

# A Mathematical Consideration of Cane Sampling

By G. S. MOBERLY.

Mr. G. S. Moberly read the following paper:—

The extreme difficulty of obtaining representative samples of cane either in the field or the mill-yard has long been recognised by the sugar industry. Each stick of cane forms such a large unit in any sample of manageable size, and there are such extreme variations between individual sticks that each stick selected affects the result. The selection of a complete stool or a number of stools from a field is likewise valueless, as the same variations occur between adjacent stools. Variations between 11% and 15% of sucrose have been found in adjacent stools.

Let us consider the case of a field containing stools of 11%, 12%, 13%, 14% and 15% sucrose. Experiments show this to be possible. If such stools are evenly distributed the average percentage would be 13%, but if only one stool were selected there would be two chances in five of a 2% variation, two chances of a 1% variation, and only one chance in five of a fairly accurate result. Such a test would therefore be valueless. If two sticks were taken the chances in 25 tests would work out as follows:

2% variation . . . . .	2 chances.
1.5%    "   . . . . .	4    "
1%       "   . . . . .	6    "
.5%      "   . . . . .	8    "
Accurate . . . . .	5    "

The chance of an accurate test remains the same, but the chances of a variation of 1% or over are now 12 in 25 instead of 20 in 25. This is a slight improvement, but the value of the test is not twice as great.

If three stools are taken, there are 125 different possible combinations. Of these 19 are accurate, but another 36 are only .33 out. Twenty will be 1% out, and another 20 will be more than 1% out.

Here again there is an improvement, but we have less than an even chance of being nearly right, and one chance in three of being 1% or more in error.

Four samples give 625 possible combinations. Of these 245 are accurate or within .25%, while another 120 are .50% out, 140 are 1% or more out.

The improvement continues, but although four stools would be a bulky sample to analyse this would be far from being a reliable sample of the field under consideration.

Similar figures could be calculated for larger numbers of stools, but they become cumbersome as they get larger, and the results can be better shown in a graph.

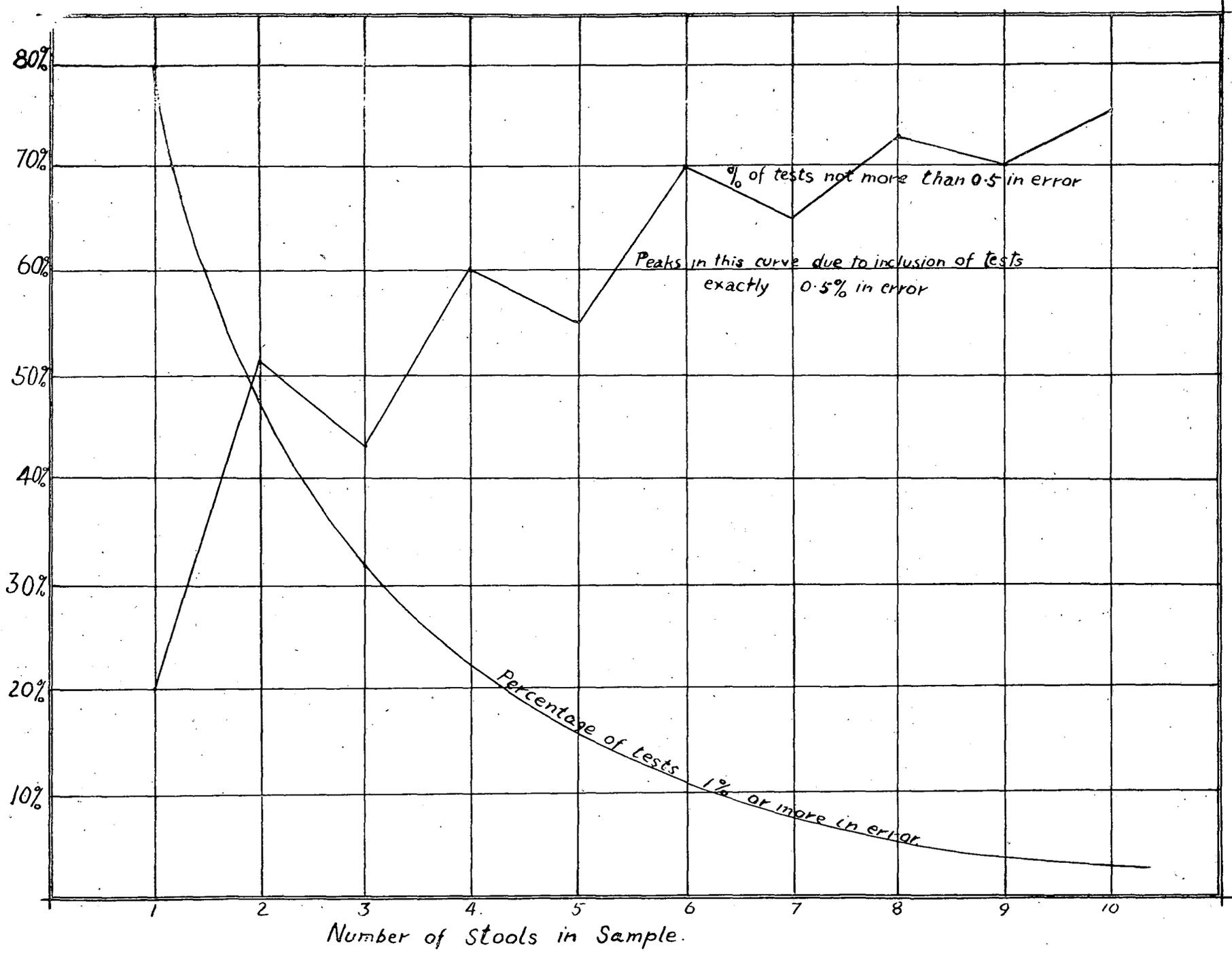
The attached graph shows that even with 10 stools 3% of the tests will be 1% or more wrong and only 75% will be within 0.5%. With 8 stools the results are very much the same. Now 8 or 10 stools will be far too bulky a sample to handle and drastic sub-sampling becomes necessary. Here again we are up against the same difficulty because the same variations occur between different sticks of the same stool as occur between adjacent stools of the same field.

