to have experiments brought forward which nobody knows anything about. I do not know whether Mr. Dyer is here or not. He is certainly not here for us. My friend is putting these figures to the witness and asking whether he agrees with them. They are experiments upon certain cows. How can he agree or disagree. We know nothing as to how the samples were taken or how the results were obtained, and upon cross-examination they are quite worthless. My friend's question is—"Do you agree that such and such cow's milk, when tested, produced such and such results?"

Mr. Cottingham: I ask him how he supports his minimum standard of 9 per cent., or the standard of this Society in the presence of this statement, and these experiments made and offered to the Society by one of the members.

The Recorder: If I am not wrong, I understand his answer to be this with regard to Mr. Hehner: That a different process of testing will bring out different results; and if you adopt a different process of testing you must of course adopt a different standard of purity. That I can perfectly well understand.

Mr. Cottingham: I think I have not made my point clear. The standard which he sets forth, which is the 9 per cent. standard, cannot be an infallible standard.

The Recorder: Nobody, as far as I know, ever suggested that it is an infallible standard. The suggestion is merely that it is an accurate standard.

Mr. Cottingham: I think I have not conveyed my point?

The Recorder: I think I see your point, and I think I see the answer to it.

Mr. Cottingham: This 9 per cent. is arrived at by a certain process. That process according to Mr. Hehner is not the most reliable process, and he has substituted another. When some of the same milk which yields 9 per cent. as a minimum only of non-fatty solids is treated in the other way, it lowers the amount of non-fatty solids to 8·05.

The Recorder: I am not expressing an opinion. I am only repeating what I understand this gentleman says. His answer is perfectly sensible and perfectly intelligible even to my mind, and I know nothing of these matters.

Mr. Cottingham: We are upon the question of the standard, and when we find that this standard is impeached and the method by which it is arrived at is impeached also, I want to know from Mr. Estcourt how he can assert that that standard is a reliable one.

The Recorder: He has given his reason, and it appears to me to be a very good one. I am afraid shall expose my ignorance, but I will venture to put in my own words what I understand. You are

to boil a certain quantity of the thing three times with ether. That will produce a certain result. That might be a very good standard. If you boil it six times you produce different results.

Mr. Cottingham: Let me put this question to you. Supposing you had adopted the second method and brought out the non-fatty solids 8·5, would you have pronounced the milk to be genuine? A. I have already said that I do not understand the method to be an accurate one, therefore I should not pronounce an opinion founded upon it. A portion of the solids not fat are dissolved out.

The Recorder: If you thought that was a more correct method of analysis you would then bring your standard down from 9 to 8.

Mr. Cottingham: What reason have you for saying that the former method of evaporating for three hours, but still leaving a quantity of moisture not evaporated, and still leaving weight that might be got rid of —

Mr. Gully: He has not said all that.

The Witness: I do not say that moisture is left at all.

Mr. Cottingham: The method that you adopted was drying for three hours? A. Yes.

Q. You admit now that there would still be a residuum of moisture? A. I have not admitted that.

Q. Do you contend still that after evaporation for three hours you would get down to a constant weight? A. No. I will explain it if you like.

Q. Answer the question.

The Recorder: He is giving you his answer.

The Witness: I say that the milk is decomposed by heating for a long time, when decomposition causes the loss, and not a loss of moisture.

Mr. Cottingham: You are speaking in the presence of many other chemists who will be called by-and-bye. A. Yes, I know that.

Q. You know Dr. Voelcker? A. Yes.

Q. You say that the more recent method is not a reliable one, and that the method you adopted is the proper one? A. It is more reliable I say.

The Recorder: You do not say that the other one is not, but you say your own is the best? A. Yes.

Mr. Cottingham: You admit that the different methods would produce different results? A. Yes, and different standards.

Q. The writer of this paper, Mr. Bernard Dyer, goes on to say this, that on the 4th, 5th, 11th, 12th, and 18th August, he finds that the non-fatty solids amount to 8·74, and with that he concludes his experiments. Then he says—"It will be noticed that B"—that is the Table I have last referred to—"averaged only 8·7 per cent. of solids not fat, and only on one occasion was the limit of the Society actually reached, viz., on Aug. 19th, when the morning milk yielded 9·08 per cent. of solids not fat."

Mr. Gully: I think, Sir, that you should know that this was the milk of separate special cows which were fed and experimented with in different ways to show what the effect upon their milk was. I cannot see what light it throws upon this.

Mr. Cottingham: It shows the variation of the standard—whatever it is worth. (THE ANALYST, vol. vi., p. 61.)

Q. I want to know what you say to this: "The proportion of fat should be very carefully considered in conjunction with the solids not fat before an opinion as to adulteration is pronounced." You say that the proportion of fat is of no account at all? A. I will answer if you permit me.

Q. I understand you to say that you do not consider the amount of fat any test at all? A. I will give you an explanation which will satisfy you if you like.

Q. I want to get your answer.

The Recorder: He cannot answer categorically Yes or No.

The Witness: If I were analysing a sample of cream which contained 20 or 30 per cent. of fat, I should find the solids not fat very low indeed, probably only 7 per cent.; but the fat there would be very marked in its amount—20 per cent. instead of 2·8 or 2·5—and that amount of fat would be accompanied by a diminution of the amount of solids not fat; both of them cannot exist in the same

100 grains; one is depressed and the other is raised. In that case if I found a large amount of fat I should not condemn the milk: I should know it was a cream.

Mr. Gully: He says he should know that they had given him a specimen of cream and not of milk.

Mr. Cottingham: When you said that you did not weigh the fat, I understood you to say that you did not weigh it because you thought that the presence of an amount of fat was of no value at all in estimating the amount of non-fatty solids? A. I find that the readiest, and best, and most certain method. It is equally as certain as weighing the fat. I deducted the solids not fat from the total solids.

Q. You say that the amount of fat is of no moment at all in the estimation of the non-fatty solids when you test for water. Do you think that the consideration of it is of any moment in testing for adulteration by water? A. No, not unless it exceeds a certain amount.

Q. "The proportion of fat should be very carefully considered in conjunction with the solids not fat before an opinion as to adulteration is pronounced." Can you reconcile your proposition with that statement? A. Yes, I have given you an example. If it were a cream I should consider the fat.

Q. The fat in this case that we have been dealing with now was over the Society's standard was it not? A. It was.

Re-examined by Mr. Gully:—

Q. You have told us the number of hours that you apply heat. In one way or another, first you get rid of the water, then you get rid of the fat? A. Yes.

Q. Applying the process of yours, and the amount of heat that you do apply by that process, ought pure milk to have at least 9 per cent. residuum of solid non-fatty matter? A. It ought.

Q. Applying the process, you mean? A. Yes.

Q. Is it a rule which you have found invariable in your experiments? A. I have.

Q. You say the non-fatty matter may safely be put as 9·3? A. Yes.

Q. If you go on applying heat—excessive heat if need be—to the residuum, will you go on diminishing its weight? A. Yes.

Q. Supposing you were to go on for days applying a red heat it would reduce it to ashes? A. Yes, undoubtedly.

Q. The weight of what was left would be infinitely little? A. Quite so.

Q. Is it a question of judgment at what point it is not worth while going on to apply heat? A. It was found by calculations and experiments that three hours was sufficient, and it was agreed that it should be the method.

Q. The experiment of Mr. Hehner, which my friend read, seems to have shewn that they got only a loss of 6/100 after applying three hours of additional heat to the residuum? A. Yes.

Q. That is 1/100th part of a grain per half hour? A. Yes.

Q. Is it in your opinion worth carrying on that experiment? A. It is not.

Q. Have you got all that is practically valuable for the purpose of testing at the end of the heating in your experiments? A. Yes.

Q. After a certain application of heat, when you have got rid practically of all moisture, is what is diminished by the continued application of heat the solid itself? A. Yes. Then the solid itself begins to decompose.

Q. You say for the purpose of ascertaining really what is the amount of solid residuum, yours is the proper method? A. With the limit that we apply it is the best method.

Q. I suppose that it would be almost impossible to ascertain the precise moment or second of time at which you ought to cease, or at which you would be able to say—Now I have exhausted every particle of moisture, and have not exhausted a single particle of solid? A. That would depend upon the delicacy of the balance simply.

Q. It would be a thing almost impossible for any man to draw? A. Yes.

Q. You have drawn it from your experience as nearly as you can? A. Yes.

Q. If you follow the other experiment and continue the heat, you get a lower figure when you leave off heating? A. Yes.

Q. Does it make any difference as to what is the quality of the milk? A. Granting the same standard it cannot affect it.

Q. If you know what the difference of process is, are those two quite consistent? A. They are.

Q. That you should bring out 9, and somebody else should bring out 8·5? A. Yes, or 8·6, or 8·3.

Q. Then less than 9 under your process you say is a sure sign of adulteration? A. I do.

Q. You know nothing personally of these experiments which have been read to you? A. I do not.

Q. How those samples appearing there were taken you do not know? A. I do not.
Q. On that a good deal depends? A. That is so.

JAMES ALFRED WANKLYN, SWORN.—Examined by Mr. Hopkinson :

Q. I think Mr. Wanklyn you are a Member of the Royal College of Surgeons, and you are Public Analyst for Peterborough and Shrewsbury, and other places? A. I am.

Q. Have you analysed many thousands of samples of milk for dairies? A. Yes; during the last thirteen years I have analysed thousands of samples.

Q. Is the method you employed in analysing those substantially the same as that which has been named by Mr. Estcourt to-day? A. Substantially it is the method brought out by myself about the year 1870, and it is generally adopted by the Public Analysts.

Q. And in this method you for three hours evaporate milk at a temperature of 212° Fah.? A. Yes, I keep milk at a temperature of 212° Fah. for at least three hours. In this way I get the total solids. Then I take the total solids. Then I extract the fat with ether. Then I take the solution in ether, evaporate off the ether and weigh the fat; then I subtract the fat from the total solids, and the difference is the solids not fat.

Q. Is your method substantially the same as that which Mr. Estcourt says that he adopted in this case? A. Yes, it is.

The Recorder: It is not quite the same? A. The difference is, that they weigh the solids not fat. As a matter of fact, I have never weighed the solids not fat. Observations have been made by others that the same result is got whether you actually weigh the solids not fat, or get your total solids and subtract the fat from it. I regard the method that I use as the safest. I do not say that it is the best; I say it is the safest.

Q. What should you say was the lowest limit of solids not fat in milk analysed by that method? A. In pure milk it is certainly not below 9. The average is 9·3. My experience is that in real milk the solids not fat never fall so low as 9.

Q. That is when you analyse by that method? A. Certainly.

Q. For that method would you say that 9 was a safe standard? A. That is the limit.

Q. The safe minimum limit? A. 9 is the safe minimum limit; 9·3 is the standard, and 9 is the limit.

Q. You have read the account of the method employed by the Analysts at Somerset House? A. I have.

Q. By this method does the heating go on for a longer time than by your method? A. Very much longer, and the heating is managed in a different way. They not only dry up in the water-bath as I do, but they afterwards dry up in the water-oven.

Q. Is it possible to increase the temperature? A. It is a very bad method indeed. When I arranged the method originally I avoided the water-oven. The water-oven is a source of uncertainty. It may be a pressure vessel.

Q. Increasing the temperature for too long a time would have the effect of decomposing the milk? A. Yes—it would de-hydrate the milk sugar.

Q. Do you find that according to Dr. Bell the milk sugar contains an atom less of water than your own? A. Yes. I should look to get water combined with the milk sugar. Apparently Mr. Bell wants to get anhydrous milk sugar. I get the crystallized milk sugar with a certain amount of water chemically combined with it. Dr. Bell writes the formula anhydrous milk sugar, and apparently works to get it. Keeping strictly at 100° and drying in the way I do, you get the hydrated milk sugar; and if you raise the temperature you get off the water which is chemically combined.

Q. You would decompose the residue? A. You would decompose the hydrated milk sugar and get anhydrous milk sugar.

Q. Would not the effect of that be if you attempted to set up a milk standard, on the Somerset House process, that you must have a lower standard than 9? A. You would have to have a lower standard. You would decompose the hydrated milk sugar and get anhydrous milk sugar.

Q. I think they also state that after the evaporation the residue is moistened with water. What would be the effect of moistening with water before the ether was put in? A. You risk dissolving away a little milk sugar along with the fat.

Q. That would be another possible cause of reduction of solids not fat? A. You would get them down.

Q. The solids not fat would be slightly decreased? A. The fat would be increased. 9 is the limit. 9·3 is the standard. I always calculate by 9·3. For instance, in this case I should say 7 or 8 per cent. of water. Strictly it should be "Most probably 7 or 8 per cent. of water, but 4 certainly"—"at least 4, but most probably 7 or 8.

Q. When the standard is 9, you think that the minimum amount of adulteration here is 4? A. Yes, the real adulteration in that case I should believe to be 7 or 8.

Mr. Gully: If the milk had been 9·3 or 9·4 in its pure state? A. Milk does contain 9·3 or 9·4 of solids not fat according to my method; but as a matter of fact I do not believe that milk ever goes down to 9 and the little difference of ·3 is allowed to cover error in manipulation and possible variation.

Q. Then of course you would say it was perfectly absurd to compare an analysis made by your method with a standard made by the Somerset House method? A. Certainly.

Q. In that case you would pass milk that was very much adulterated, I suppose? A. If you used my process and applied the Somerset House standard you would pass watered milk undoubtedly.

Q. With regard to decomposed milk. You have analysed various decomposed milks? A. I have.

Q. Is not it a usual result of decomposition, to some extent diminish the amount of solids not fat? A. It is, but the diminution is very irregular indeed.

The Recorder: Under what process? A. Keeping. If you keep milk for a length of time and then examine it, you may find the same result as at first, or you may find a loss of solids not fat.

Mr. Hopkinson: You find that usually the result of decomposition is to diminish the solids? A. Usually, but it is not invariably so.

Q. Have you found cases in which after keeping for a length of time the milk contains exactly the same amount of solids not fat as before keeping? A. I have, or practically the same. It may make a very considerable difference, or it may make next to no difference, or only a difference within the limits of experimental error.

Q. Can you then from the result of an analysis of decomposed milk say with certainty what that milk consisted of when fresh? A. Not accurately. In very gross cases, of course, you could tell. In a case where, for instance, it was half water, the ash would shew that. In a very gross case it would shew it; but in cases which are not gross the ash, which you rely upon when the milk is decomposed, would shew nothing.

Q. Could you detect 5 per cent. of added water? A. Oh, no; you could not detect 20 per cent. with certainty by the ash.

Q. Do you think any trustworthy results can be arrived at by a process of adding so much per week in respect to decomposition? A. Oh, no; I am sure it cannot.

The Recorder: I can quite understand that question and answer, but for my own information I should like to know whether in the calculations made by the Somerset House Analysts any rule has been laid down as to length of time.

Mr. Hopkinson: Perhaps you will allow me to read the cross-examination which took place on the previous occasion.

Mr. Cottingham: No.

The Recorder: Is that the view you take of the course adopted by the Government Analysts?

Mr. Hopkinson: The view we take of the course adopted by the Government Analysts is this. By the actual analysis they found in one case 8·2 per cent. of solids not fat, and in the other case a rather smaller quantity. After having found that as a matter of fact they say this milk is a little decomposed, we shall add on so much per day or per week for the loss by decomposition by rule of thumb.

Mr. Cottingham: Not by rule of thumb.

Mr. Hopkinson: That is what we say.

Mr. Gully: What we say is that they have added $\frac{1}{35}$ ths on that, and they have done that by this process: They have said the milk loses ·24 per cent. in the first seven days. Then in the next 14 days it loses another ·10, and ·01 for every following day, and by that sort of calculation they say they arrive at a figure of ·38 or ·40, which should be added to or allowed.

The Recorder: This witness says that is fallacious.

Mr. Gully : Fallacious altogether. It may possibly be right on one occasion, or nothing may be the right amount to allow, or on another occasion twice as much as they have calculated should be allowed.

Mr. Cottingham : Where my friend Mr. Gully got what he has just stated to the Court I do not know ; it never has been in evidence at all.

The Recorder : I only want to know what the argument is on the case for the respondents. I want to be fully informed of what their case is. I am not anticipating what the case of the other side may be.

Mr. Cottingham : Their case is that their standard is right and their method is right. My case is that they are both wrong.

Mr. Hopkinson : Assuming, with regard to any given sample of milk, that so much a day or so much a week was added for decomposition, would you say that the result might be wholly fallacious ? A. Certainly. I have known old and apparently decomposed milk give the same figures as at first, or very nearly the same figures.

Q. Even although the milk was apparently decomposed ? A. Yes. I have been surprised at the slight alteration that accompanied what appeared to be decomposition. I think it is quite hopeless to attempt any correction of this kind.

Q. If you had made an addition of so much per week according to any average standard for decomposition, you might have passed milk that was watered as much as 7 or 8 or 10 per cent ? A. Oh, yes.

Q. With regard to ash. Is ash a reliable test with regard to small amounts of adulteration with water ? A. No.

Q. It is a test when you get to very gross cases ? A. To show 50 per cent., but it will hardly show 20 per cent.

Q. When you are analysing samples of milk either to establish a standard or otherwise, do you yourself see the milk taken ? A. I take a very great variety of precautions of one kind or another. The samples that I have obtained from dairies were obtained many years ago, and I took what I considered efficient precautions.

Cross-examined by Mr. Cottingham :—

Q. In making your analysis you usually weigh the fat ? A. I do, and subtract the weight from the total solids.

Mr. Cottingham : You weigh the total solids first ? A. Yes.

Q. Then you extract the fat and weigh it. Then you deduct the fat, and that gives the weight of the solids not fat ? A. Yes.

Q. Do the different constituents of milk vary, or are they in genuine milk supposed to be constant ? A. The fat in real milk varies enormously. The solids not fat rise from 9·3 to sometimes 10 $\frac{1}{2}$, but they never go down.

Q. They never go down below what ? A. I believe they really never go down below 9·3.

Q. In arriving at your standard of 9 per cent., is that founded upon a number of cases and taking the mean—the average ? A. 9·3 is the mean, and is obtained from an immense quantity of work—my work ; and it is all the work that I could lay my hands upon.

Q. In point of fact that 9·3 is the mean or average of a great number of cases ? A. The mean probably of many thousand tons of milk.

Q. May we take it that some of the instances from which this average was taken must have been below—showing a lesser quantity than 9·3. A. Very slightly below.

Q. But still there were instances ? A. There must have been.

The Recorder : I am not intimate with these matters, but same must have been below and some above.

The Witness : Very slightly below.

Mr. Cottingham : Milk may be perfectly genuine, yet even by your method yielding less than 9·3. A. Yielding 9·3 or very slightly below 9·3. To allow for that we take the limit of 9.

Q. In your analyses do you use ether or benzoline ? A. I always extract with ether.

Q. Your process is that which is adopted by the Society of Public Analysts ? A. It is the process almost universally adopted.

Q. Do you mean to say that you have never found in any sample of genuine milk, non-fatty solids to fall below 9 per cent ? A. I have done so many analyses that I cannot answer Yes ; but this I can say, that I know of no such case where I should attribute the difference to anything but experimental error.

Q. I want to know whether you have found in your own analyses, or whether you will say you have never found in your own analyses, a single instance of genuine milk produce less than 9 per cent. of non-fatty solids ? A. Certainly I have never found that.

Q. How has it come down from your standard of 9·3 to the minimum of 9? A. It has not come down to that as a minimum, the standard is 9·3—that is the true figure, but we admit a limit of 9. The limit of 9 is really invented by the Society of Public Analysts. 9·3 is the standard used by me, and 9 was accepted by me as the limit.

Q. Except that it is adopted by this body, called the Public Analysts' Society, is it adopted by any other society in England? A. There is no other society in England that the question would come before, as far as I know.

Q. Has it ever been to your knowledge officially adopted by Somerset House? A. I have no knowledge of what they do at Somerset House. I have never been in the laboratory.

Q. It is only adopted by this body of Public Analysts? That does really all the work that there is.

Q. You are not a Member of the Society of Public Analysts? A. I left it some years ago. I was Vice-President at one time.

Re-examined by Mr. Gully:

Q. I do not know whether you know that this same minimum has been actually adopted in the United States by law? A. I know that my book has had a sale in the States. I heard that it was reprinted in the States.

The Recorder: I want to ask you a question or two. What are these non-fatty substances? A. They consist of caseine, plus milk sugar, plus mineral matter—ash.

Q. Do the proportions differ according to whether the animals are fed upon produce from different soils? A. The relative proportions of milk sugar and caseine vary much—that is to say, one milk will contain more milk sugar than another, and one milk will contain more caseine than another. It curiously happens that when the caseine is low the milk sugar is high, and when the milk sugar is high the caseine is low. The solids not fat are more constant than either ash, milk sugar, or caseine.

Q. Does the mineral vary much? A. The mode of determining the mineral matter as we do it is in the percentage accuracy not very accurate. There are only 7 per cent. of mineral matters in milk.

Q. What is the reason why milk from one soil will produce better cheese than another? A. The amount of fat is very variable in milk.

Q. Not the amount of caseine? A. No, the great variable is the fat. Cheese consists of fat to a great extent. It is a popular error that it is caseine alone. The curd is fat and caseine together, and it is the curd that makes the cheese.

Mr. CHAS. ESTCOURT RECALLED.—Further cross-examined by Mr. Cottingham:

Q. As to these samples that you have taken from the cows, since this case commenced, in Cheshire and elsewhere. How were those cows fed—were they highly fed cows? A. They were out at grass, and some were stall-fed upon brewers' grains.

Q. Some of them were stall-fed. What proportion of the samples were taken from the stall-fed cows? A. I do not know that the grass-fed cows would be. I presume they would be fairly fed.

Q. You saw the cows? A. Yes.

Q. Were they all beasts in high condition? A. I should not say so, they were fair average cows I suppose. In some cases they might be better cows than in others.

Q. That is really not an answer. You saw some of the samples taken yourself? A. A large number of them.

Q. As to the samples that you saw taken yourself, you can speak to the condition of the cows from which the samples were taken in your presence. Were they all well-conditioned and well-fed cows? A. They were all in fair condition.

The Recorder: Is it your experience that highly-fed cattle produce better milk than others? A. It is not.

Mr. OSWALD WILKINSON, sworn.—Examined by Mr. Gully:

Q. Are you a chemist by profession in Arcade Chambers, Market Street, and Public Analyst for Stockport; were Lecturer on Chemistry at Owen's College for two years? A. Yes.

Q. Did Mr. Wardle, the appellant, bring you a sample of this milk? A. He brought me a sample which was numbered 204, which I presumed to be the same.

Q. When did he bring it? A. On the 27th of April.

Q. Was it at that time in a state fit for analysing? A. Certainly.

Q. There was no decomposition? A. Not that would affect the analysis. It was slightly acid and somewhat curdled.

Q. Did you analyse it by the same process generally that Mr. Estcourt described? A. A similar process, but different inasmuch as I weighed my fat.

Q. A similar process except that you weighed the fat? A. I weighed the total solids and also the fat, but not the solids not fat.

Q. The same process, but you weighed the fat in addition? A. Identical, with that exception.

The Recorder: Mr. Wanklyn and this gentleman weighed the fat; the other gentleman weighed the solids.

Mr. Gully: He weighed it by the same process, but with the added precaution of weighing the fat.

The Witness: That is so. I did not weigh the solids not fat. I obtained those by difference. I weighed the total solids, including solids not fat.

Q. Did you ascertain by the process that has been described what was the quantity of solids not fat and of fat? A. Yes, I got the solids not fat by difference. There were solids not fat, 8·66 per cent.; fat, 2·86, obtained by weight.

Q. Does that in your opinion indicate adulteration by water—that is the presence of water not naturally in the milk? A. Certainly.

Q. To what extent? A. To the extent of about 4 per cent. I have 3·8 in my report because I certified the exact percentage I got from my calculation, but I should say in round numbers 4 per cent. if not more.

Q. You were doing that as you say at Mr. Wardle's request. Had you any communication whatever with Mr. Estcourt in the matter till long afterwards? A. I could not say how long, but a considerable time.

Q. Is it the process you would always use for the purpose of seeing whether milk had been adulterated by water? A. Yes.

Q. As far as your experience goes is it the safest, and is it the process generally used? A. Yes, by Public Analysts.

Q. Do you think that a test of milk 21 days' old can be fairly compared with a test of the same milk taken while it was fresh? A. I do not think so. I think the results obtained would simply be approximate.

Q. Would you place any reliance upon such a test if you found that you had before you two or three tests taken while the milk was fresh? A. No; I should give the fresh ones the preference decidedly.

Cross-examined by Mr. Cottingham:

Q. You got this sample on the 27th April—that would be five days after Mr. Estcourt's sample?

A. I received mine on the 27th at 4.30 p.m., that would be three days.

Q. Was the milk at all sour? A. It was slightly; it was decidedly acid.

Q. Was there incipient decomposition? A. No; I do not think so.

Q. Did you treat the milk first with any alkali? A. None whatever.

Q. Do you consider the milk in that sour state is as favourable for analysis without alkaline treatment as when the milk is fresh? A. I think there would be no appreciable difference in the result of the analysis. I say that it is not less than 3·8, possibly more.

Q. You cannot arrive at this deduction from the amount of non-fatty solids. That does not give you the absolute determinate quantity of adulteration; it is merely approximate to it.

The Recorder: He does not say that at all, and it is no good trying to make him say it. What he does say is that the minimum is 3·8.

The Witness: It is 3·8, and I hold to that statement.

Q. As there is a maximum and minimum, the quantity of adulteration is not fixed absolutely in any of these cases? A. That depends upon what standard you take. The milk may have been very rich milk, and then it is much more adulterated.

Mr. Cottingham: Was not the fat in this milk rather above the standard? A. No, rather below—2·86 per cent.

Q. Do you know the standard of the Public Analysts' Society for fat? A. 2·5 is the lowest minimum of the Public Analysts' Society; but I believe in genuine milk it is higher.

Q. It is quite clear from this analysis of yours that the total solids were rather above the Society's standard?

The Recorder: It is not a question of standard. You keep altering the term. He gives you a limit, and he gives you a standard, and you keep calling the limit the standard.

Mr. Cottingham : Your total solids were 11·52 per cent? A. Yes, and the fat 2·86. I say that the minimum limit of the Public Analysts' Society is 2·5, but that is very low ; it is taken as the lowest limit. I should say that 3 is low.

Mr. OTTO HEHNER, SWORN.—Examined by Mr. Hopkinson :

Q. I think you are a Fellow of the Chemical Society, and you are Public Analyst for South Derbyshire ? Yes.

Q. Did you analyse a sample of milk sent to you by the defendant in the month of April? A. I should like to ask the Recorder first whether I am obliged to give evidence on the subject. I got this sample from Mr. Wardle, and I consider, until I am obliged to give this evidence, that it is the property of Mr. Wardle. I am subpoenaed by the borough to give property which does not belong to me.

The Recorder : I do not think the law allows any privilege in this case.

Mr. Cottingham : On behalf of Mr. Wardle we do not make any objection.

The Recorder : You are quite right not to wish to give it, because it is to some extent confidential, but at the same time there is no privilege known to the law in your case.

The Witness : I got a sample of milk which I was told came from Mr. Wardle on the 28th April of this year, and analysed it on that day ; the milk was in such a condition as to be capable of being properly analysed.

Q. What was the number of the sample. Was there a number on it? A. The sample was labelled "City of Manchester, Food and Drugs' Act, 1875 : Sample No. 203." I declared the sample to be adulterated to the extent of about ten per cent. ; but I explained in my certificate that the sample was sour when received, hence it is impossible to ascertain very accurately the extent of added water. Decomposition had not advanced sufficiently to interfere very seriously with the result of the analysis.

Q. What was the amount of solids not fat found by you? A. 8·29, and fat 2·71.

The Recorder : Surely I have made a mistake. It cannot be 8·29. A. Yes—solids not fat. Then fat 2·71, total 11·00.

Mr. Hopkinson : What method did you employ? A. Substantially that one which has been several times spoken of by other witnesses. I weighed the solids not fat. I should say that the method is not exactly that which has been used, but it is substantially the same. I have had a good deal of experience in analysis of milk.

Q. What is your view with regard to the possibility of arriving at the original composition of milk from an analysis of decomposed milk? A. You can never arrive with the same certainty at any result, and when a certain point has been reached in decomposition it is an impossibility.

Q. I suppose the rate of decomposition depends upon a very large number of circumstances. A. Very many circumstances.

Q. Weather and heating? A. Yes ; temperature, time, air, bottling, and a great many circumstances.

Mr. Cottingham : Decomposition depends upon what? A. The temperature, the season of the year, the amount of water, the manner in which it is filled and bottled, whether the bottle is filled entirely or only half full, and many little circumstances of that kind. I would undertake myself to fill from the same sample two bottles, and the one bottle shall decompose at the rate of 2 per cent. per week, and the other shall not decompose at all ; that is to say I can keep a sample a week at will, or can cause it to decompose very rapidly according to circumstances.

Mr. Hopkinson : Do you think it is possible to make a calculation according to the length of time the milk is kept and add that to the result of your analysis as a mode of arriving at the composition of the milk? A. You cannot.

Q. Supposing you employed the process described by Mr. Wanklyn, what should you say was the minimum standard for solids not fat? A. 9 per cent. is perfectly fair.

Mr. Hopkinson : Is the Wanklyn process, or one that is substantially the same, one that is regularly adopted by Public Analysts in England? A. Yes, generally ; and the Society of Public Analysts includes pretty well every analyst in England ; and other analysts have scarcely ever milk to analyse.

Q. What is your opinion with regard to ash as a mode of arriving at the amount of adulteration by water. A. You cannot use it for ascertaining the exact amount ; but it has some value in connection with other estimation. If the ash is very low, lower than it could possibly occur in natural milk it would help in forming an opinion.

Q. Milk sugar is soluble in water? A. Yes.

Q. Is the effect of prolonged heating to some extent to alter the composition of milk sugar? A. It does. If you take we will say a pound of milk sugar pure and dry, and heat for some length of time, it will lose in weight about 7 or 8 per cent. It loses what is called water of crystallization.

Q. Accordingly if you make a milk standard or a minimum standard of milk with the method described in Dr. Bell's book, you would arrive at quite a different result from that which you would get by Wanklyn's process? A. You would naturally arrive at a lower result.

Cross-examined by Mr. Cottingham:

Q. You will not say that that lower result is not an accurate result? A. It is not a question of accurate result; it is a question of getting the result under certain conditions.

Q. Your standard of 9 per cent. is obtained under certain conditions? A. And is accurate for those conditions.

Q. This milk you say was sour. Did you estimate the amount of acid? A. I did.

Q. What was the amount of acid? A. 51 per cent. of lactic acid which had been generated by decomposition.

Mr. Cottingham: You are of opinion that this standard of 9 per cent. is rather too high? A. Not at all.

Q. Allow me to draw your attention to your own paper. "It appears to me that as much more concordant results are obtained when the solids are dried to constant weight than for three hours only, and that as the fat is much more completely, readily, and with a less amount of trouble extracted in an extractor such as Soxhlet's, it would be well to discard the old plan, and accordingly to lower the limit of solids not fat from 9 to 8.5 per cent." A. It was my opinion then and is now that it would be better to alter the process and to alter the limit; but the limit is good for the process, and if you alter the process you must alter the limit.

Q. Do you say that the Wanklyn process or that the Somerset House process is the best? A. I give no opinion upon the Somerset House process.

The Recorder: They are both good processes? A. I do not think so. I think one is a bad process.

Mr. Gully: Tell us which is bad by all means.

Mr. Cottingham: You think that the Somerset House or Government process —— A. It is not a Government process. I am a Government official just as much as the Somerset House people are. It is not a Government process at all.

Q. It is the process adopted by those who analyse for the Government? A. No; we analyse for the Government also. It is the process adopted by those gentlemen.

Q. What do you mean by saying "It would be well to discard the old plan." What was the old plan you allude to? A. The Wanklyn process. The plan that is in use is the old plan.

Q. That is the Wanklyn plan? A. Yes.

Q. You have advocated discarding Wanklyn's plan and you suggested the lowering of the solids not milk from 9 to 8.5? A. In connection with the process.

The Recorder: What is the alteration of process? Give it to us shortly. A. Without putting my opinion against Mr. Wanklyn's, which is more valuable than mine, I would prefer to dry till the solids cease to lose weight—to dehydrate the milk sugar, and extract the fat in a manner which I consider more convenient than Mr. Wanklyn's manner. It is only a difference in manipulation.

Mr. Cottingham: You prefer not to leave off drying until you get to a constant weight? A. I think it would be better.

Q. In fact that was the result you came to by this long table of experiments we have here? A. Yes. Re-examined by Mr. Gully:

Q. What is it that you object to in Dr. Bell's process? A. I object to the manner in which the fat is extracted. Mr. Bell does not only extract fat, but he extracts other things which he adds on naturally to the fat, or rather the fat appears larger by his process than it is, and in consequence the solids not fat appear smaller than they are actually.

Q. In his analysis by his process the fat appears larger than it is really at the expense of the solids not fat? A. Yes, that is so.

Q. That has nothing to do with the mere applying of the heat, has it? A. No, that is in the extraction of the fat.

Q. By what means? A. By means of ether. It is well known that pure ether will only dissolve the fat, but as soon as ether contains water, as it must do in Bell's process, the sugar of milk is dissolved in addition, and I have no doubt also mineral substances.

Q. In his process when he comes to dissolve the fat, he puts water with the ether? A. Yes.

Q. You say that the effect is not merely to take away the fat, but something else? A. Yes. Some of the solids which go away with the fat is weighed with the fat. That renders it uncertain for the purpose of testing the solids not fat. These are the important things to test for in testing for adulteration by water.

Q. Whether you dry by the process which you recommend (which I understand is not Bell's process, but a process of heating longer and more), or whether you apply the process which you did apply in this case are you equally satisfied that this milk which you tested was adulterated by water? A. Entirely. I should say that I did not know anything about the statement of the Public Analyst when I made my report. It is a perfectly independent report. I was inclined to be in favour of my client if anything.

Dr. A. DUPRÉ, F.R.S., SWORN.—Examined by Mr. Gully:—

Q. Are you Professor of Chemistry at Westminster Hospital, and employed by the Home Office, and by the Medical Department of the Local Government Board, and Public Analyst for the Westminster District Board of Works? A. I am.

Q. Wherever you have reported adulteration there has been a conviction upon it? A. In every case.

Q. I want to know what process you have followed in those cases, and what do you consider the best process. A. I adopted substantially the process described; but I also weighed the solids not fat. I think it gives easier and more accurate results.