Therefore, to get a reasonable sub-sample of any individual stool it would be necessary to take at least 8 or 10 sticks. This means from 64 to 100 sticks in the united sub-sample, which is still too large to manage. We might, however, take a definite number of sticks from the united sample without considering which stools they came from. It will be shown later that at least 40 sticks would have to be so taken.

This is as near as we can possibly hope to get in sampling a field of cane showing the variations mentioned. Of course the stools in some fields might be more homogeneous, but since such variations are possible they must be allowed for.

The same considerations apply when sampling cut cane in the mill-yard, or the carrier.

With cane of 13% sucrose the same variations are possible between stick and stick, as we have already considered as between stool and stool. However, there is a possibility that a majority of the



canes will approximate to the true average sucrose. I have therefore considered the case where an average ten sticks would be composed of the following individual sticks:—

1 at 11%  
2 at 12%  
4 at 13%  
2 at 14%  
1 at 15%

As the mathematical calculation now becomes very involved a practical demonstration has been resorted to. The above figures were written on ten cards from which random selections were made. Three series of selections each consisting of 10 averages of 10 each, gave the following results:—

1st series.	Average variation, .3	Maximum variation, .7
2nd series.	" " .2	" " .7
3rd series.	" " .3	" " .7

When, however, 20 selections were averaged at a time, the following results were obtained:—

1st series.	Average variation, .20	Maximum variation, .40
2nd series.	" " .24	" " .50
3rd series.	" " .25	" " .60

There is a slight improvement here, but the results are still not good enough. With 40 selections we get the following:—

1st series.	Average variation, .18	Maximum variation, .35
2nd series.	" " .24	" " .45
3rd series.	" " .20	" " .45

Still the figures are not very good, but the size of the sample is getting out of hand. Sub-sampling by taking a smaller number of complete sticks won't help us as the effect would be merely the same as taking the smaller number of sticks to begin with. We can, however, cut each of the 40 sticks in half and take tops and bottoms of alternate sticks, leaving 40 half sticks. The process could then be repeated again with halving, leaving 40 quarter sticks. This would leave a sample of manageable size.

In view of these figures, I consider that the minimum requirements of cane sampling should be:

#### Field Samples.

Take 10 stools at random from the field. Unite them, mix well, and take out 40 sticks. Then sub-sample as described below.

#### Mill Samples.

Take 40 sticks at random from different parts of the consignment. Cut in half and take tops and bottoms of alternate sticks. Repeat the process until the final sample consists of 40 quarter sticks.

The above is very cumbersome, but nothing less will give anything like reliable results. Cane sampling should only therefore be resorted to in exceptional cases and a greater accuracy than .5% in the sucrose reading should not be looked for.

have a large experimental error, as you necessarily have in the case of sampling a very variable material like cane, as you have all stages of growth in every stool. It is a problem we are continually faced with in experimental work, not only in getting an accurate idea of the analyses of cane from a particular area but in the matter of getting yields also. When this matter was first brought up before the Committee on Chemical Control I felt sure that our existing method of cane sampling by basing any conclusions on one sample is absolutely useless and worse than useless because in many cases it would be misleading. Whether the modifications which Mr. Moberly proposes of cutting the sticks into a smaller number of sections and taking alternate tops and bottoms and so forth, will reduce the experimental error to manageable proportions remains to be seen. But there are indications that it would be a very useful modification of the practice. You can of course overcome the error of the series, the experimental error, by increasing the number of tests. There is no doubt many of you are aware that the standard deviation from the mean—the experimental error—can be reduced by the ratio of the square root to the number of tests. For example, if you have a series of experiments with an average deviation from the mean of say 10, if you have only two duplicates or only have two replications of the experiment that error may be reduced from 10 to 10 divided by the root of 2, 10 divided by 1.4 approximately, which does not bring us down very much, but if you have four you may divide your deviation by 2 to get at the probable error, and if you have 9 replications you may divide by 3, and so on. It shows what a large number of tests is necessary to get a low probable error by such means where you have a very variable substance to deal with as we have in the case of sugar cane.

Mr. BOOTH: With reference to Mr. Moberly's report, I received this paper some days ago in draft more or less, and I am sorry I had not any notes with me. I have in the past years done quite a lot of sampling, but have never reduced anything to mathematical formula in this way, but I based my sampling on Dr. Barber's investigations in which he reduced the growth of cane to mathematical formula. Taking Mr. Moberly's basis I am afraid that does not represent the Uba cane, and I think his basis is wrong, and his further argument I think is on false premises. I would not like to substitute anything else for it but wish to point out one thing. Here you are dealing with plant life, the quality of the sticks cut from the stool depends on so many variations. I have repeated this at the meetings several times over. You have to take into consideration soil conditions, plant growth, humidity of soil, etc., before you can possibly arrive at any mathematical or hypothetical formula such as Mr. Moberly has done here. Personally I went to the

CHAIRMAN: This paper illustrates some of the difficulties and complexities in sampling where you

trouble of taking out a whole stool of plant cane and ratoon cane so as to get the actual growth, to try and prove or disprove and satisfy my own mind that Dr. Barber was correct. Upon these studies I then formulated my scheme of sampling cane, and I am quite satisfied in my own mind that I could pick a sample of cane without any resort to mathematical forms, but by biological observation, if you might call it so, a figure which I could almost swear to when that field in bulk was put through the mill. I may say further that my samples came very close to Mr. Moberly's. I consider nothing less than 40 lbs. to be a representative sample, which was chopped in three separate parts, split down the centre, and then the final figure obtained.

Mr. BECHARD: What Mr. Moberly has told us is very interesting. What Mr. Booth says is quite possible, but it requires a trained sampler; it is not everybody who can sample in that way. In view of that and the difficulty of sampling, I think we should discourage all field sampling; why not leave it where the Chemical Control Committee had it? I would certainly advise leaving out one stool altogether. When you get to two stools it becomes something not quite 25 per cent. as bad as the other. As 10 cane stools are quite out of the question, I don't think we can do anything except discourage field sampling of cane, and specially hand-picking of samples by planters.

Mr. MOBERLY: I would like to point out that these sticks I have taken are not in any way meant to represent cane as it is, but to take what is a possible bad case, and in sampling of this sort you have to allow for the bad cases. It is possible that there are even worse distributions than that, although the normal may be considerably better. As to what Mr. Booth says about sampling—undoubtedly you would have to have an expert sampler, but if it were possible his method would probably be ideal. I think Mr. Bechard is right in saying that what we should do is to discourage field sampling as far as possible. That is the main idea here. As far as sub-dividing and cutting down is concerned, that is in conformity with the ordinary method of sampling, that is diminution and division together. You get no advantage if you simply sub-divide and sub-divide unless with each division you bring about a breaking up. If you don't do that you might just as well take your small sample to begin with.

Dr. HEDLEY: I think Mr. Moberly diminishes his sample incorrectly when he chops it across the cane. I think you should split it down the centre and then split your sections again down the centre. I found that in milling tests in order to get something like a sample one had to get a large sample, and the only way to get an average result is to do two or three analyses on the same sample from the mill, and I found the best results were obtained by cutting down the centre, splitting the cane, and not cutting across. It is a point in taking samples.