Q. Do you consider that applying that process, milk below 9 is adulterated? A. I have no doubt about it in my own mind.

Q. Is that the principle you always act upon as a Public Analyst? A. I always make my calculation upon 9·3, but I would not report against a milk if it contained 9.

Q. But if less than 9? A. If less than 9 I always report against it.

Q. Do you consider that an analysis taken of milk when it is three weeks old can be safely compared with an analysis of the same milk when it was fresh? I would not pay any attention to the one three weeks old. I think it is perfectly useless.

Q. Do you find in fact, in your experience, that where milk is sent out less than 9 per cent. of solids that it can be brought up to over 9 again? A. I do not know, but I notice this: whenever my inspectors have not been round for a few months the milk in my district sinks down to 9 and a little below frequently; but after they have once been round, and go round again, the next week the milk invariably goes up to 9·3 and 9·4. It has never yet been otherwise.

Q. From time to time when you or your inspectors are active, the milk can always be brought up to 9·3? A. Yes. If they go round on week days it is 9·3. If they occasionally go round on Sundays it is below 9.

Q. You know Dr. Bell's work? A. Yes, I do.

Q. I daresay you have read the tables upon which he bases his views in that book? A. I have.

Q. Are those results which you say are from properly taken samples accurate? A. I do not think so. I say that these tables demonstrate that Mr. Bell's process is not accurate; it is demonstrated to be inaccurate by the tables he puts forward; because the most easily taken figure, and the one which is generally most accurate—the specific gravity of milk, depends especially upon two factors—upon the solids not fat and upon the fat. The solids not fat raise it, and the fat depresses it; but if you have the total solids and the fat you can always calculate—or even if you have the specific gravity and the total solids you can always calculate the solids not fat from the specific gravity with a considerable degree of accuracy. If you look over these tables they are most extraordinary. There is no relation whatever between the fat and the solids not fat, and the specific gravity. You sometimes get as much as 1086 sp. gr., and instead of giving you milk with more solids it actually gives you milk with less.

Q. Does that confirm you in the opinion expressed by the last witness, that by his process he deducts from the non-fatty matter and actually adds it to the fat? A. Yes, he takes more fat than is really present, and sometimes he adds apparently very much to the fat, and sometimes he adds a little to the fat.

Q. And you say that that is shewn upon these tables to a great extent? A. To a very considerable extent.

Q. And that could not be if the process were accurate? A. It could not be.

Cross-examined by Mr. Cottingham :

Q. Have you recommended prosecutions in a great number of cases ? A. I never recommend prosecutions. I only give my certificate. I have nothing to do with prosecutions. I only know when a prosecution has taken place.

Q. How many prosecutions can you call to mind in which you have given a certificate of 4 per cent. added water ? A. As it happens I have not amongst the 320 samples of milk a single one which is 4 per cent.; I have one or two 5 per cent. They are generally either above 9 or very much below 9. I give them as adulterated to the extent of 5 or 10 per cent. The great majority have more than 10 per cent.

Re-examined by Mr. Gully :—

Q. In this case the fatty solids are low ? A. Yes.

Q. Were there prosecutions in those cases you spoke of where the adulteration was 5 per cent.? A. Yes, and no protest generally : there was some explanation why it must be so ; either that the milk had been left standing in the rain, or that the milk had ran short and they were obliged to buy some, or something like that—clearly indicating to my mind a knowledge that it was adulterated.

Q. Do you put it in round figures or decimals ? A. Always in round figures—about so much. I calculate from the 9·3. I would say that this milk was adulterated to the extent of 7 or 8 per cent. of water

Mr. GEORGE WILLIAM WIGNER, SWORN.—Examined by Mr. Hopkinson :

Q. I think, Mr. Wigner, you reside in London, and you are President of the Society of Public Analysts ; and have had great experience in the analysis of milk ? A. I have.

Q. What should you say was the fair minimum standard, if you employed the process that has been described by Mr. Wanklyn ? A. I fully agree with 9 per cent. as the limit, but I invariably calculate upon 9·3 when adulteration is once found.

Q. Do you think that anything lower than 9 would be too low ? A. Anything lower than 9 would allow watered milk to pass ; in fact 9 frequently allows watered milk to pass.

Q. Then taking the figures given by Mr. Estcourt as accurate, would you in your judgment say that this milk was watered to the extent of at least 4 per cent ? A. If the sample had been brought by one of my inspectors I should have certified to an adulteration of 7 per cent.

Q. What is your view with regard to the possibility of arriving at an accurate analysis of decomposed milk ? A. It is almost useless when the decomposition has got to such a stage that there is a cheesy smell in the milk ; and it is very uncertain, even when it has not got so far as that.

Q. As regards any specific sample of milk, can you say that the original composition of milk could be arrived at by making an addition to the analysis of decomposed milk ? A. No, it could not.

Q. Would an addition that might be right in one case be totally misleading in another ? A. The addition would have to be regulated by so many different circumstances that one specific correction cannot be applied. A most material thing in altering the rate of decomposition is that watered milk decomposes at a very different rate to genuine milk.

The Recorder : Faster ? A. Generally faster.

Mr. Hopkinson : From analysing milk that was decomposed and three or four weeks old, could you possibly arrive at the composition of the decomposed milk ? A. You might by accident come somewhat near the truth, but there would be no certainty.

A. You would not venture to give a certificate that milk had not been watered after analysing it when it was three or four weeks old ? A. Certainly not.

Q. Supposing you were analysing decomposed milk three or four weeks old, would you be incapable of pronouncing an opinion as to its original composition ? A. In some cases I might be able to say that it had been watered, but I should never be able to say that it had not been watered. If it had been watered to the extent of 50 per cent. I could tell that.

Q. Could you, after such a lapse of time, detect a small amount of water ? A. No, certainly not.

Q. Accordingly you would not certify under such circumstances that milk had been watered or not, unless the amount of added water were very large indeed ? A. If the amount of water was very large it would be possible to find it.

Q. Have you looked at the tables given in Dr. James Bell's book ? A. I have.

Q. I think in the first column he gives the specific gravity, then he gives the amount of solids not fat, and then the amount of solids which are fat. As Dr. Duprè has told us, the specific gravity of milk is higher if the solids not fat are large, and lower if the solids which are fat are large ? A. Yes.

Q. The specific gravity of milk varies according to the amount of solids which are not fat, and inversely as the solids which are fat ? A. Yes.

Q. Looking at the figures given for the specific gravity of milk in those tables, and the amount of solids which are fat and which are not fat, are the results possible? A. They are quite impossible; they are quite incomparable with anything ever done in analysis of milk before. The tables are such as could not have been obtained by any accurate process from any samples of milk.

Q. Look at Table V. The first column gives the specific gravity, then you have the solids not fat, and then you have the solids fat. Take two of those and compare them. Can you find cases in which the specific gravity differs largely where the fat is constant, and yet where the solids not fat are not as they would be, judged from the specific gravity? A. The figures in the different columns do not tally one with another.

Mr. Cottingham: Which figures do not tally? A. About half way down on page 20, there is a figure of 1028·35 in the specific gravity column, and as against it there is 10·42 of non-fatty solids and 5·66 of fatty solids.

Mr. Hopkinson: That is a low specific gravity is not it, and a very high amount of non-fatty solids? A. A very high amount; a perfectly abnormal amount.

Q. Is the high quantity of fat enough to account for the discrepancy? A. No. Then about the fifth or sixth from the bottom is another one. The specific gravity is 1035·56.

Q. That is a very high specific gravity? A. A very high specific gravity. Then there is 9·71 of non-fatty solids and 4·13 fat.

Q. Are those two cases possible to be both true? A. They are not at all comparable.

The Recorder: I have followed everything up to this, but I do not follow this.

Mr. Hopkinson: Very shortly it is this. The specific gravity of milk is large if the non-fatty solids are large. The non-fatty solids are heavier than water, so that if the amount of non-fatty solids is large the specific gravity is large too. That being so, one would expect where the specific gravity is 1028·35, which is a low specific gravity, that the non-fatty solids would be very low also; instead of that we find them in this example very high. On the other hand, 1035·56 is a high specific gravity for milk; yet we find in that case where the specific gravity is high, that there is actually a lower amount of non-fatty solids.

The Recorder: Now I understand.

The Witness: It is physically impossible that the analysis has been properly conducted, unless the difference in the fat was sufficient to account for it.

Mr. Hopkinson: Mr. Wigner has told us that it is impossible for the difference in the fat, which is small, to account for the discrepancy. The fatty solids, if high, rather reduce the specific gravity, so that what we say is, that on the face of this table, as Dr. Duprè has said, and as this gentleman says, it is quite impossible that these results, in this standard, here arrived at, can be accurate. On the face of it they are demonstrably wrong.

Q. Have you taken other samples which shew the same thing? A. I have taken out several other examples of the same kind. In fact the next line above the first I mentioned will illustrate it again. There you have 1033·60 specific gravity, and there are others which shew the same.

Cross-examined by Mr. Cottingham:

Q. About the question of specific gravity. You say the specific gravity depends upon the amount of non-fatty solids? A. And fatty solids.

Q. The non-fatty solids are composed of sugar of milk, caseine and mineral ash? A. Yes.

Q. May not those constituents differ amongst themselves in the same milk? A. They do to some extent. When the amount of caseine increases, the amount of milk sugar generally decreases.

Q. That surely would influence the specific gravity of the mixture? A. It would influence it but very slightly.

Q. You have been criticising these analyses of Dr. Bell. Have you made any experiments yourself to justify what you have been saying? A. I have made more than 30 analyses during the last fortnight according to Dr. Bell's process. The process you must know was never disclosed till the last hearing of this case. It has been a secret process during the last eight years: it has never been known to anybody.

Q. What are the experiments you have made on which you found your attack on Dr. Bell's method? A. I have taken 30 recent samples of milk which I have analysed in my laboratory by the Wanklyn process, and I have analysed those samples side by side by Bell's process.

Q. Have you made allowance in your experiments for the variation in the non-fatty solids amongst themselves? A. There is no allowance needed, because I have taken the same milk side by side for the two processes.

Q. Have you, in the course of your experience, never found genuine milk which had less than 9 per cent. of non-fatty solids? A. I have seen genuine milk from a single diseased cow below 9 per cent., but I have never seen it from a herd of cows.

Q. Have you never seen it in any case other than that one case? A. I have seen it in other cases with foot and mouth disease.

Q. Have you never, except in case of cows having foot and mouth disease, found genuine milk with less than 9 per cent. of non-fatty solids? A. I have not.

Q. I am going to quote now from Dr. Bell's book, page 27. What do you say to this statement of Mr. Dyer. You know Mr. Dyer? A. I do, very well.

Q. What do you say to this remark on page 27 (*ANALYST*, vol. vi.)? I say that Mr. Dyer did not see the cows milked.

Q. "The foregoing analyses illustrate what has frequently been pointed out before—that stall-fed cows give richer milk than cows at grass, even when supplied with additional food in the shape of oil cake, and they give good examples of the great variations to which the milk, even of individual cows, is subject." Do you admit that that was stated? A. Yes.

Mr. Cottingham: What do you say to this? "In a third instance, Dr. P. Vieth stated that in a herd of 120 cows in Raden, in Germany, the average yield of non-fatty solids, for the years 1879-80, fell in most cases between 8·5 and 9·0 per cent., and that they never rose above 9·0, but fell occasionally below 8·5 per cent. In the case of individual cows the non-fatty solids varied, as a rule, from 8 to 9 per cent., but they sometimes fell below 8·0, and in a few instances they rose above 9·0 per cent. At Kiel, the average of the milk of 10 cows was as follows:—

| | |
|----------------|----------------------------------|
| "In 1878 | Non-fatty Solids, 8·73 per cent. |
| 1879 | " " 8·71 per cent. |
| 1881 | " " 8·53 per cent." |

Have you any reason for impeaching the authenticity of that statement?

A. The analyses which are spoken of there are not made according to the Wanklyn process. The fat has been extracted in a totally different way.

Q. What process were those made by? A. They were extracted in a Soxhlet apparatus. They were, in addition, mixed with sea-sand, and pulverized in a mortar before the fat was taken out.

Q. What would be the effect of the sand upon the non-fatty solids—would it increase or diminish the amount? A. When milk is dried down by the Wanklyn process with a given sample, you get 9 per cent. of non-fatty solids; and by the other method, 100 grains of milk is put into a platinum basin to be dried down, and you put in 500 grains of sea-sand, and carry out the analysis in that way the solids would come out 8·6 and about 2·9 of fat.

Q. I should like to know what difference in the ultimate result the use of sea-sand makes?

A. That is exactly what I have been trying to say to you, and you would not let me. Instead of getting 9 per cent. of solids not fat, you get 8·5, or 8·6.

The Recorder: Why should the process you have just described produce a less amount of non-fatty solids than the process which is generally used now? A. Because the sand process would insure the extraction of the very last trace of the fat, and in fact a little of the milk sugar with the fat, and that would be counted as being all fat; whereas, by the process carried out by Mr. Wanklyn, it is always admitted that we leave a small portion of fat not extracted from the milk. In that same paper Dr. Vieth says: "I am fully aware that those figures just communicated to you cannot be compared directly with figures obtained by Public Analysts, as our methods of analysing differ." I am reading from the same paper.

Mr. Cottingham: Then the use of the sand is for the purpose of drying? A. It is used really for the purpose of making the extraction of the fat more complete.

Q. The use of the sand is simply to assist in the extraction of the fat? A. Yes.

Q. How is the milk sugar brought out by the sand? A. It is dissolved out by the ether.

Q. That is the way you explain how the use of the sand interferes with, or alters the weight of, the non-fatty solids? A. Yes.

Q. By which of the two methods—Wanklyn's method, or the other—do you extract the greatest quantity of fat? A. The sand method.

Q. In the method that you have been speaking of do you use ether? A. Yes.

Q. That is for the extraction of the fat? A. Yes. The method will be perfectly familiar to all the gentlemen behind you. The sand is put in the Soxhlet apparatus and boiled for several hours.

Q. Now here is another quotation from THE ANALYST of April, 1882,—“At Proskau, in 1879, the average of non-fatty solids was 8·42 per cent.”? A. That is a portion of the same paper.

Q. Yes. “Dr. P. Vieth further stated, as the result of 18 months' experience in England, that 9·0 per cent. as a standard for non-fatty solids is too high? A. I find that in Dr. Bell's book, and I think it is a most unfair quotation. It is on page 27, and it is a quotation taken out without taking the context with it, where he says: “I am fully aware that those figures just communicated to you cannot be compared directly with figures obtained by Public Analysts, as the methods of analysing differ.”

Mr. Sutton: It is the truth, but not the whole truth.

Mr. Cottingham: “At the dairy experimental station at Kiel, ten cows are kept exclusively for the purpose of making experiments.” This is a paper read before the Public Analysts' Society by Dr. Vieth, on the 15th March, 1882. He speaks of his researches at Raden, then at Kiel, and he brings out the result of his experiments at Kiel thus (*See ANALYST*) :—

| | | |
|-------------------------|------------------------------|--------------------|
| “In the year 1878 | Total Solids 12·43 per cent. | Fat 8·70 per cent. |
| ” 1879 | ” 12·13 per cent. | ” 8·42 per cent. |
| ” 1881 | ” 11·93 per cent. | ” 8·40 per cent. |

“The solids not fat generally fall between 8·5 and 9·0 per cent.”

Q. What do you say to this? A. I say that the whole of it is done by a different process; therefore it is not comparable with our 9 per cent. standard. I quite agree that from that process the standard would have to be lowered from 9 per cent. to 8·5. As we have no intention of changing the process, we cannot change the standard, and all that will not apply.

Mr. Cottingham: I have just one more question to ask you. Is the process suggested by this writer, Dr. Vieth, the best process or not? A. It is not in my opinion. It is not in Dr. Vieth's opinion.

Re-examined by Mr. Hopkinson :

Q. He is Analyst for a Dairy Company? A. Yes, he is Analyst for a Dairy Company.

Q. In that paper Dr. Vieth is speaking of the best way to get out the whole of the fat? A. Certainly.

Q. I suppose the Somerset House process, or the Soxhlet process, is a good way of getting out all the fat? Is it as good a way as the Wanklyn process or better? A. The Soxhlet process will get out more fat.

Q. Therefore certain things appear as fat which ought to appear as non-fatty solids? A. Yes.

Q. And therefore as a standard? A. It would be too low to be applicable to any other process.

Q. Have you by experiment yourself tried whether the use of that process or the Somerset House process does in fact take out something which is not fat, and which is weighed as fat? A. I have tried both. Soxhlet's method I have tried many times, and sometimes a considerable proportion of the non-fatty solids—milk sugar, is in fact brought out.

Q. And that appears in the analysis as though it were fat? A. Yes. I have tried the Somerset House process during the last three weeks, and I assert that something like 10 per cent on the average of what is extracted, when that process is strictly carried out, is not fat, but milk sugar.

Q. Have you tried a number of samples and analysed them by both the Wanklyn process and the Somerset House process? A. Yes, about 20 samples.

Q. As the result of those analyses, which method do you think is the better method for arriving at the amount of solids not fat? A. I do not think that any two persons can work alike by the Somerset House process, and I do not think it will give you reliable results.

Q. The same milk may give different results in different analyses by the Somerset House process? A. Yes, that I found by actual experiment.