Mr. MOBERLY: I think that might be incorporated with the other. It would be rather difficult to make more than one split. But if the first division was made along the length of the cane and the second across the cane, then I think you would get a division which is not too tedious and more accurate than the other.

Mr. BECHARD: In the case of field sampling I don't think splitting should be advisable. The field sample has to be carried to the Lab. to be tested and probably the cane would be affected by cutting in the centre. But as I said before we should not have field sampling at all.

CHAIRMAN: I think that is the ideal, to abandon field sampling as far as possible. It has been found in most countries that it is impracticable and we ought to recognise that here.

Mr. DYMOND: The object of hand sampling is usually to provide the planter with an indication as to what field to cut first, so that while one agrees that a single hand sample is quite inadequate for pre-determining the actual sucrose content of a field, it is still a practical necessity at times to provide an indication of the ripeness or otherwise of a field of cane. The difficulties involved in obtaining a representative hand sample of cane are illustrated by the following observations—each stick in a single stool of cane will vary both in fibre and sucrose by approximately 5 per cent.—in any single stick the fibre will vary node by node from 23 per cent. to 9 per cent., while eight adjacent stools of ratoon cane apparently identical varied from 12 to 15 per cent. in sucrose. Bearing in mind, however, the object in view, namely, to provide an indication only of the maturity of a field of cane, the difficulty can be overcome to a certain degree by increasing the number of samples taken to a point, where the average of a greater number will reduce the possible error within practical limits. The erratic nature of the results should be shown by recording all the results obtained. I consider, therefore, that at least four separate stools of cane should be specially tested, the results would then provide the necessary indication of a field's maturity.

Mr. BOOTH: I have had quite a lot of experience in sampling cane. I support Mr. Dymond in this statement that it is only an indication. For practical purposes I think if a farmer knows his job he does not want any chemist to come along and tell him. He has his cycle of cutting, he knows the climatic conditions, and he knows when to cut. My experience is give me a planter who keeps his eyes open; he does not want any chemist in the field. I don't hold with Mr. Dymond's statement as regards the wide variation, as a little study of the growth of the cane acts as an essential guide as to what constitutes sampling. Probably, as Mr. Bechard has said, it is a specialised job. I think it is. If you send any junior out he may take 50 samples and get

different figures. I have taken three samples in a truck and produced figures varying from 1% to 4%. It shows what can be done by anybody who spends a little time finding out what constitutes field growth. As regards field sampling generally I consider it is of very little service.

CHAIRMAN: I heartily agree with Mr. Dymond's suggestion that we should take at least four samples to get a reliable test. It may be that Mr. Booth with his long experience could pick a representative sample of cane that thoroughly represented the stage of maturity of that cane, but it is not altogether a question of maturity of cane. It is affected as we know by the numerous differences in composition of soil and so on. I don't see how it is possible to get any one sample which you can feel assured is representative enough for chemical analysis. The only way to do it to my mind is to do as Mr. Dymond suggests; and take at least four samples to find out what your experimental error is. That implies a good deal of work I know, but unless that is done it seems to me the whole procedure of cane sampling and analysis is labour in vain.

Mr. H. M. JACOBS: I would like to express the view of the Committee. The idea they had was that unless some method could be found for an accurate sample of cane being taken by an unskilled person, field sampling of cane ought to be discarded from our methods altogether. At the Committee meeting I quoted an example where a planter had had some cane rejected. This was on the Umhlatuzi flats, and the ripening period on the flats was late. This planter had a consignment rejected and he wanted to change his field. He sent a sample of an adjoining field, and this sample, taken by himself, tested over 12%. He thereupon put in a big burn in this field and sent a consignment to the mill, in fact it was two days' consignment. They were duly rejected. I agree that had we been able to give that planter any reasonable information about that consignment of cane he would have approached the field, at any rate with caution, whereas he went at it like a bull at a gate and he had two large consignments of cane rejected.

Mr. BOOTH: He sampled that cane?

Mr. JACOBS: Yes.

Mr. BOOTH: Enough said. I may say that I never accept a planter's sampling.

Mr. JACOBS: That is just our point.

Mr. BECHARD: In the last three years I have probably dealt with 150 field samples in each year; some of that sampling has been taken by planters, some by myself, and my experience is that is that all samples I have taken have been slightly better. If your sample is well taken it gives you a fair

indication. So far as planters' samples are concerned, I found them unreliable. Some planters deceive themselves altogether. They top it much lower than they will top eventually, select the ripest looking sticks, and so on. Field samples are totally inadequate and very often are a source of danger to the planters.

Mr. DYMOND: There is another factor which must be borne in mind when comparing the results of hand samples against the actual results obtained on milling that cane, and that is that cane on being crushed may have been subjected to a fire of varying intensity, while a period of anything up to five days may have elapsed between cutting and milling, during which time it may have been subjected to any variation of weather conditions. The cane, therefore, on milling, may not bear the least comparison with that originally existing in the field. I may add from my own experience that cane with an original fibre per cent. of 14 can be altered by fire, and hot weather over five days to a fibre content of over 22 per cent.

Mr. JACOBS: I would like to make the proposal that we leave field sampling of cane right out of our methods and discourage it altogether.

Mr. MOBERLY seconds Mr. Jacobs' proposal.

Mr. DYMOND: I would like to add that where field sampling has to be done at least four samples should be taken and each analysis recorded.

Mr. BECHARD: I would like to add a further proviso that no samples be taken unless by a responsible chemist.

Mr. Jacobs stated he could not agree with the proviso.

Mr. RAULT: Does not the proposal of Mr. Jacobs throw some doubt on the work done at the Experiment Station in testing canes by field samples?

CHAIRMAN: It means that the tests are unreliable unless on the average of a large number of samples such as Mr. Dymond suggests, and we always do so wherever we place any weight on the results. This resolution only affects the procedure of chemical control.

Mr. RAULT: For years past the practice at Natal Estates has been to test the canes from field samples previous to cutting. The figures obtained are not considered strictly accurate, but afford a very good indication of the average sucrose in the district at that time, and also a guide as to the degree of maturity in individual fields which are to be cut. As many as 200 samples may be tested. Our experience has been that on the whole there is a fair agreement between the average analysis

obtained from these field samples and the actual tests from the canes sampled at the factory a fortnight or two later. On that account we cannot altogether discontinue the practice of testing canes from field samples, as this practice serves a useful purpose. We, however, agree with Mr. Jacobs that such tests should not be considered as final on account of possible variations.

CHAIRMAN: That is just our point, that with a large number of samples it gives some indication, but with small numbers of samples it is valueless.

SECRETARY: In the changes in terminology now recommended by the Chemical Control Committee, such as pol. instead of apparent sucrose, it strikes me there may be some collision with the terminology of the Fahey Agreement. If that happens to be so it might cause difficulty in the future. Perhaps Mr. Jacobs would let us know whether the change of terminology proposed will have any important reflections on the terminology of the Fahey Agreement in so far as it deals with the analysis and method of payment?

Mr. JACOBS: I think that could be obviated by including the old definition in any new one stating that this used to be the definition. Including the old definition would make it quite clear that this is a new definition.

Mr. MOBERLY: I don't think the term "sucrose % cane" is affected.

Mr. JACOBS: No. So far as I can recollect the only possible place it can come in in the Fahey Agreement is in the method of carrying out planter's tests. The rest of the methods are not defined.

SECRETARY: It is not convenient at the moment perhaps to deal with this, but it seems to have a certain amount of importance.

Mr. JACOBS stated he would look this point up during the lunch hour.

CHAIRMAN: This is not the final form. It is being submitted to this meeting for your approval. The intention is to have further committee meetings to decide on the final form in the light of the resolutions of this Conference and then produce these official methods as a separate handbook issued in a convenient form for the use of the sugar chemists generally.

Mr. JACOBS: With regard to hand samples taken at the mill from trucks, I would like to propose that the method mentioned in Mr. Moberly's report, towards the end of page 3, be adopted in case it should ever be required.

Mr. MOBERLY: Perhaps it would be advisable to leave that over until some definite move is made in the matter of fibre.

Mr. JACOBS: I refer mostly to the clause in the Fahey Agreement which says that cane may be rejected on hand sample analysis.

Mr. MOBERLY: For that purpose I think it might be accepted.

Mr. JACOBS: I think we should adopt this method. The method in the Fahey Agreement is quite inadequate.

Mr. BECHARD: Take the recommendation on bagasse sampling. I don't know how this is going to work out. If there is a stoppage it is going to complicate matters severely.