Q. Have you found the Wanklyn process, with the same milk, always give the result? A. Not exactly the same, but a man who understands the work properly would not make a difference of more than one-half per cent. of water.

Q. The Wanklyn process substantially 'gives constant results'? A. You have it here in three different analyses by different men, by the Wanklyn process, unknown to one another; the water does not differ more than .2 per cent.

Mr. Cottingham: One goes up as high as 10 per cent. of adulteration. There are not two who agree.

The Witness: I purposely omitted one—the 10 per cent.—that is a fourth.

Mr. Hopkinson: If you used the Somerset House process for a number of samples, would you be sure that that standard was too low? A. I do not think you could possibly take that for founding a system upon. The Somerset House process could not possibly be taken for founding a standard upon.

Q. Is the reason of that, that in the Somerset House process, or the Soxhlet process, you take out as fat a great deal that is not fat? A. That is part of the reason; but I think two more reasons should be pointed out. The instructions given for the Somerset House process are not definite instructions as to dryness.

Mr. Cottingham: Pardon me, I must object to this. This gentleman cannot possibly tell what instructions are given at Somerset House.

The Witness: I am referring to Dr. Bell's printed book. I will alter my answer by saying Dr. Bell's process, if you like.

Mr. Hopkinson: You take the instructions as to time given? A. I take the instructions as to time. I say it is not a specific drying down to a certain point for which instructions are given; the instructions are that it is to be dried only to a pasty condition. There are no two of us in this Court, even chemists who would agree exactly as to what a pasty condition is. Then if that condition is altered ever so little, the amount of milk sugar extracted would be altered.

Mr. Hopkinson: I have more witnesses whom I might call, but I only propose to call this next gentleman, Dr. Blyth.

Dr. ALEXANDER WINTER BLYTH, SWORN.—Examined by Mr. Hopkinson:

Q. I think you are Medical Officer of Health and Public Analyst for Marylebone? A. I am.

Q. Do you think the Wanklyn process is a substantially fair one for arriving at whether milk is adulterated or not? A. I do.

Q. If that process is used, what should you say is the proper minimum standard to adopt for the non-fatty solids? A. A safe limit is 9. I have always held that it is too low; but still I think it is a safe limit to work with, and I work with it. According to my individual experience it is too low. I have never found a healthy cow give milk so low as 9 although I work to that limit.

Q. As applied to the analysis of milk of a dairy, would you say Mr. Estcourt's method being used that milk had been watered if the non-fatty solids fell below 9—could you say so safely? A. Yes.

Q. I think you have actually written a work on the subject of milk analysis, and you have paid great attention to the subject? A. I have.

Q. With regard to analysing decomposed milk, can you obtain any trustworthy results from it? A. Only under certain conditions; under ordinary conditions you certainly cannot.

Q. Would you say, that adding to the actual results of your analysis so much for loss by decomposition per week would bring you to any accurate results? A. No, that would be most unjust; because I have found from experiment that, if pure drinking water is added to milk, the decomposition is very much less than if water containing sewage contaminations is added to milk. There you get a different growth altogether; you get different microscopic appearance, and the growth is very much more rapid.

The Recorder: The growth of decomposition? A. Oh yes. The growth of microscopic organisms are the cause of decomposition.

Mr. Hopkinson: If there were an average, would that lead to grossly inaccurate results as to a particular specimen? A. Yes.

Q. If the original composition of milk is sought to be arrived at by an analysis of the milk when decomposed, and an addition is made to it of so much per week for loss by decomposition, would you say that the result was untrustworthy? A. Certainly.

Cross-examined by Mr. Cottingham :

Q. Do you mean to say that you cannot safely analyse any milk after a certain number of days—how many days? A. I could not state the time, and I never said that. Of course if the adulteration is very large you can tell even in putrid milk.

Mr. Cottingham : What do you say is the interval of time from the milking of the cow within which a sample should be analysed—what is maximum interval? A. I could not say at all. It may be very great under certain conditions; in cold weather or in an ice-house it might be analysed a year after.

Q. Say in the months of April and May. How many days do you say might intervene so as to leave a sample in a sufficiently reliable condition? A. It is impossible to say, unless you tell me the conditions under which that sample is kept.

Mr. Cottingham : I think, Sir, it would probably be the most convenient thing for me to call my witnesses, and then address you afterwards. Mr. Gully does not object to that course.

Mr. RICHARD WARDLE, SWORN.—Examined by Mr. Ferguson :

Q. You are a farmer at Weston Underwood, and the appellant in this case? A. Yes.

The Recorder : Where is Weston Underwood? A. In Derbyshire, about six miles north-west of Derby, near Kedleston.

Mr. Ferguson : Does the morning milk and the evening milk go at the same time to Manchester? A. Yes, both at the same time.

Q. I suppose it leaves your premises in the same state as it comes from the cows? A. Exactly.

Q. Do you superintend the dairy arrangements yourself? A. Generally.

Q. You never put water in the milk? A. There is not a drop put in.

Q. Nor do you allow other people to put it in? A. I always order them not to do. I have always given strict orders that none should be put in.

Q. At some farms they rinse the cans out with a liberal allowance of water? A. That is the case very often, but we do not do it with ours.

Q. Did you see this milk sent off, about which this complaint was made? A. Yes, I did.

Q. Were your cows at the time in the fields, or kept in the sheds? A. Altogether in the sheds.

Q. It is not a good time of the year for the milk? A. It is generally considered a very poor one. It is generally weaker at that time of the year as far as our experience goes.

Q. Why is that? A. I really cannot tell. I know that it is a result, so far as our observation goes with cheese-making. We can always make a very much greater amount of cheese in the autumn than we can during the spring months, from the same quantity of milk.

Q. I suppose it has something to do with the food? A. Yes, and then the period of the year—the milk is not supposed I believe to be so good just after calving, and cows calve just about that time of the year.

Q. I suppose at that time of the year you eat up the remains of the winter food. A. Yes, and food has not been good at all during the last few years—during these wet seasons.

Q. Wet seasons make a difference? A. A very great difference in the fodder.

Q. And consequently in the milk. I believe you are a Member of the Farmers' Society? A. There is a sort of association in Derbyshire.

Cross-examined by Mr. Gully :

Q. How many cows have you? A. We vary a little.

Q. How many had you in April? A. 33 or 34.

Q. In how many cans would their milk be put in the morning? A. Two at that time.

Q. Do you mean that there would be the milk of 16 cows put into one can? A. Something like that. There were two full cans.

Q. It would represent an average of about 15 or 16 cows—each can? A. Yes, I suppose so—something of that sort.

Q. You have had complaints about your milk from Mr. Halewood? A. Mr. Halewood wrote to me in January. That was the first and only complaint I had from him.

Q. Did you see him? A. No.

Q. Did he shew you an analysis he had got? A. He wrote and told me he had had an analysis made.

Q. Did he tell you that he had had the milk analysed and that he had found that it was adulterated with 7 per cent. of water? A. I do not know whether he named the amount. He said it was adulterated, that he had had an analysis made and that there was so much water in it. I do not remember the amount.

Q. Was that in January? A. In January.

Q. Was it in January that you stopped what you called rinsing? A. After I got that note from Mr. Halewood.

Q. You never began it again? A. I never began it again at all. I may say that the rinsing was about half a pint at the end of the milk.

Q. Did Mr. Halewood complain or speak to you in April? A. Yes, but he never made any more complaints to me.

Q. There used to be some water put in up to January? A. Yes, just as I tell you.

Q. He complained and said that he had got an analysis shewing that there was an adulteration with water? A. Yes.

Q. Then you stopped the rinsing, and his complaints stopped? A. He did not complain afterwards.

Q. How many men do you employ about the cows? A. One with the cows directly—only one that attends to the cows. As to milkers, there are four—three men and a boy.

Q. When the milk has been got, is it poured into the refrigerator? A. It is poured out of one

Q. Through the refrigerator? A. Over one.

Q. Does that refrigerator consist of a winding pipe and worm, with cold water in it? A. It is a straight bar like *this*, something [meaning the bar round the witness-box] with water running through the inside.

Q. Is there a tap at the bottom of that? A. Yes, there is a tap to allow the water to come in. It comes in at the bottom and gradually goes up to the top and goes over the top.

Re-examined by Mr. Cottingham:

Q. You say there was this trifling addition of water from the rinsing. What is the rinsing?

A. Supposing you had milk in a vessel, we put say half a pint—that is usually the case.

Q. A half pint of water? A. A half pint of water.

Q. To clear out the milk at the bottom of the vessel? A. Yes.

Mr. Gully: Mr. Ferguson said "a liberal allowance."

Mr. Cottingham: What do you say is the contents of the vessel into which you milk? A. About three gallons.

Q. Then there would be a certain amount of milk left in this vessel? Yes, hanging round the side.

Q. For the purpose of washing it out you put in how much? A. About half a pint.

Q. You rinse it and then put that into the churns for sending off? A. Yes.

Q. After you had this complaint from Mr. Halewood you desisted from that? A. Yes; there was not a drop of water put in.

Q. You never had a complaint after? A. No.

Q. You told my friend you saw this milk taken from the cows and sent off yourself, and you were present during the whole of the time? A. Yes.

Q. So that no water could have been added without your knowledge? A. There could not.

Q. You positively swear there was none? A. I do.

Q. How soon was the milk sent off after the milking? A. Immediately.

Q. You saw the cows milked, you saw the milk sent off, you were present the whole time, and you swear there was no adulteration with water? A. I do.

Dr. JAMES BELL, SWORN:

The Witness: Seeing that in our position we are perfectly neutral as between the defendant and the other side, and seeing that there are grave charges made against us, and that various criticisms have been made upon our various processes, perhaps your Worship, instead of allowing either Counsel to examine me, will allow me to meet all the points that have been brought forward without any direct examination.

The Recorder : Long experience in Courts of Justice teaches us that that is not the best way.

The Witness : I have not supplied material to either counsel.

The Recorder : I dare say not.

Mr. Cottingham : You are subpoenaed by both sides ? A. Yes, I am.

Q. You are subpoenaed by those who instruct my friend, and you are brought down here on the part of the magistrates ? A. Yes.

The Recorder : Keep to your leading questions. It is a mere matter of form. You must examine the witness.

Examined by Mr. Cottingham :

Q. You are the Principal of the Laboratory at Somerset House ? A. I am.

Q. How long have you been in that position ? A. I have been now ever since 1874 or 1875. I was then appointed Principal. I was Deputy-principal before that.

Q. Had you been in Somerset House in any other position before you were appointed chief ? A. As Deputy-principal of the Laboratory.

Q. How long have you been in the Laboratory altogether ? A. Practically in the Chemical Department since the year 1852.

Q. Under the Food and Drugs' Act you were appointed referee ? I was.

Q. You have examined, I suppose, a great variety of articles for the Customs' Board, the Board of Admiralty, and samples of adulterated food ? A. Yes.

Q. At the request of the Magistrates for the City of Manchester, did you analyse two samples of milk sent up to you in this case ? A. Yes, I did.

Q. Nos. 203 and 204 ? A. Nos. 203 and 204. In the case of 203, the non-fatty solids were 8·20 and 2·82 of fat, but they slightly differ in the certificate I think. Those are the results I have averaged in pencil from the book. I do not know whether it corresponds within $\frac{1}{10}$ or $\frac{1}{100}$ with what you have.

Mr. Gully : 8·20 and 2·80 is what we have ? A. Yes, they were done in duplicate. In the second case, No. 204, the non-fatty solids were 8·04 and 8·01. I suppose it will be about 8·02 in the certificate !!

Mr. Gully : And 3·01 fat ? A. Yes, and fat 3·01. So that here we have 8·20 of non-fatty solids in No. 203, and 2·80 fat, making together 11·00, and to that we added $\frac{1}{10}$ for loss by decomposition, making together 11·38. In the other case the non-fatty solids were 8·01 and the fat 3·01, and adding $\frac{1}{10}$ to that makes 11·40 of total solids. Now in the case of No. 204, it will be noticed that Mr. Estcourt made the total solids 11·43, and on the hearing of the case before the magistrates I was perfectly ignorant of the result of Mr. Estcourt's analysis, when I stated that our allowance for loss through decomposition was $\frac{1}{10}$, so that we practically agree within a few hundredths with the result obtained by Mr. Estcourt, and in the other case a similar agreement occurs.

Mr. Gully : I think not ; it is between 11·00 and 11·21 ? A. Then with regard to the scale of allowance, that is founded on a long series of carefully conducted experiments ; and from those experiments we have deduced the ordinary amount of decomposition, or loss that occurs through decomposition, in the samples by keeping—and our scale is founded upon those results. That method is perfectly scientific, and a similar arrangement occurs, for instance, in the determination of the specific gravity of beer upon which our Board pay a drawback of over half a million a year ; and the mode in which the scale was determined was founded upon actual experiments in that case ; and the system or principle is exactly similar and analogous to the principle that we have adopted in the present case for making these allowances on kept milks.

The Recorder : If I understand you aright, with the addition of .38 per cent. for decomposition, you do arrive practically at the same analysis as Mr. Estcourt arrived at without making any allowance for decomposition ? A. Yes, quite so.

The Recorder : If that is so, that part of the case becomes unimportant.

Mr. Gully : Except upon the question whether the addition is a thing of any value.

The Recorder : If they both arrive at practically the same analysis, then the result must depend upon whether the amount alleged on the one hand to prove adulteration is conclusive proof of the adulteration or not.

Mr. Gully : Except this—that they do not arrive really at the same analysis. The analysis of No. 204 is 8·02 as against 8·62 ; and, in order to make the two correspond, Mr. Bell adds on a figure to represent an allowance, which addition is no part of his analysis, but a figure taken, as he says, as the result of his experience, as the average allowance which should be added on in order to make decomposed milk 21 days old correspond with fresh milk. That is not part of his analysis.

Mr. Cottingham : Yes, it is.

Mr. Gully : I say it is not.

The Witness : I say it is part of the analysis.

The Recorder : I do not care. In the view I am taking at the present moment—I daresay it may be a wrong one—it does not seem to me to be important as to how he arrives at it. Supposing he is wrong in his analysis, you are wrong too.

Mr. Gully : I do not follow you.

The Recorder : If you both arrive, by whatever road, at practically the same conclusion, you are either both right, or both wrong.

Mr. Gully : No. By this process of his, and by our process we ought to arrive at different conclusions. The same figure does not denote the same milk if arrived at by the two processes.

Mr. Cottingham : But the results of the analysis are practically the same in both cases and by the same sets of analyses.

Mr. Gully : I say that, supposing Mr. Bell, with fresh milk, had produced the result of 8·58 of solids not fat, or 11·38 total solids, that would correspond to a higher figure with us.

The Recorder : Yes, but I suppose you are prepared to take your stand upon the analyses which you have made.

Mr. Gully : The fresh milk analyses.

The Recorder : Then you are agreed about that ?

Mr. Gully : I say that there are three fresh milk analyses which all bring out a figure which is inconsistent with the first figure of Dr. Bell, and Dr. Bell makes them consistent by adding on a figure which is not found in his analysis.

The Recorder : I quite agree with you ; but when you have arrived at this it does not signify, for the purpose of this enquiry, how you arrive at the conclusion, if you are all agreed that on the 24th April this milk had in it a certain amount of solids fat, and a certain amount of solids not fat. How does it signify upon this enquiry how the conclusion is arrived at ?

Mr. Gully : Because we say that Dr. Bell's 8·60, which he brings it up to, means a higher thing than our 8·60. I should be quite content if it were put that his 8·60 means no better milk than our 8·60 ; then I should be prepared to accept that.

The Witness : I am prepared to agree to that.

Mr. Gully : We are going upon the basis that I accept what Mr. Bell says. He says that by his additions he brings out the same result as Mr. Estcourt. But take for example No. 204. It is an important point. The non-fatty solids were 8·02. Adding Dr. Bell's ·38 to that makes 8·40 as against our 8·62, shewing that he does not profess that they are made to accord.

The Recorder : What he says now is that practically they have arrived at the same conclusions by different roads.

Mr. Gully : Decomposition would not destroy the fat. It is not the fat that would be destroyed by the decomposition ; it is the other materials, therefore the ·38 would go on to them.

The Recorder : Is that so ?

Mr. Gully : Is not that so, that the waste by decomposition would be in the non-fatty matters and not in the fat. A. Quite so.

Q. Therefore the ·38 would be put on the 8·02 and would make 8·40, and comparing that with 8·62 it would not bring them to the same figure ? A. Only Mr. Estcourt has got some fat in his non-fatty solids, which accounts for the difference. (Mr. Estcourt here denied that he used the same process.)

Mr. Gully : You cannot have your pudding and eat it.

Mr. Cottingham : You cannot have your fat and attribute it to our non-fatty solids. That is the mistake you make.

The Recorder : Mr. Estcourt says that his non-fatty solids amounted to 8·67. Dr. Bell says from his non-fatty substances he arrives at 8·20.

Mr. Gully : Dr. Bell says that a certain amount has disappeared. We say that is not correct.

The Recorder : I see now what I could not understand before. You are very nearly agreed as to what the non-fatty solids were when the milk was fresh. I do not see that there is much difference between you.

Mr. Gully : There is a considerable difference.

The Recorder : According to Mr. Estcourt, the non-fatty solids were 8·67.

Mr. Gully : Arrived at by his process.

The Recorder : According to Mr. Bell his calculation produced 8·58.

Mr. Gully : Assuming it were done upon fresh milk.

The Recorder : It seems to me a very small difference.

Mr. Gully : It is what our witnesses were going into in some detail. They say that their process ought always to shew in pure milk at least 9 or 9·3 per cent. of non-fatty matter and ; they say that if you apply to the same milk Dr. Bell's process, you would have as a result less than 9 or 9·3. You would have a smaller result upon the very same sample by applying Dr. Bell's process ; therefore the figures do not compare.

The Recorder : I understand that. Now, what I mean is : that you have both arrived at the conclusion I have just mentioned, whatever your processes may be. It seems to me that you are placed in this difficulty ; that if you shew that Dr. Bell is wrong, you have to shew that you under-estimated the non-fatty solids.

Mr. Gully : If Dr. Bell accepts our view that no pure milk ought to have less than 9 per cent. of non-fatty solids, those figures prove our case.

Mr. Cottingham : They do not indeed. The Recorder is perfectly right.

Mr. Gully : He will be glad to hear you say so, Mr. Cottingham.

The Recorder : If you shew that Dr. Bell's process does produce a less amount of non-fatty solids than your process does, then no doubt you would be able to shew that this milk is better than you make it. That is all.

Mr. Gully : No, it does not come to that.

The Recorder : It does.

The Witness : Most certainly it does.

Mr. Gully : Even allowing the ·38 to be added, it is not so.

The Recorder : Assuming at the present moment that the figures come to be the same, then if you prove that Dr. Bell's process of analysis of the same milk produces a less quantity of non-fatty solids than the Wanklyn process, then you will have proved that this milk was better than Mr. Estcourt says it was.

Mr. Gully : I think not, for this reason—Dr. Bell's 8·20, speaking somewhat roughly, would, I believe, correspond to the 8·67 brought out by our process.

The Recorder : That seems to be so.

Mr. Gully : I agree in that. I submit if that were so, then this would shew a result got out by him of 8·67 or 8·58 by our process. I am leaving out the ·38 altogether, though I agree it is a most important question. The two points upon which I rely are these—first of all that pure milk cannot shew less than 9 per cent. of solids, not fat, and secondly that you cannot rely at all upon the analysis of a decomposed specimen of milk.

The Recorder : I perfectly understand. With regard to the second point what I am now saying is. Why need you care about whether Dr. Bell's analysis is comparatively worthless or is valuable, if it produces the same results as you arrive at?

Mr. Gully : If it does, I quite agree. Why need I,—but I should like to know what Dr. Bell's evidence is before I say that.

The Recorder : Do you follow me ?

The Witness : Quite so.

Mr. Gully : If he says that this milk when fresh was only worth 8·67 even if tested by our process.

The Witness : Our results agree with yours.

Mr. Cottingham : The results are the same. The scientific conclusions to be drawn from those results are *toto cælo* different.

The Recorder : I understand quite and am prepared to give my decision upon it if necessary. That question of decomposition appears to me not to be a question of value now in this appeal, as I understand the case at present.

Mr. Cottingham : It never was.

The Recorder : Let us have it perfectly clear, because these subjects are perhaps almost as new to me as they are to you, so we had better have no misunderstanding. What I understand is this : that Dr. Bell practically does not differ in his analysis of this milk from Mr. Estcourt.

Mr. Cottingham : Except in the process used.

The Recorder : In the analysis.

Mr. Gully : If he does not, and if he accepts this—that this milk when fresh, tried by Wanklyn's process, produced only 8·67, then that is all he is asked to admit about it. Then I say, further, that that is the proper process.

The Witness : Your Worship, I agree as to the figures. The learned counsel is wrong in saying that the process used by Mr. Estcourt is Wanklyn's process. It is not. I say that he has practically lapsed into our drying to a constant weight (Mr. Estcourt here demurred to this statement); therefore we agree in our results. That is the explanation of it.

Mr. Gully : If it be so, perhaps you will let the conviction stand at once.

The Recorder : I am sorry to interrupt you so often, but this is quite a novel kind of question to me. Mr. Wanklyn and Mr. Estcourt I daresay might arrive in ninety-nine cases out of a hundred at the same result, but they do adopt a different process in one particular : one of them weighs the fat and the other weighs the non-fat, and they deduct the other weight ; but the conclusion they would come to would nearly always be the same ? A. Yes.

Mr. Gully : I will call it Estcourt's process. According to our evidence, the thing is the same for all practical purposes. The admission that we should like to have, if Dr. Bell is prepared to go so far, is this : that testing by Mr. Estcourt's process—which I shall ask you to say was for practical purposes the same as Wanklyn's—testing properly by that process, when the milk was fresh, the analysis shewed that the solids not fat were 8·67. That is the first point. Then I should ask, further, that where you find that result taken by that process it shews an adulteration, in so far as it shews a result less than 9 per cent. of solids not fat.

The Recorder : Yes, I quite understand it. That point Dr. Bell does not agree with.

The Witness : That is the second part of the question.

The Recorder : There has been a good deal of evidence about that first point, but that point disappears now, and the time has not been at all thrown away.

Mr. Gully : Do not let me for a moment mislead you in this. I do not say that the other process of Mr. Bell by which he adds on that allowance for decomposition is correct. I think when you hear the rest of Mr. Bell's evidence in which he will question our process, you will find that that question is material.

The Recorder : I can understand it being a most interesting question, but I do not see that it affects the matter now.

Mr. Gully : If, when Mr. Bell's evidence is over, you say it does not affect it, I will not say anything.

The Recorder : Then I will discharge both of you from any further argument with regard to the process by which the parties mutually arrive at the analysis which was made by Mr. Estcourt, and which is admitted now on all sides to be substantially correct. Now the point in question is, whether that analysis proves in criminal courts beyond all reasonable doubt that there must be water in the milk.

Mr. Cottingham : That really is the ultimate question.

Mr. Gully : I quite agree.

Mr. Cottingham : Of course you know what Wanklyn's analysis is ? A. Yes, I have stated so.

Q. I presume you have resorted to it upon certain occasions and you rejected it ? A. Yes, we first tried Wanklyn's process most religiously. We tried to work it, but we found it varied so in the same

sample done in the same way that we did not continue it. It varied from 2/10ths to 8/10ths of difference—I believe that I am not overstating it, and I think that in Mr. Hehner's paper which has been read before the Court to-day it will be found that the range is nearly the same.

Q. From .2 to .8? A. At all events from .3. I remember it varied from .3 to .8. The great difficulty was to dry samples always to the same degree of dryness, in the three hours—in other words, to dry off the same amount of moisture from the milk in that time. Sometimes a film will get over the top of the milk when it is put over the water-bath, and so on, and that will interfere with the evaporation. Hence we adopted the other process—that is, to dry the non-fatty solids to constant weight, and we determine the fat as well as determine the whole of the constituents. The reason that the difference arose was this: that if you put two quantities on the water-bath—that is, equal quantities of milk in the capsules, and then at the end of three hours you removed them, and extracted the fat from them, you might practically get the same result or the same quantities of fat from each; but when you deducted it from the total weight which you ascertained in each case, at the end of three hours there would be a difference which varied from 8/10ths to 8/10ths; consequently, seeing the uncertainty of getting the evaporation carried down to the uniform scale or quantity always, we were obliged to abandon the process. I have no doubt that is what is suggested entirely in the spirit of Mr. Hehner's paper, of which I entirely approve.

The Recorder: Now will you tell me, in popular language, what is the process that you adopt, which is, you say, a better process? A. We always make our experiments in duplicate. We weigh out two quantities, they are put on the water-bath until they attain near dryness, not quite—not quite so much as if evaporated for three hours, but until the moisture is practically gone or really gone. We then take and treat them with pure ether, and extract the fat from the total solids.

The Recorder: Are you certain when you extract the fat from the total solids that you do not extract some other solids at the same time? A. Quite so, because we are most careful. After the fat is separated and dried, we are most careful to dissolve the fat with dry ether, and ascertain whether any portion of the non-fatty solids has been dissolved out besides the fat. That is the invariable practice; so that we prove absolutely that we extract nothing from the total solids except fat. Then, having separated the fat, we put the non-fatty solids in the bath, and we dry them to constant weight.

The Recorder: What is the meaning of that? A. That is to say until they cease to lose weight. Then we get them dry. The fat is treated in the same way. We do not determine one constituent and deduct it from another, but we determine the whole of the constituents, and the two added together ought to make the total solids.

The Recorder: Whereas Mr. Wanklyn after his process weighs the non-fatty solids? A. The fat.

Q. And then deducts the weight from the other, and whereas Mr. Estcourt weighs the non-fatty solids and then deducts it from the other, you weigh both? A. Yes.

Mr. Cottingham: And compare the sum of the weights with the total solids? A. Yes.

Q. So that by that means you furnish a test for the accuracy of your analysis? A. Yes.

The Recorder: When you weighed the two together and then deducted the one, did you practically ever find any difference between that, and the weight of the two together? A. Not if the total solids are properly dried.

Q. Did you ever practically find that they had not been? A. With sour milks there is a difficulty in getting them to agree exactly; but the results are within practical agreement.

Q. Then there is no advantage in weighing each? A. We have to be extremely careful in arriving at reliable results—results that we can defend and produce to the court.

Mr. Cottingham: Do you think it would be safe to simply weigh the total solids and then weigh the fat, by whatsoever means extracted, and deduct the weight of the non-fatty solids? Would that be without any check of weighing the two? A. I have explained to his Worship, that by doing that you have no evidence whatever as to whether the water has been entirely expelled from the milk—no check whatever.

Q. In fact you would have no check, and if you have duplicated your experiment you may repeat a mistake? A. Yes, there may be a repetition of the error, or it may be greater.

Q. There can be no mistake if you weigh the fat and weigh the non-fatty solids, and if the sum of the two weights equal the weight of the total solids? A. Quite so.

Q. That is a crucial test.

The Recorder: Have you often to make use of the double weighing in your calculations. A. We make use of it in every sample.

Q. Do you find that it is often of use? Does it ever produce different results? A. Sometimes a difference of 1/10th; that is within the limits of an error of experiment between the two methods.

Mr. Cottingham: Would not a very small error in the amount of the solids cause a considerable error in the calculation of the amount of adulteration by water? A. I do not see the point exactly.

Q. From a certain amount of non-fatty solids Mr. Estcourt infers the presence of 4 per cent. of added water. Supposing Mr. Estcourt, for want of the test you have mentioned, went wrong in the weight of the solids, would that cause a considerable difference in the amount of added water? A. I understand his Worship has decided that question, and that we have gone from it. I understand your Worship that we agree ——

The Recorder: Do not say that I have decided. It is a conclusion I have arrived at, that you do agree.

Mr. Cottingham: You agree as to the analysis, but not as to the conclusion to be arrived at from it? A. Of course, our certificate shews that.

Q. Do you consider that the weighing of the fat in the manner that you have described, after drying it, is very essential in coming to a proper conclusion as to the amount of the solids? A. Of course; if we did not we would not do it.

Q. Now after having analysed the milk in the manner in which you have described, have you found anything in the milk which is not perfectly consistent with genuine milk? A. Oh, no; it is perfectly consistent with a sample of genuine milk.

Q. You can find nothing that indicates adulteration? A. If we had we would have stated so. Of course we are perfectly unbiassed in that respect. An attack has been made upon our Tables ——

The Recorder: I was coming to that afterwards for my own satisfaction, but I thought I would leave that for the present. I should like to hear what is the explanation given of the difference between the specific gravities.

Mr. Cottingham: Perhaps you will explain that now before we get further? A. It was a very interesting matter, and we made several experiments on the subject. On page 11, the last paragraph, you will find I have dealt with the subject. I state "An indirect method of arriving at the percentage of fat and non-fatty solids was suggested by Mayer & Clausnitzer, and recently a modification of their formula for calculating the result has been proposed by O. Hehner. The method is based on the accurate determination of the specific gravity and total solids of the milk, and the application to these of certain experimental data derived from the specific gravity of the fat and non-fatty solids. The theoretical results, however, which are calculated from even the modified formula proposed by Hehner, are in most instances too high in the non-fatty solids, and to the same extent too low in the fat; but the amounts are sufficiently near accuracy, especially in the case of samples of average quality." There is the point of difference. I found a considerable agreement always when they were samples of average quality, but not when they deviated from samples above or below average samples. If your Worship will turn to page 20 and refer to the two cases that were pointed out by Mr. Wigner, 1028·35 the specific gravity, and 10·42 the non-fatty solids, and 5·66 of fat, your Worship will see at once that that is a sample far above the average, both in non-fatty solids and in fat. The fat is 5·66 and the non-fatty solids 10·42.

The Recorder: Let me remain at that. What he says is, that it is unreasonable to assert that milk, the specific gravity of which is 1028·35, should have so large a percentage of non-fatty solids and of fatty solids. He says it is unreasonable to suppose that such a thing with such figures as those could co-exist? A. When it is worked out according to the method laid down by Mr. Hehner, the result does not correspond with the results given in this table; but I say that this is not an average milk. The non-fatty solids 10·42 are very high, and the fat 5·66 is very high; and therefore I should expect a considerable deviation.

Q. Then what he says is, that if it is good milk the specific gravity ought to be higher? A. No, because it contains nearly six per cent. of fat, which reduces the specific gravity. The more fat, the lower the specific gravity of the milk.

Q. Where did you get these analyses on Table V.? A. Those are all milks that were carefully collected. I deputed one of our gentlemen to go to different parts of the country, and see the cows milked. He brought these samples up direct to the laboratory to us, and they were analysed. Those are the results of the analyses of the samples we obtained ourselves from the dairies under the different farmers.

Q. Take the other instance, the 1035·56. That is a high specific gravity? A. Yes, and there the non-fatty solids are 9·71 and the fat 4·13. There the fat is not so high as it is in the other case where the specific gravity is 1028·35.

Q. Although the figures are surprising, you still think they are not so surprising as to suggest any doubt to your mind as to their being correct? A. I think it will be shown presently by Dr. Voeleker that they are correct. He has shewn me two instances of his own, and the results are quite as abnormal as these are, or at least differing as much from the ordinary averages.

Mr. Cottingham : These specific gravities, and the solids put opposite to them, are not the results of theory but what you have ascertained by actual analysis? A. Yes. I have told his Worship that the whole of the samples in this Table V., also those in Table VI., are of our own obtaining, and can be authenticated as genuine milk.

Q. As authenticated facts? A. Yes; the gentleman who did it was one of the officers of the Board, and therefore he was a responsible person.

The Recorder : A perfectly responsible person and an intelligent person might make a mistake, but you do not think those are mistakes? A. I do not.

Mr. Cottingham : Would you come to this conclusion with regard to Mr. Wigner's theory—do you say it does not apply to the extreme or limit cases? A. Quite so. We find considerable variation.

Q. And you say that these instances here are facts outside his theory? A. Yes, I have stated to his Worship so.

Q. Supposing that in the analysis of this milk you had proceeded on Wanklyn's mode, would you or would you not have obtained a higher amount of fatty solids?

The Recorder : We have disposed of that?

The Witness : We have disposed of that. The question now, as I understand, is whether milk containing 8·6 of non-fatty solids—whether the milk in the present instance is adulterated.

Mr. Gully : We are not agreed.

The Recorder : I agree with you, Dr. Bell, about that.

Mr. Gully : I put it as I did before——

The Recorder : I was merely simply saying that the question for me is whether milk, the analysis of which is like this, must necessarily be adulterated or not.

Mr. Gully : By analysis obtained by a certain process—that is all essential. A different process upon the same sample will produce different results.

The Recorder : I am assuming that your process upon this sample is a correct one. Somebody else by another process has arrived at the same result.

Mr. Gully : It is enough for me——

The Recorder : I do not say that the process is a correct one. I do not go so far as that; but I say that the process you adopted has brought you to the same conclusion.

Mr. Gully : As to actual contents?

The Recorder : Yes.

Mr. Cottingham : The real question between us is this: Assuming that both sets of analysts arrive at the same results, are the conclusions from those results the same, and if not which is correct?

The Recorder : Yes.

Mr. Cottingham : In the analysis of this particular milk do you bring the amount of non-fatty solids within some of the instances in your own table? A. Yes. In the case of individual cows—that

is in the tables as published here—nearly 40 per cent. of the samples fall below 9 per cent. of non-fatty solids, and in the case of dairy samples nearly half of them fall below 9 per cent. of non-fatty solids. Analysed more minutely, in Table V., there are 14·9 per cent. under 8·6.

Q. Begin at page 22. Table V. commences at page 20. I say that 14·9 per cent. of the samples fall under 8·6 of solids not fat. 28·9 per cent. are over 8·6 and under 9·00, and 46·00 per cent. 9, and upwards. The variations in the non-fatty solids range in the tables from 1 per cent. up to 11·27 per cent.; and the fat ranges from 1·92 to 6·87. There is only one sample so low as 1·92.

Q. Where is that? A. That is on page 22, the last line but one. Your Worship will notice that that is a sample which would have passed the standard of the Public Analysts so far as non-fatty solids are concerned.

The Recorder: I do not understand your view about that.

Mr. Cottingham: He is speaking now of the fat.

The Recorder: I do not understand for what purpose you mention that? A. Simply the range—to point out to your Worship the variations that occur in the various constituents of milk.

Q. What you meant to show was that in some milk the weight of fat and non-fatty solids differ very much? A. Yes.

Q. This you mention as an extreme case? A. Yes.

Mr. Cottingham: And that, notwithstanding the high specific gravity? A. We have passed that. Then in the case of dairy samples the range of non-fatty solids is from 8·5 to 9·91. That is taken from Table VI.