CHAIRMAN: It was felt that the existing methods led to the possibility of inaccuracy due to times when stoppages occurred being purposely avoided, and consequently the sampling of bagasse taken at those times only when there were no stoppages would not be representative of the others. Therefore we decided to recommend as an alternative that the sample be taken at definite fixed times whether there was a stoppage or not. Only in that way we thought we could get representative sampling.

Mr. BECHARD: I don't like this. I would much prefer that the matter be left over. It is quite worth while the work of a different Committee being appointed on the question of sampling bagasse. More could be done in the matter of mechanical sampling of bagasse.

Mr. MOBERLY: With regard to automatic sampling, I have seen automatic samplers which might well be allowed to stand. But I see the difficulty that not all factories operate to the same degree of skilled supervision, and allowing of automatic samplers might be very well in some of the leading factories, but I know, without mentioning names invidiously, that there are some places where it would be a dangerous thing to allow.

Mr. JACOBS: In the Committee we purposely left out mechanical samplers for bagasse because of the differences in types of the milling plants we have in this country. In a very big milling plant it is possible a mechanical sampler could be arranged but when we consider very small plants which operate under the Fahey Agreement where the bagasse is not uniform in size, then I contend that a mechanical sampler would never give you a correct sample.

Mr. BECHARD: I am sorry to have to come back on the subject, but I still think that much work could be done on the subject, and if some suitable form of mechanical sampler could be obtained we could have them submitted to the Committee for consideration.

Mr. RAULT: The constant danger in bagasse analyses is usually to show a sucrose which is on the low side, inasmuch as the sucrose determination is carried out on a sample which does not always include a proportionate part of the large and imperfectly crushed bagasse. Where the mill work is very irregular, and particularly where catch samples are the rule, there is a great temptation to neglect the partly crushed bagasse, resulting from a choke or a light feed, the bagasse from a full load feed being considered as representing the average work of the mills. Such a system of sampling is wrong, as it eventually reflects on the sucrose per cent. cane, which is made to appear lower than it would correctly have been, had the total sucrose in cane been determined on the whole cane, previous to its separation into juice and bagasse. It is not surprising to find factories with poor mill work having a tendency to record low Java ratios. Mr. Moberly, who is in daily contact with all the local conditions at various mills, should be able to give us his opinion on the relation between mill work and Java ratio. For the above reason we favour continuous mechanical bagasse samples.

Mr. MOBERLY: In general that is very largely the case. In small mills where inefficient working is done the Java ratio tends to be low. Our highest ratios come from the large mills where more efficient crushing is done. I think an examination of the next paper shows that.

CHAIRMAN: With regard to Mr. Bechard's proposal, do I understand you wish to have this subject referred back to the existing committee or do you propose the formation of another committee?

Mr. BECHARD: It is quite immaterial. I thought probably the Chemical Control Committee would nominate a sub-committee to deal with the matter. I believe that at one time a sub-committee was appointed to study that question but did not carry it through.

CHAIRMAN: A sub-committee was appointed for the question of weighing of bagasse. It is down in the proceedings as the committee for the weighing of bagasse. I was under the impression at the time that they were going to tackle the question of sampling also. However, the question has been very fully discussed by the Committee on Chemical Control, and at present the opinion is that they are against automatic sampling for the reasons Mr. Moberly has brought forward. If you care to move a recommendation that the matter be referred back, of course they will endeavour to do so.

Mr. BECHARD: What I am really afraid of is friction which is going to exist between the engineers and the chemists.

Mr. JACOBS: May I ask Mr. Bechard if it is his intention that we should investigate automatic sampling to make it compulsory for everyone?

Mr. BECHARD: No, not compulsory—at least not at first.

Mr. JACOBS: It could then still be put in as an alternative method.

Mr. BOOTH: Has no one in the room experience of mechanical samplers to give the meeting?

Mr. DYMOND: I think an automatic sampler is perfectly accurate in high-powered mills where the final bagasse is even and well broken up, but in the small mills I consider the proposition impracticable. My experience with an automatic sampler has shown that when the milling is even and good, the sucrose in bagasse follows the sucrose in cane very closely, while irregularities can always be traced to some fault in the milling work. Where a close control of this work is required an automatic sampler of bagasse is indispensable.

CHAIRMAN: Shall we leave the subject, that the method of automatic sampling as an optional or alternative method be recommended for the consideration of the committee?

Mr. BECHARD: Yes, that would meet the case.

Mr. MOBERLY: There is a point in these sampling methods which has come to my notice since this report was prepared, and that is the sampling of mixed juice. We have the case of those mills who are working with the pre-heating of juice. We say here where this is possible this must be automatic, and the sampling at the tanks is allowed as an alternative, where that is not practicable. Where you are going to have any degree of heating of juice and the possibility of evaporation during weighing it would be inadvisable to have weighing at one point and sampling at another. The difficulty I know exists in the hot sample evaporating, but I know one factory where the sample is kept cool; it is taken at the hot tanks and the sample is kept in running water.

Mr. BECHARD: I would like to endorse Mr. Moberly's opinion on this subject. We could lay down that juices should be sampled in the same state and temperature as weighed.

Mr. Jacobs seconded the proposal.

Recommendation noted for guidance of committee.

CHAIRMAN: There is another subject we have referred to this meeting, and that is the analysis of the mixed juice, whether we are to use single polarisation or double polarisation, or by the Clerget method. The position is summed up in the report of the Committee that Mr. Jacobs read to you, and we would like to have the feeling of the general body of members.

Mr. McRAE: During the past year quite a number of chemists have voiced their opinion that in connection with the analysis of mixed juice it might be better instead of making a composite four-hourly sample and doing the Clerget sucrose determination on that sample, to replace that by doing an hourly sample of mixed juice and doing direct polarisation only on that sample. They have come to this conclusion in view of the fact that in very many factories very small differences have been recorded between Clerget sucrose and the apparent sucrose. Another thing which has influenced them in this is the fact that the Clerget determination of sucrose is a chemical analysis, a real chemical analysis, and to determine that correctly one has to adhere very strictly to the conditions laid down, and we feel that in ordinary routine work it is impossible for the assistants always to fulfil these conditions, and that in the long run we get more accurate results by doing say four direct polarisations instead of the one polarisation every four hours with the Clerget sucrose. Of course we have to face the fact that the Clerget method is more accurate than the other. On the other hand, under the circumstances, it is the opinion of many chemists that a direct polarisation is sufficient considering the fact, as I said before, that in a large number of factories the difference between direct polarisation and Clerget is practically nil. Under these circumstances the Committee thought it as well to bring the matter up here so that we could have the opinion of all chemists on this point. It is an important point to consider as you know, because of the payment to planters on sucrose content. The determination of sucrose in mixed juice is the crux of control in the factory. It is the important thing and we have to consider which is the most accurate method for its determination.

Mr. BECHARD: What Mr. McRae says is very true, but I am afraid that we cannot alter it. I think that it is already provided for in the Fahey Agreement, that the determination shall be done by the Clerget method. If that is not so I would like it very much. It has been my experience that with composite juice samples tested four-hourly, unless the greatest care is taken in cleanliness between planter's laboratory and miller's laboratory. At some times in the past I have had occasion to record very large differences. I will say that afterwards when the necessary care was taken we got some remarkably concordant results between the mill laboratory and the planter's laboratory. I would like to endorse Mr. McRae's opinion if it is possible without prejudicing the Fahey Agreement to test the mixed juice every hour for direct polarisation instead of four-hourly. I think it would be much better.

Mr. JACOBS: I think the reading of the Fahey Agreement is that the sucrose in cane shall be determined by the methods recommended by the Sugar Technologists' Association.

CHAIRMAN: If it can be proved that the difference between the two methods is so small as to be within the limits of experimental error, in other words, that there is no practical difference between the two, the question can be rapidly solved. We have had one or two instances where it is the case. Is there any member who can give examples to the contrary where both methods have been followed out? Are there cases on record where the difference has been material?

Mr. DYMOND: Over a period of five crops the results at Empangeni have been as follow:—

	Increase over Polarisation.	Decrease.
1925	1.1	—
1926	—	.04
1927	—	.09
1928	—	.02
1929	—	.01