The Recorder: Then what were those other samples? A. Individual cows. The others are dairy samples. As to those, since this case was heard before the Magistrates I have looked over the samples in our books as to the places from which we obtained them. I notice that we obtained some from Draycott, Keddleston and Duffield. At Draycott, taking individual cows, the non-fatty solids were 8·6, the next one 8·97, the next 9·09, the next 8·5, the next 8·95, the next 9·12.

The Recorder: You say they are lower than the general average of the country? A. That was at the end of March, and we should have expected at that time that there was not much grass; and any grass there would be moist, and that necessarily affects the character of the milk.

Q. Is that, or not, considerably lower than the average? A. No, I think these results somewhat correspond with the results in Table V., taking them as a whole. Then at Keddleston the non-fatty solids were—8·64, 8·35, 9·03, 9·59, 9·93 and 8·82; and the average of 17 cows at Keddleston yielded 8·70 of non-fatty solids, and 3·21 of fat.

Mr. Cottingham: Many of those samples—if not all—are from the neighbourhood where the defendant has his dairy? A. Yes. The average sample in the dairy samples stands about 6 down the Table VI.

Q. This Wanklyn standard of 9 per cent. was fixed a great many years ago? A. Yes. I think it was fixed about the year 1874, or so.

Q. Was that before the passing of the Adulteration Act of 1875? A. It was.

Mr. Cottingham: Do you consider fat an important ingredient in the analysis in coming to your conclusion? A. Yes.

Q. In fact you consider all the constituents—their proportion to each other? A. We do. We take the whole of the constituents into account in dealing with the sample.

Q. Did you find in this milk the normal proportion of constituents to each other? A. Yes, quite the constituents of genuine milk.

Cross-examined by Mr. Gully:

Q. Do you adopt my friend's phrase, "normal proportion"? There was rather an excess of water, was not there? A. I cannot say there was an excess of water.

Q. I am right in saying that this does not shew the normal proportions of solid matter to water? A. The range in the variations of the various constituents of milk are so great that this falls quite within it.

Q. You would get at an average? A. It is below the average.

Q. Then it is not the normal proportion ; it is below ? You rely upon the Table ? A. I rely upon the Table as the result of experiments and investigations.

Q. Were all these analyses your own ? A. They were all made under my own superintendence.

Q. For the purpose of experimenting to see what was the standard ? A. For the purpose of ascertaining or investigating variations in the composition of milk.

Q. A number of these results are very abnormal ones, are not they ? A. They are wide—the range is very wide.

Q. Fat 1·92 is very out of the way ? A. It is low.

Q. Leaving this book out of the question altogether—if someone brought you a specimen of milk containing only 1·92 of fat, would not that raise strong suspicion in your mind of skimming ? A. If a Public Analyst reported a thing of that kind I should consider the case one in which the defendant ought to prove that it was genuine milk.

Q. You would not think it unreasonable for anyone to come to the conclusion that there had been skimming ? A. No, I think that is fair and reasonable.

Q. The same with a great many of these low figures for non-fatty solids ? A. Yes, when you go below 8·5 I think there should be some evidence on the part of the defendant that the milk is genuine.

Q. Take for example the third item on page 22. The specific gravity is 1027·05. That is a low specific gravity, is not it ? A. Yes. It is poor milk. It has only 8·00 per cent. of non-fatty solids.

Q. It is a low specific gravity, and a very low amount of non-fatty matter—8·00 only ? A. Yes.

Q. That is very low ? A. We have had lower, only I have not included them. I thought it in the public interest not to do so.

The Recorder : 8·00 is the lowest I see here ? A. Yes.

The Recorder : You must assume it is abnormal ? A. Yes.

Mr. Gully : Do you say that was genuine milk ? A. Yes, I do.

Q. You are quite sure that was genuine milk ? A. I have no doubt whatever at all about it.

Q. Would you pass milk that was brought to you for analysis like that ? Supposing the Court sent up to you, at Somerset House, a sample to analyse which contained only 8 per cent. of non-fatty matter, would you pass it ? A. No, I should not. As I say, I consider that in all these cases the defendant ought to be called upon to shew that the milk was genuine.

Q. Supposing you found non-fatty matter 8·00 and fat 2·31, would not you certify, if that sample were sent to you, that it had been adulterated ? A. If it were represented as a dairy sample.

The Recorder : I suppose what you mean by that is, that the combined milk of 16 cows, producing non-fatty matter, 8·00, and fatty matter 2·31, would be so astonishing that you would not believe it ? A. Quite so.

Mr. Gully : The 8·00 alone would be quite enough, would not it ? A. Yes, we should not pass it.

Q. If that were sent up to you as a specimen without your being told that it was milk from a single cow or from a dairy, would not you refuse to pass that, and say that it had been adulterated ? A. Yes, I daresay we should ; but I may remark, that in cases of this kind, where it comes on the border line, I have invariably written to the clerk of the magistrates to ask some particulars as to the history of the sample.

Q. What is the lowest that you find in your dairy samples ? A. 8·50, I think.

Q. After adding this .38 in this case you only bring this up to 8·58 ? A. Yes, I think that is so.

Q. 8·50 is the lowest of the dairy samples, and is somewhat abnormally low ? A. It is a low sample of course.

Q. Would you pass milk at 8·50 ? A. If the sample of milk in every respect afforded evidence of being a genuine sample we should pass it.

Q. What do you mean by that ? Supposing a sample like this were sent up to you containing 8·50 of solids not fat, would you pass that as a dairy sample ? A. It is a very general question, because we take the fat into account.

Q. Does that affect the question of adulteration by water ? A. Of course it does. That is just the difference between the Public Analysts and us. We take the whole of the constituents into account. We have every desire to support the Public Analysts as far as we can, but we have always to consider the others as well. If it goes below a certain point, I say that the defendant ought to be called upon to shew that it is a genuine sample.

Q. You have to certify—that is the duty you have to perform? A. We have to consider the results before doing that.

Q. I ask you, would you not certify that a sample had been adulterated, if sent up to you containing 8·50 per cent. of non-fatty solids? A. No; because there might be 4 or 5 per cent. of fat upon that.

Q. You would not do more than say that it was a suspicious circumstance? A. We should say that it was of low quality for a dairy sample.

Q. It would raise an inference? A. It might really be a very rich milk. If that contained 5 per cent. of fat it would be very rich milk indeed, very much richer than milk having 8 or 9 per cent. of non-fatty matter and 2·5 of fat.

Q. Then 8·50 you would pass? A. Yes.

Q. You would pass 8·4? A. That would depend upon the fat. If there was a good quantity of fat, or a reasonable quantity of fat, we should.

Q. Did not you say before that you would pass 8·4, and that you would not pass 8·3? I did not give the answer as it is stated there, nor may I give you an answer in the same form in which it is given there, because I qualify it. If it contained 8·4 of non-fatty solids and a fair proportion of fat, and the ash and other constituents were satisfactory, or shewed evidence of a genuine sample, we should pass it.

Q. I want to know if this is correct—"Would you pass it at anything under 8·5?" A. I should.

Q. Would you pass it at 8·2? A. No, I should not. Q. Would you at 8·3? A. No. Q. Nor at 8·4? Yes, if the other constituents were right."

Q. You draw the line somewhere between 8·3 and 8·4? A. If it comes below that point I say the defendant ought to be called upon to shew that the sample was a sample of genuine milk.

Q. Are those results as to non-fatty solids obtained by your process? A. They are.

Q. Take that one which by your process brings out 8·00. If, instead of testing by that process, you had tested in the way Mr. Estcourt had tested, would not that have brought out a larger figure. A. As I have stated from the beginning, by Wanklyn's process we might get 8·3 or 8·4.

Q. You would get a larger figure? A. You might.

Q. Would you expect a larger one? A. Yes.

Q. With less heating? A. Yes.

Q. With your system you apply more heat, and dry more? A. Yes, we reduce to constant weight.

Q. Then as to non-fatty matters, the results are not the same if you test a given quantity of milk by your method and by his process? A. Not if you strictly adhere to his process.

Q. Or by Mr. Wanklyn's process? A. By Mr. Estcourt's process you will get the percentage, because he dried to constant weight.

Q. He did not say so? A. He did.

Q. Not practically? A. Practically it was dried to constant weight.

Q. He said he dried for a certain time (three-quarters of an hour I think it was), which left only 5/100ths or 6/100ths of moisture.

Mr. Gully: You found 8·02 in one of those samples? A. In the Tables—yes.

Q. Take it by itself. Practically 8·02 is the same thing as 8·00, there is only .02 difference—practically we may take it that as low as the lowest, although it is a dairy sample? A. It is not one of the dairy samples.

Q. I say that the 8·02 is a sample from a dairy—it was the milk of 15 or 16 cows?

The Recorder: I think you are wrong, Mr. Gully.

Mr. Gully: I was saying that the sample which he produced, No. 204, showing 8·02 was a dairy sample? A. Yes, that is so.

Q. I will leave out of consideration the addition, or allowance you make for decomposition. That sample came out as low as the lowest of the samples from individual cows, and lower by .5 per cent. than the lowest dairy sample you have in your Table VI.? A. Quite so.

Q. That was the actual analysis, and you added something on for decomposition? A. We did.

Q. Assuming that that was correctly added on, even when you added that .38 for decomposition, you only bring it out 8·40, which is lower than the lowest dairy sample in your Table VI by .10? A. Yes.

Q. 8·50 is the lowest. There are two 8·50, one 8·62, one 8·70, and one 8·80? A. But there is over three per cent. of fat in that sample, which shows 8·02.

Q. There was 8·02 solids not fat, and there was 9·01 of fat in the sample you took of No. 204. In your lowest dairy sample in Table VI. there was 8·50 non-fatty solids, and 8·65 of fat—still more? A. Yes, we got over three per cent. of fat.

Q. What I am pointing out is that it is lower (even after you have corrected it), both in non-fatty substances, and in fatty substances, than the very lowest of all the dairy samples in Table VI.? A. It is only 1/10th.

Q. You made it as high as you did make it only by adding that ·38? A. Yes.

Q. How do you get at that ·38? A. By the results of experiments made as I pointed out at the beginning—we made an investigation.

Q. Is that an average? A. It represents on an average the amount of loss that occurs.

Q. Is that the average of figures which varied a good deal like Table V.

The Recorder : I do not quite see the value of this, Mr. Gully.

Mr. Gully : If this test is valueless by reason of adding on ·38, and there is no authority for doing it, there is then left only the 8·02, which would be admittedly bad.

The Recorder : You have not quite followed that which I thought was the result of the former part of the discussion, that by either of the scientific processes adopted by them they both arrive at the same conclusion, or they have arrived at the same conclusion by a happy accident. In either case both sides are agreed that the condition of the milk at the time when it was examined, was that which Mr. Esteourt has described. Then what does it signify how he has arrived at the result?

Mr. Gully : I submit that it is material in this way. We say that, tried by our process, this sample shewed solids not fat, 8·67. That, if it had been pure milk, would have produced at least 9·00; therefore it is bad. We say that this gentleman's process produced a lower result than ours, somewhat; and we say that in point of fact he did produce, by this analysis of No. 204, 8·02; and we say that if you are to take his analysis to check ours—which we deny, considering that we had other independent analyses made at the same time—if you are to take his analysis as a check against ours, then I say it is open to two observations. In the first place, we object to his method of doing it, which we say reduces the matter of solids, a fact which you will have present to your mind; and in the next place, I say that he cannot add anything to that 8·02, because it is a mere question of luck whether he hits the right figure or not. It may be quite true that it is the average of a number of results obtained with regard to the loss by decomposition; but you cannot tell where in that average this particular milk would stand. I say that when you have had the milk tested, while fresh, by scientific men who have agreed upon positive results, this gentleman cannot correct his figures by a mere average, which may not apply to this particular case at all. Supposing the average amount of loss by decomposition to be ·38, that may be the average between a loss ranging from ·001 to ·5. You may very well imagine a very large range. How can he tell what the loss was in this particular case. It may be a case in which the amount of loss was very small.

The Recorder : In my view, this at present has been proved—that this milk when analysed produced the figures which Mr. Esteourt has stated. The calculations made by Dr. Bell with regard to all the other specimens of milk, were made upon a different system from that which Mr. Esteourt adopts, and Dr. Bell's system would only produce a smaller amount of non-fatty substances, but so small an amount as to be almost inappreciable, where Mr. Esteourt's is said to be inaccurate.

Mr. Gully : I follow; but it seems to me that the importance of it is this. Here you have, as I was saying, an analysis taken at the time the milk was fresh and what my friend really relies upon in this case, is not merely Dr. Bell's critical observations upon our process, but on the fact that Dr. Bell made an analysis of his own, which he says bears him out in his opinion. I want to show that that ought to be set aside altogether, and if you tell me that you cast aside Dr. Bell's examination and analysis of this milk altogether, that you discard it from your mind, and attach no weight to it, I have nothing more to say.

The Recorder : I am not going to say that I discard it from my mind and attach no weight to it; but I so far discard it from my mind in deciding this case, that I think it is of no importance in the decision of this case at all.

Mr. Gully : Then I do not know that it would be any use for me to go further.

The Recorder : I do not say to a gentleman of Dr. Bell's eminence that I discard his evidence altogether. It would not be true, to begin with, but I do not think it is an element which will assist me in coming to the conclusion at which I shall have ultimately to arrive.

The Witness : Perhaps your Worship will allow me to say this : The evidence upon which we rely to shew that our method is not quite a rule of thumb, or an average is this :—the allowances are not invariable, as I pointed out before. Mr. Hehner in this case made the non-fatty solids 8·29, and he made the acid $\frac{1}{10}$. We find the acid only a few days afterwards $\frac{1}{10}$. He came down only a little lower than we did.

The Witness : I want to satisfy the Public Analysts, as well as the counsel for the prosecution, that we do not do things, even in making this allowance, altogether by rule of thumb. We have evidence in the sample itself. It is acid, and that increases according to the degree to which the decomposition has proceeded.

Mr. Gully : Mr. Hopkinson is with me to-day, and he was in the case when it was before the Court below : perhaps you will hear him upon this point, and why he thinks it is important.

Mr. Hopkinson : It is in this way—the two processes really arrive usually at different results. The Somerset House process usually makes the fatty substances rather more, and the non-fatty substances rather less than the other process, by reason as Mr. Wanklyn said, of a certain part of the milk sugar being dissolved and carried over with the fat. The Somerset House people have no doubt done their best to make a proper analysis, but their fallacy is this : they are trying to compare this analysis of Mr. Estcourt, made by Mr. Wanklyn's method, with a standard arrived at by their method. That is the fallacy.

The Recorder : It appears to me, if they stated that, that there would be a fallacy in it, but the difference is so very small that it does not signify.

Mr. Cottingham : My friend is in error.

The Recorder : What I understand is that Dr. Bell's process of analysis will give a different amount of constituent parts of milk from Mr. Estcourt's, but if Mr. Estcourt's analysis is accurately taken, the difference between the result of Mr. Estcourt's analysis and Mr. Bell's is so small that it does not matter.

Mr. Hopkinson : It applies here—Dr. Bell gets his standard from an analysis of fresh milk, applying his own process. He analyses a sample of this milk by his own method, but plus a certain rule of thumb, which our witnesses have proved, may be totally inapplicable to the sample. He may have arrived at a result tallying with ours after applying that rule of thumb, but he arrives at a totally different result when he sets up the correct standard of milk.

The Recorder : It appears to me that this difference is one of those small somethings that it does not seem possible to give much weight to in a question of this sort. I admit it exists.

Mr. Hopkinson : Of course neither of those methods may be quite accurate in the amount of non-fatty solids they arrive at ; but according to the Somerset House people, they say that our method leaves too much in the non-fatty solids, and we say that their method leaves too little.

The Recorder : I quite admit that that would be a matter of very great nicety, but it does not appear to me that the difference is such that I could decide what is practically a criminal case upon it, if it means that.

Mr. Hopkinson : We put it rather in this way. We have proved that 9 is the lowest minimum according to our method. Dr. Bell's evidence does not touch that for a moment. He does not say that for Wanklyn's method 9 per cent. of solids not fat is not the proper standard ; he only says that by another method that is not a proper standard.

The Recorder : If I understand Dr. Bell rightly, he says that 9 per cent. taken correctly by the Wanklyn method is too high a standard.

The Witness : That the result is not accurate.

Q. Do not you also say this : that although there may be variations in Estcourt's method, yet where it is accurate, it produces 9 per cent. of solids not fat from milk, it is still possible that that milk may be unadulterated ? A. If Wanklyn's process is followed exactly, the probability is that it will be

accurate, or within certainly a few tenths of the method followed by us ; but I gather that in the present case the contention is not between Wanklyn's process and our process.

Q. Not at all ? A. Inasmuch as our process agrees with the process followed by Mr. Estcourt in this case.

Mr. Sutton : I appear for the Justices, and if I may be allowed to say so, there is a conspicuous fallacy in the mind of the Court and in the mind of the witness. So far as regards this particular question, what the witness says is quite true : the result has become the same. But what we say is that Your process is uncertain, in consequence of the fact that you not only dry your solids in a hot water-bath, but that having dried them in a hot water-bath, you then take your solids out of it, and dry them in a hot water-oven, and the effect of that is that the heat you are able to apply to these solid substances varies so much from circumstances, over which you have no control, that the standard you arrive at in each individual instance is different. You get no certain result. Therefore, this table of analyses, or standard you have prepared, having been prepared by a process which in itself is so liable to uncertainty as to be worthless, cannot be brought forward to test the analysis of a sample of milk which we have obtained by a process which is certain.

The Recorder : I entirely agree with your argument. I suppose that it is want of habit in expressing an opinion on such a scientific question as this that obscures what I say. What I mean to say is that the whole question is that which you have raised. I express no opinion as to the conclusion you draw. If it can be shewn that Dr Bell's system of analysis, which shews that it may be good milk, is fallacious, then his standard becomes worthless.

Mr. Sutton : That is a question of fact.

The Recorder : It is a question of fact.

Mr. Sutton : As a matter of fact, what has been left out of sight by the Court is this : that our witnesses, who came into the box, did state that Bell's process cannot be relied upon. Mr. Bell now goes into the box and says : "It is admitted by you that my process is to be relied upon."

The Recorder : No, all Mr. Bell says is : "My process has, by some marvellous means, brought out the same as your process."

Mr. Sutton : In this particular case.

The Recorder : I quite agree, Mr. Sutton, that the important question is whether it is possible the milk, unadulterated, can have so low an amount of non-fatty substances as 9 per cent.

Mr. Cottingham : The standard of 9 per cent. for non-fatty solids was Mr. Wanklyn's test, which must be taken, together with Mr. Wanklyn's process. Mr. Estcourt has not adopted Wanklyn's process, but he has adopted Wanklyn's test, arrived at by another process than that which he has used.

Mr. Gully : We say that he has used Wanklyn's process.

The Recorder : Do not ask me my opinion, or I should give it. My opinion is that practically he has done so.

Mr. Cottingham : That we deny ; therefore he has no right to set up that standard.

The Recorder : Again I repeat what I have before said : the only question that presses me in the case is whether it is, or is not possible, or consistent that milk which only has in it this amount of non-fatty matter is unadulterated. The knowledge that it does contain only that amount of non-fatty solids may be arrived at I do not care how.

Mr. Gully : The only difficulty is one which I have to deal with before I get to your question, that is, that it is material how it is got at. We say that 8·67 of non-fatty solids, by Dr. Bell's process, is the same thing, or nearly the same thing as 9 by our process.

The Recorder : Then go on if you think so.

Mr. Gully : That is what I say the evidence is.

The Recorder ; Going upon that point, I cannot help thinking that the process by which you have arrived at that point is wholly unimportant. I quite agree that the real question is whether these experiments are worth anything if they are taken by a process different from that used by Mr. Estcourt.

The Witness : You will allow me to repeat, that they rely upon the non-fatty solids to determine whether the milk is adulterated or not. Mr. Estcourt has distinctly stated that he dried the non-fatty solids practically to a constant weight, and consequently he has adopted essentially our process.

Mr. Gully : We differ from that entirely.

The Witness : That is my argument in the matter.

Further cross-examined by Mr. Gully :

Q. Supposing it was tested by Wanklyn's process and produced 9 per cent., and then you took a sample of the same milk and tested it by your process would it further reduce the weight? A. That would have to be ascertained.

Q. Supposing you took a sample of precisely the same milk and tested it by your process, which as I understand would reduce the weight more than Mr. Wanklyn's process would, would yours come out to about 8·6 or 8·7? A. At the beginning I pointed out that it varied from 3/10ths up to 8/10ths.

Q. There we differ again. Would it not vary at least to that extent? A. To what extent?

Q. Would not you by your process reduce what they brought out at 9 to 8·6 or 8·5? A. It might, or to 8·3. But Wanklyn's process has not been applied to this case. There is confirmatory evidence in the matter, because we have Mr. Hehner's results.

The Recorder : Let me see that I understand it. That is a larger difference than I thought existed between your two estimates.

The Witness : Your Worship, the whole thing depends upon the non-fatty solids being dried to constant weight. Mr. Estcourt has admitted that he dried them to constant weight and that is the essence of our process.

Mr. Gully : Mr. Estcourt never did say so.

The Recorder : I know exactly what he said.

The Witness : Therefore I say he has adopted our process, and it is clear that he has adopted our process because it is confirmed by the amount we added to make up for the loss by decomposition, and it is almost confirmed by the result obtained by Mr. Hehner, because he obtained from the same sample 8·29, which contained $\frac{1}{10}$ of acid. We obtained from the other portion of the sample 8·02 with $\frac{1}{10}$ of acid; clearly shewing that the whole thing was done according to our method and one result confirms the other.

Q. Is it the fact that Dr. Duprè is a gentleman, as he has told us, of very large experience in these things? A. Oh, yes ; he is a man of considerable ability and experience.

Q. We have had a number of gentlemen here who have every day practice in this matter, and on whose certificates hundreds of people have probably been convicted and that without appeal ; do you say that those gentlemen are all under an error in putting 9 per cent. as a safe standard, at which, to say adulteration has taken place? Do you say they are all wrong? A. I think Dr. Duprè will admit that he does not act upon that standard.

Q. Do not let us go off upon that. Dr. Duprè told us that in every case where he found it under 9, he had certified that there had been adulteration, and in every such case there had been a prosecution and conviction without appeal, and he had always put the amount of adulteration as from 9·3 when he certified, but that he did not certify unless the non-fatty solids were under 9. Do you say that all those gentlemen are wrong altogether, and that they have been certifying all this time upon a totally wrong basis? A. The cases have not come under my observation.

Q. Has there been any case in which anyone against whom Dr. Duprè has certified has sent the case on to you? A. No, not an instance.

Q. Do you say that this is error on their part altogether, and that in future they must alter their proceedings altogether, and while they test by the same process they are to reduce their figure to 8·3, 8·4 or 8·5? A. I know this : as a matter of fact _____

Q. Do you say that?

The Recorder : He has to give his evidence as to fact and not to consider the result of it. It is like trying to terrify a jury to prevent them from bringing in a verdict of guilty against a man because of the frightful consequences.

Mr. Gully : It is a question of science. I am asking this gentleman whether he says as a scientific chemist that that basis, which has been followed so long by so many chemists who ought to understand their business, is erroneous.

The Recorder : I will answer the question for him. He says it is wrong.

The Witness : I know as a matter of fact that there are well-known analysts in London that would not think of recommending a prosecution for so small a percentage.

Re-examined by Mr. Cottingham :

Q. Have you ever certified for a prosecution for adulteration for so small an amount as 4 per cent. of water? A. We have.

Q. In what case? A. A case in Hammersmith, in which it was about 4 per cent.

Q. Then there was a considerable difference in the amount of fat as well? A. No.

Q. Under what circumstances did you certify in that case? A. It came down to a point at which we were perfectly justified in doing so.

Q. After investigation? A. Yes.

Mr. Gully: You have certified for a prosecution where there had been adulteration to the extent only of 4 per cent. A. But not on the 9 per cent. standard.

The Recorder: I am not in the least biased by what has been done before.

AUGUSTUS VOELCKER, sworn.—Examined by Mr. Cottingham:

Q. You are Doctor of Philosophy, and a Fellow of the Royal Society, and Chemist to the Royal Agricultural Society? A. Yes, and I have been for the last twenty-five years concerned in chemistry, and connected with the Chemical Society of England. Previous to that I was fourteen years Professor of Chemistry in the Royal Agricultural College of Cirencester.

Q. You have had a very large and lengthened experience in the analysis of milk and other articles of food? A. Yes, extending over a good many years.

Q. Have you turned your attention particularly to the composition of milk and the circumstances affecting its composition? A. Yes, I have done so.

Q. You have found that the variations as regards the solid matter are considerable? A. Very considerable; in fact all the constituents, without exception, of milk are subject to variations. The variations are greatest in the case of fat, and less in the non-fat; but still they are variations in the proportion of the caseine or curd which constitutes solids not fat—variations between the curd and milk sugar and mineral matters; so that you have no constancy in the composition of the milk which varies with varied circumstances—for instance, the time of the year, the food given to the cows, and also the breed of the cattle. There are some cows which, if their milk were analysed alone by any Public Analyst, would be universally condemned, and perhaps justly so in a certain sense, as being below the reasonably fair good quality of milk. I speak of the Dutch cows. I find that there is sometimes as much as 90 and 90½ per cent. of water, and the totals of solids scarcely more than 10; but the fact is that you get such a constancy of composition in a large town because the milkmen understand their business and they work up to the constancy of the Public Analysts. There is a regular technical name amongst milk dealers—they know how to "blend" their milk. They buy from poor country districts—the very crust of the land—milk which is generally poor, and blend it with milk which is kept in the neighbourhood of towns, by cowmen who deal largely in milk, who feed richly and produce milk which is rich in all constituents. You may get as much as 10 or 10½ of solids not fat, and as much as 4 to 5 or 5½ of solid fat, and by blending those together they can produce milk which comes up to a given standard. That would account in a measure for the apparent uniformity of results that you obtain by analysing milk as supplied to towns.

Mr. Cottingham: You have been in Court while Dr. Bell was giving his evidence, and you heard his evidence? A. Yes.

Q. Do you agree with that evidence? A. Yes, I do in all essential particulars.

Were you also examined as a witness in the Committee Room on this bill? A. Yes, and I strongly opposed the notion of fixing a standard, because a standard has a tendency, which I foresaw then, to this: that the milk dealers would work up to a given standard; and what they do at the present time is, they allow the milk producer to skim off partially the milk, and yet, by blending with non-skimmed milk they bring it up to the standard required by the Public Analysts, whereas a most valuable portion is now deliberately taken off—the cream is taken off from the milk and the standard is still maintained; and milk which unquestionably is skimmed is frequently sold as perfectly genuine, and milk which is genuine, but falls below the standard of 9 per cent. of solids not fat, is condemned, and injustice in that way is done and has been done.

Q. You have, in your experience, known numerous instances of milk falling below the standard of 9 per cent. solids not fat, yet still being genuine milk? A. I have.

The Recorder: That is a question of importance. Do not answer that hastily. Do you mean that in your experience you have tested milk which has fallen below the standard? A. Below 9. With your permission I will give those instances, or hand them over to your Worship afterwards. As early as 1863, I published a paper in which I gave the average composition of 22 samples of milk taken from a herd of cows.

Mr. Gully : By yourself ? A. By myself. In fact our students at the College, at the time I was Principal at the College, complained of the quality of the milk. I was struck with the milk being very poor at the time, and I enquired into the circumstances. This led me to make an investigation of the influence of the time of the year, and when the cows were milked, on the quality of the milk. I analysed the milk of the whole herd. It was not for sale, but merely for the supply of the College. Sometimes we had not enough. There were about 15 cows, I believe, at the time, and I analysed the milk from those cows every month twice, the morning and the evening milk, for eleven consecutive months, with the exception of August, when I was away for the vacation. I found then that of the 22 samples of the milk of the whole herd, 9 samples contained less than 9 per cent. of solids not fat, and one of the 22 samples contained as much as 10·7—there was in round numbers 10 per cent. of solids not fat; and another contained as little as 7½ per cent. of solids not fat. Thus you have here a range of 7½ to 10, that is 2½ difference in the solids not fat.

The Recorder : Were all those cows fed the same ? A. All fed in the same way. Then during the last four years the British Dairy Farmers' Association give prizes for milking cows that produce not only the largest quantity, but also the richest milk, taking into consideration the quality as well as the quantity, and by assigning certain points for quality, and certain points for quantity, we are able to say at the conclusion which are the best milking cows. Therefore, you may rest assured that no cows are sent up but those in good condition, the really good cows and well fed cows; but I find that the influence of race is very great, as, indeed, every milk dealer knows who has any experience in the milk of Alderneys or the milk of the large breed of cows, the red Oxfordshire old cow, or the Shorthorn and the Dutch cows—one is very much richer than the other. I found the following results, in the following years, with individual cows which were separately milked in my presence, with the exception of this year, when I could not be present, but my son was present, and the milk was received by me for analysis : In 1879, 1880, 1881 and 1882, I was present all the time they were milking when the samples were taken. I took the samples myself and bottled them up, and they were analysed in my laboratory. I found in 1879, in four samples out of twelve, less solids not fat than 9 per cent.; eight varied from over 9 up to 10. Then, in 1880, at the Dairy Show, I found that all the Shorthorns and cross-bred cows (there were only four shewn for competition for the milk prize) contained on an average somewhat under 9 per cent. of solids not fat. Every one of the four cows that were shewn, or competed for the milk prize, produced milk, the solids not fat in which were under 9 per cent. Some came very near, but they were under 9 per cent. Four out of six cows, of the Jersey and Ayrshire class, gave milk containing less than 9 per cent., five contained about 9; and seven cows out of nine, in the Dutch class, yielded milk containing less than 9 per cent. Then, in the Dairy Show for 1881, seven samples out of fifteen contained less than 9 per cent. of solids not fat.

Mr. Gully : Those are your own samples ? A. My own samples.

Q. And your own analyses ? A. My own analyses, that is to say in the sense in which Dr. Bell has explained, made under my own immediate superintendence, mostly by my son, and done in my laboratory, and I was there present all the time. Seven out of fifteen samples contained less than 9 per cent. of solids not fat. Two samples contained less than 8 per cent. of solids not fat, and another sample contained as much as 10½ per cent. Then last year, in 1882, out of twenty-six samples nine were found to contain less than 9 per cent. of solids not fat. This year comparing a few samples taken from seventeen cows which competed for the milk prize, three out of seventeen gave less solids not fat than 9 per cent.

The Recorder : What were those cows—what sort of cows ? A. They were mostly Dutch or cross-breds—large cows. You will seldom find in the Jersey or Ayrshire classes that they yield less than 9 per cent. of solids not fat; generally above. You may find as much as 10½ solids not fat. So that you see how difficult it is to fix anything like a standard. I do not know whether I may be permitted to make any remarks on this question of standards.

Mr. Gully : I would rather that my friend asked questions. I must really ask my friend to conduct his case in the usual way.

The Witness : I find that the standard adopted in Paris is 11 per cent. total solids, of which 3 per cent. ought to be fat, which leaves solids not fat 8. I think that is a very reasonable standard; 3 per cent. of fat makes it high. If you ask me the question, Is the standard adopted by the Public Analysts fair or low or high, I should say——

Mr. Gully : This is not evidence. This is a sort of historical lecture. It is impossible to check the process by which they say this is to be ascertained.

The Recorder : It is quite open to that objection.

Mr. Gully : 8 there may mean precisely the same thing as 9 here.

Mr. Cottingham : Are these measurements you have given us the same? A. Yes, the kind of method which is adopted would not produce any practical variation.

The Recorder : The impression upon my mind has been for some time that any scientific process would not make any very great difference.

Mr. Cottingham : That I quite agree with. The question here is the conclusion to be drawn from these analyses.

The Witness : I was going to remark that if I were asked whether the standard adopted by the Public Analysts was a low or a high one, I should say it is decidedly too low a one, because they do not require a fair average proportion of fat. You may expect during the greater period of the year a higher percentage than $2\frac{1}{2}$ of fat. The average is much nearer 3 than $2\frac{1}{2}$. It is only in exceptional cases of very poor food or in the spring of the year, in March or April, when the grass is just springing afresh and is immature, and rainy weather sets in, and where you have an additional quantity of water given with the food, that the milk is exceptionally low; but throughout nine months of the year I should say by the adoption of the Public Analysts' standard a sort of legal right is given to milk dealers to skim their milk and to sell milk of too low a quality, for I need not remind your Worship that 2 per cent. of solids fat is a great deal more valuable than 2 per cent. of solids not fat. They blend the milk together. It is a practice with many of the large milk dealers to keep chemists for the purpose of seeing that none goes out that is below the standard.

Mr. Cottingham : What do you say—Is 9 per cent. of non-fat too low. A. If I were to give a standard I would say raise your standard in fats—lower $\frac{1}{2}$ per cent. in solids not fat, and screw up the milkman to really unskimmed milk. I am not prepared to recommend any standard, because although you may have in your own mind a sort of standard, you must apply it with discrimination and take into consideration even the price. I know that some milk dealers actually get 1d. to $1\frac{1}{2}$ d. more per gallon than others because their milk is so much better for blending.

Q. Have you seen the analyses in the case before the Court ? A. I have.

Q. In your judgment it is impossible for any chemist to come to the conclusion that any water had been added as a scientific conclusion from these analyses, assuming those analyses to be correct? A. You cannot say it.

Q. You could not affirm that any water whatever had been added—that there was any adulteration ? A. You could not.

Q. Then you come to the conclusion that these analyses are perfectly consistent with perfectly genuine milk ? A. Yes.

Q. So that this milk which Mr. Wardle has been convicted of selling adulterated may be in your estimation perfectly genuine ? A. Yes, taking into consideration the time o the year when the milk was sold, and also the probability of the fact that the cows had no concentrated food in the shape of cake or meal and were fed on the natural produce of the land.

Mr. Gully : We have had no evidence of that.

The Witness : Assuming that I have had no evidence upon the case if the cows were fed upon grass alone at that time of the year, all I can say is that it would be fairish milk, but rather poor for that time of the year.

Mr. Cottingham : 4 per cent. is a very very low amount of water to adulterate with ? A. I do not think a man would risk his character for that.

Mr. Gully : Is this evidence that a man would or would not risk his character for the purpose of making money. It is not a question of risking character ?

The Witness : I do not think he would do it for his own credit's sake.

Mr. Wardle : I am sure I would not.

Mr. Cettingham : Did you hear that paper of Mr. Hehner's read to day ?

Q. Do you agree with what he says about a standard there ? A. Quite. I quite agree with all Mr. Hehner has said.

Q. Do you agree with the paper I read from THE ANALYST ? A. I agree with that.

Cross-examined by Mr. Gully :

Q. And you agree with Mr. Hehner's evidence generally to-day ? A. Yes, I do.

Q. Do you differ from Dr. Dupré's evidence ? A. With the exception of his fixing a standard for solids not fat at 9. I certainly do not agree with that.

Q. With the exception of that you agree with him ? You are against all standards ? A. I am against all standards.

Q. How would you test milk practically if you were a Public Analyst ? A. There is the difficulty, because you cannot distinguish between naturally poor milk and watered milk.

Q. A Public Analyst has so much milk sent to him in a vessel. If you had not a standard how would you test it ? A. I am glad I am not a Public Analyst to have to decide that question.

Q. You have no other theory as to how it should be done ? A. No, as I said, because I must take all things into consideration ; I certainly would analyse it, and if I found the milk below the standard that I have fixed in my own mind I would take means to get full particulars.

Q. I am speaking of this : supposing you were a Public Analyst, and were called upon to certify in a certain statutory form whether this milk had been adulterated or not. How would you ascertain whether it had been, or not, except by a standard ? That is what Public Analysts' have to do ; they are not allowed to go to the farm. A. The Government has carefully abstained from adopting a standard, and so has the Board of Trade.

Q. You have not offered any other resource. You arrived at 7·50 non-fatty solids with one sample. Was that from a single cow ? A. That was a single cow.

Q. Can you shew any average of the milk of 15 or 16 cows giving less than 9 per cent. ; I do not mean picking out exceptional cows ? A. Yes, I can. That was 7·50.

Q. Was this your own experiment ? A. My own experiment—that was 7·50, the average of 15 cows—the whole herd.

Q. Where was that ? A. That was at Cirencester when I was resident there.

Q. When was that ? A. The paper was published in 1863—that was in 1862 then.

Q. Was that a herd that had been starved or ill-fed ? A. They were poorly fed ; they had not enough to eat.

Q. They had been badly treated ? A. Yes, they had not sufficient food.

Q. Supposing the Government had set up a standard, you would hardly let a case like that interfere with your acting ? A. No, I would not ; certainly not. There is a danger of fixing the standard too low.

Q. Even under the shadow of the Royal College of Agriculture they had been starving. With the exception of that case, do you know any case where an average of the milk of 15 cows has given solids not fat below 7·50 ? A. No.

Q. Below 8·00 ? A. Yes.

Q. Where was that ? A. 8 out of 22 where the percentage of solids not fat fell below 9 —————

Q. Supposing you take the 22. What is the average of the 22 ? A. With the exception of that one unusually poor, I have others with 8½ solids not fat, then 8½ again, 8·88 and 8·70.

Q. Then the others are over 9 ? A. The others are all over 9, some as high as 10.

Q. The average of the 22 would be higher than 9 ? A. Yes, it would.

Q. May we not take it that the average of that herd of 15 or 16 cows will be over 9 ? A. Taking it throughout the whole year, but not in separate months.

Q. As regards Dutch cows and so forth : Dutch cows are not imported for the purpose of being fed in Derbyshire, to supply milk in Lancashire ? A. They are chiefly imported for the sake of the milk supply.

Q. Do you find in Derbyshire and Cheshire Dutch cows with that very small proportion you have told us of ? A. No, I do not think they keep them in Cheshire. They are chiefly kept by milkmen in the neighbourhood of towns.

Q. When you analysed these what process did you use ? A. I have used, I may say, every process which has been published at various times.

Q. In 1862 ? A. In 1862, I extracted the total solids with ether.

Q. How much milk did you take ? A. I took various proportions from the determination of the total solids. I took out about 10 grammes, and for the extraction of the oil I took as much as 3 times the quantity—30 grammes—so as to get a fair average.

Q. How long did it take to complete an analysis from beginning to end. What time was spent over it ? A. For practical purposes an unreasonably long time.

Q. How long, about ? A. Perhaps some three days for each analysis.

Q. So that the milk would be ten days old by the time it was finished ? A. Oh no, they were all done immediately the milk was taken. I had only two samples every month.

Q. Is it more accurate than it was then ? A. I cannot say that ; but for practical purposes you get sufficiently accurate results, with a plan like that of Wanklyn.

Q. You have not been a Public Analyst of any kind, nor had to certify for purposes of this kind ? A. No, but I have frequently to report on milk, whether it is genuine or not.

Q. Do you find that the quantity of solid non-fatty matter varies according to the time of the year ? A. Yes, the solid non-fatty matters.

Q. To what extent—within what range ? I am not speaking of exceptional cases, but what do you find is the fair range that you can depend upon ? A. I should say it ranges from $8\frac{1}{2}$ to $9\frac{1}{2}$ solids not fat.

Q. It varies to that extent—it ranges over one in fact ? A. It ranges over 1, but you may have greater variations ; I only give you the average.

Q. Is the $8\frac{1}{2}$ arrived at by a process like Dr. Bell's ? A. By the perfect extraction of the oil, which is difficult to realize by the adoption of Mr. Wanklyn's process. I am sure you will forgive me for saying so, but I have perfectly extracted it, even by Wanklyn's process.

Q. I want to follow what you really did. Did you follow the same process that Dr. Bell followed, preferring that to Mr. Wanklyn's, because you got a more perfect extraction of the fat ? A. I prefer the extraction with anhydrous ether, or what is practically very strong ether, leaving all the watery portion of the ordinary ether out of contact with the dry residue, so that I can extract fully the oil.

Q. Is not it a fact that Wanklyn's process leaves a greater weight than your process ? A. It may, or may not do ; it depends how it is worked. I am sure Mr. Wanklyn would not leave much oil in because he does it perfectly.

Q. I mean in the way described by Mr. Wanklyn and Mr. Wilkinson ? A. The tendency is that there is some oil left in the residue.

Q. Is the effect of that to make your 8.50 correspond with their 9, or thereabouts ? A. It may, it has a tendency to increase the solids not fat.

Q. Roughly speaking, would that be about the difference that you would expect to find ? A. The difference between what ?

Q. The difference between the results of the analyses—the residuum of non-fatty solids left after treating the milk by your process, and by Wanklyn's process ? A. You may have a difference of between 3/10ths to half per cent. even ; it depends very much upon the quality of the milk ; and what applies to one sample of milk, will not apply to another.

Q. Treating it by your process, would you say that 8.50 of non-fatty solids was a very low average for 15 or 16 cows ? A. I should say that it was a fair average.

Q. Treated by your process ? A. Yes, but rather low—below the average.

Re-examined by Mr. Cottingham :

Q. Was there anything in the constitution of this at all unusual—I mean do you find, in fact, all the constituents of genuine milk in this milk, according to the analysis? A. Yes.

Q. All the constituents of genuine milk, a fair proportion and proper quantity? A. Yes, and if I had had to report upon it, I would have returned it as genuine.

WILLIAM THOMPSON, SWORN.—Examined by Mr. Cottingham :

The Recorder: What are you? A. I am an Analytical and Consulting Chemist at the Royal Institution, and a Fellow of the Royal Society of Edinburgh; Member of the Chemical Society, and a Member of the Society of Public Analysts.

Q. Have you seen the analysis of this milk? A. I have.

Q. In your opinion does that analysis justify the conclusion that 4 per cent. of water has been added? A. I should think it does not justify any conclusion.

The Recorder: As to whether——? A. As to whether it contains water or not.

Mr. Cottingham: Is there anything in the analysis you have seen, either in the quantity of non-fatty solids, the quantity of fat, or anything else that is not perfectly consistent with genuine milk? A. I believe not.

Q. In point of fact, would you have passed such a sample as genuine if it had been submitted to you? A. I should form no opinion. I should say it might be adulterated or not adulterated.

Q. There is no evidence of adulteration? A. There is no evidence of adulteration.

Cross-examined by Mr. Gully :

Q. It is low? A. It is low.

Q. And would excite suspicion if put before you as an analyst? A. It might do.

Q. Are you a Public Analyst? A. I am not.

Q. Have you analysed milk to any great extent? A. I have done a large number of samples.

Q. Do you mean for farmers who have brought it to you? A. Yes.

Q. Or do you mean by way of experiment? A. For farmers, and by way of experiment also.

Q. Farmers often bring you their milk do they? Yes, we have a considerable number come.

Q. Would 8·50, the residuum left after Mr. Bell's process had been applied correspond to somewhere about 9 after Wanklyn's process had been applied? A. From my experience I should think it would not be so.

Mr. Cottingham: Do you agree with the evidence given by Dr. Bell and the last witness?

The Witness: Yes, I think I have answered the question by saying that there is no evidence so far as I know that it is adulterated.

Mr. RICHARD BANNISTER, SWORN.—Examined by Mr. Ferguson.

Q. I believe you are an analytical chemist in the laboratory at Somerset House? A. I am Deputy Principal in the laboratory at Somerset House, and an analytical chemist also.

Q. In your laboratory they examine articles for the Board of Trade and the Customs?

The Recorder: We had that from Mr. Bell I think.

Mr. Ferguson: You assisted in the analysis of this milk? A. I did.

The Recorder : You signed the certificate, did not you ? A. Yes, and not only that, I saw all the weighings and calculated all the results as I always do in connection with milk cases, or any cases of adulteration.

The Recorder : As I have said before, I do not think this much signifies, because the results come to practically the same—so as to make no difference.

Mr. Ferguson : In your opinion the results arrived at are perfectly consistent with this being a genuine sample of milk ? A. Just so.

Q. Now will you tell us how you analysed this milk ? A. Is it necessary to go over the whole of it ?

The Recorder : What does it signify. I may still be wrong, but I have said some hours ago that I do not think that is at all important, because this witness has arrived at the same conclusion with the Somerset House system, as Mr. Estcourt with his system. If their system is wrong, it is a very lucky accident that they happen to come to the same conclusion.

The Witness : I have not the slightest objection to give it to your Worship, but I want to save the time of the court in every way I possibly can. Dr. Bell has done it already.

Mr. Ferguson : Is there anything in the result of the analysis to lead you to the conclusion that the milk was watered ? A. There is not.

The Recorder : We have his certificate with his opinion. You agree in the certificate ? A. Quite so, or I should not have signed it.

Q. There was some other gentleman ? A. Mr. Lewin : he is here in Court.

Q. It is a joint certificate ? A. Yes.

Q. You all did agree ? A. Exactly your Worship, or we should not have put out names to it.

Mr. Gully : I take it that this gentleman says the same thing, and that Mr. Lewin says the same thing as Dr. Bell.

Mr. Cottingham : We had another scientific witness to call, Sir, but there being a death in his family he is not able to be here. That is the case, Sir. I have only a few words to say——

The Recorder : I think I quite understand the question now. If any learned Counsel wishes to address me, I shall be glad to hear him.

Mr. Gully : Do you wish to call upon me ?

The Recorder : If it is not a courtesy, I think neither of you can throw any light upon it, or I am sure you would do so. I think I am sufficiently informed upon the matter now to form my decision. I think so.

Mr. Gully : I am quite content to cry quits with my friend upon that.

Mr. Cottingham : Then I shall not trouble you with any observations.

JUDGMENT.

The Recorder : This is a conviction under the Sale of Food and Drugs' Act, 38 and 39 Vic., cap. 63, and 42 and 43 Victoria, cap. 30.

The appellant, Richard Wardle, has been convicted in a penalty, by the Justices of Manchester, for selling adulterated milk, and he has appealed against the conviction upon several grounds. The first ground is that he is not guilty; the second is immaterial, I think; the third is a legal objection to the conviction; and that raises a question, perhaps, of some importance, viz., whether the certificate of the officers at Somerset House is conclusive or binding upon the Justices or upon the Court of Quarter Sessions.

With regard to this third ground of appeal, I am clearly of opinion that it is not well founded. The words of the 22nd Section are: "The Justices before whom any complaint may be made, or the Court before whom any appeal may be heard under this Act, may, upon the request of either party in their discretion cause any article of food or drug to be sent to the Commissioners of Inland Revenue, who shall thereupon direct the Chemical Officers of their Department at Somerset House to make the analysis, and give a certificate to such Justices of the result of the analysis," &c.

Now it appears to me perfectly clear that the object of the legislature was that in case of any error fallen into by the witnesses before the Justices in the county, that they should be corrected by the certificate sent by the authorities at Somerset House, and that the Justices or the Court of Appeal should have the advantage of such a certificate that they might form their judgment upon it; but I do not think that that at all takes away either the responsibility of the Justices or that of the Court of Quarter Sessions, who must give a perfectly independent decision upon the merits of the case, of course giving full weight to the opinion of the Chemical Officers of the Department at Somerset House; therefore, I think that that ground of appeal fails.

Now, in this case I have before me the oath of a person who says that he supplied this milk and that he did not in any way adulterate the milk; and in considering the judgment to which I come, I must take into consideration, not only the scientific evidence, but the facts of the case. I cannot conceal from myself, nor do I wish to conceal from myself, the fact that Wardle, the farmer, seems to have acted in a perfectly straightforward way. He at once sent the samples, taken from these milk cans, to perfectly independent analysts, who both gave a decision adverse to him. His conduct in that particular leads me to take a favourable view of the statements he has made, that this milk was not in any way adulterated.

Then there comes the scientific evidence. That is a vast amount of evidence of the very greatest value, which goes to shew that the analysis—I decide entirely upon this analysis of Mr. Estcourt's—leads conclusively to the result that this milk was adulterated with water. A very great deal of scientific evidence is gone into to prove that conclusion.

Now, on the other hand, there is the evidence of the certificate of the Somerset House Analysts, which, I take it, I am to use for my assistance upon this trial; and if I am not to use it, at all events I have the evidence of the gentlemen who have given the certificate. They state that after "making the addition for natural loss arising from the decomposition of the milk through keeping, the proportion of non-fatty solids is not lower than is found in genuine milk. The percentage of fat and ash are equal to those found in genuine milks. From a consideration of these results we are unable to affirm that water has been added to the milk." The correctness of that certificate is, to my mind, corroborated by the fact that the analysis made some three weeks after the milk had come from the cows, for all practical purposes, produced the same results as that which was made by Mr. Estcourt; and that rather leads me to the conclusion that the analysis could not have been at all carelessly taken or slurred over by those gentlemen, Dr. Bell, Mr. Bannister, and Mr. Lewin. I assume then that the analysis of Mr. Estcourt was correct, and that the analyses of all these gentlemen, although not quite identical, were for all practical purposes correct.

Against the oath of Mr. Wardle, and against his general demeanour and conduct, I am asked to decide that this water was put into this milk, upon scientific evidence, which is contradicted by the scientific evidence of such gentlemen as those who have been recently called. This is a matter in the nature of a criminal proceeding; and to use an expression which is always used in criminal proceedings to juries—and I sit here as judge and jury in this case—I must be satisfied beyond all reasonable doubt that this man has been guilty of the offence charged against him; and I am not satisfied. If it were necessary I would express an opinion as to the propriety of the different systems of analysis which have been adopted, because, although I know nothing of science, after hearing such extremely good evidence as I have heard on both sides, if it were part of my duty, and I were bound to do it, I would give a judgment upon that question. But it does not arise, and I am not called upon in the present case, in the view I take of it, to give any decision whatever as to which is the best mode of analysis for milk. I ground my decision not certainly upon any opinion that either of the analyses was incorrectly conducted. I say that most absolutely. I might go further if it were necessary, only it is not necessary to say it—it appears to me that both analyses were skilfully and well conducted; but it is unnecessary for me to say that upon the present occasion judicially.

The conclusion I have come to, is, that the offence charged against this man is not made out to my satisfaction, and I do not know that there is any value or use in my saying anything more upon the matter.

I thought for a considerable length of time that it might turn out that an analysis made after the milk had been kept three weeks was nearly valueless; but when I find that after three weeks the analysis made turns out to be practically the same as that made when the milk was fresh, I cannot suppose that that is a matter of chance, but that it was the result of scientific investigation and enquiry. The investigation which has taken place in this matter is one that I daresay will be advantageous to both sides if I may call them sides—both to the parties who side with one system of analysis, and the parties who side with the other system of analysis, but I am not going into that to-day.

I have already stated that it has not been proved to my satisfaction that this milk was adulterated with water, and that being the conclusion at which I have arrived, I can do nothing more than confirm this appeal and dismiss the original conviction.

Mr. Cottingham : Now, Sir, there is a second conviction which I must draw your attention to. I hope you will give us the costs of this ?

The Recorder : Yes.

Mr. Cottingham : There is the second conviction.

Mr. Gully : Does my friend want to try that ? It follows the first I suppose ?

The Recorder : Yes.

Mr. Cottingham : If it follows the first, that is dismissed also, and I have to ask you for the costs of that.

The Recorder : The costs will be taxed. There will be nothing on the second conviction.

Mr. Cottingham : I want to draw your attention to this : that there is one offence, and there ought not to have been a second conviction at all.

Mr. Gully : Does my friend want to argue that ?

Mr. Sutton : In giving the costs you do not I apprehend give costs against the Justices ?

The Recorder : Oh no.

Mr. Cottingham : Not for convicting a second time, for the same offence.

The Recorder : Certainly not. I will say—Appeal confirmed, Conviction dismissed.