A careful hourly control for one day showed the following:—

Brix.	Sucrose.	Clerget.	Brix Clerget Purity.	Change in Sucrose.	
				Plus.	Minus.
16.5	14.88	14.92	90.3	.04	
17.0	14.84	14.87	87.6	.03	
17.3	14.93	14.97	86.5	.04	
17.25	15.30	15.33	88.8	.03	
16.5	14.17	13.94	84.4		.13
16.2	14.01	13.94	86.2		.07
16.2	14.76	14.76	91.3	.00—	—00
16.7	14.81	14.88	89.3	.07	
16.9	14.71	14.77	87.5	.06	
17.1	15.24	15.21	88.9		.03
15.4	13.66	13.67	88.6	.01	
15.9	13.94	13.90	87.2		.04
15.9	14.33	14.38	90.2	.05	
17.8	15.82	15.77	88.5		.05
16.4	14.69	14.69	89.6	.00	.00
17.2	15.14	15.19	88.5	.05	
16.6	14.55	14.55	87.5	.00	.00
16.8	14.94	14.36	88.5		.08
15.5	13.46	13.41	86.7		.05
17.3	15.53	15.50	89.6		.03
16.8	14.88	14.86	88.6		.02
16.4	14.46	14.40	88.0		.06
16.95	15.35	15.36	90.6	.01	
16.50	14.92	14.87	90.0		.05
Av. 16.62	14.73	14.71	88.5	—	.02

CHAIRMAN: The point is they are within the limits of experimental error.

Mr. DYMOND: Certainly over such a large number of analyses. One might admit .03 in one analysis but not over several hundreds.

Mr. BECHARD: I think this difference depends on the time of the year. Week after week the standard difference is .02. It is very seldom there is any appreciable variation from that standard at the end of the season. The figures quoted by Mr. Dymond are practically the ruling figures right through the Industry. If you compare that with the difference in purities as between four-hourly and hourly samples I think there would be much wider differences. We had differences in purities of 1.1, .85 and .86 in three seasons, although I will say that for one-half of last season the difference was .1. This difference between Clerget and Direct has very much less influence than the differences caused by keeping the juice four hours in very many cases.

Mr. RAULT: Speaking about differences in purity, one should remember that purity being a ratio where two variables enter, namely, Brix and polarisation, it is quite possible that these differences may be accounted for by the Brix determinations and not at all by loss of polarisation due to souring. Knowing the highly air-emulsified juices that are tested when dealing with raw cane juice, one could expect that occasionally the reading of the Brix spindle is taken too early with resultant lower figures than the correct one. Such a mistake is bound to be of more frequent occurrence in the planter's laboratory where samples may have to be tested every twenty minutes. On the other hand, the mill laboratory sample, with its period of four-hourly tests, is very free of such mistakes, the remedy of which is very obvious. Cases of difference in polarisation call for more serious remedies.

Mr. BECHARD: I have the figures for the last season. The brix, compared within .01, the biggest difference for any period in the whole season in purity, was .6. The difference in purity in the last half of the season, September to December, was .1 in purity. It might have been one way or the other. As it happened, this .1 difference in purity in the last half is reflected by .05 difference in brix.

Mr. DYMOND: What preservative was used?

Mr. BECHARD: Formalin.

Mr. JACOBS: Do you know if tests were made to see that the samples were keeping in the mill laboratory?

Mr. BECHARD: Yes. It has worked out both ways. When cleanliness was adhered to we have had no difference. Under strict cleanliness very little differences have been found between keeping the samples four hours as against analysing it every hour except on very hot days when I found differences in reading polarisation of .5 in the four-hourly sample. That was the maximum difference. But all kinds of figures were obtained, from no difference at all to .5 difference.

Mr. DYMOND: The subject of fermentation was discussed in 1926 and the sampling of juice was brought down from eight-hourly to four-hourly on that account. As Mr. Bechard has stated that formalin was used I am not prepared to accept those figures; formalin to my mind is quite inadequate for keeping mixed juice for four hours during every period of the year.

CHAIRMAN: My experience has been that with scrupulous cleanliness juice will keep four hours under the most severe tropical conditions without any preservative at all. I have seen that carried out in the Cuban factories for the whole season. I know that under their conditions for six months in a very hot climate there was no deterioration of the

juice where scrupulous cleanliness was observed. There was no preservative of any kind used; it was just kept in clean glass vessels.

Mr. BECHARD: I have been studying the question of preservatives during the off-season. I find that too much formalin has an inverting action. I have carried out experiments with juice kept clean and also with inoculated juice kept clean and otherwise. Cleanliness is always the biggest factor. After that, there is not very much left between the different preservatives except perhaps some of the preservatives derived from coal tar. Some of these are very powerful indeed, and will keep any sign of fermentation away from the juice; but then the difficulty is in distributing those very powerful preservatives. I have found no difference between formalin and mercuric chloride. It has always been cleanliness first and preservatives a long way behind.

Mr. BIJOUX: I propose we test every hour instead of every four hours and that polarisation or apparent sucrose be done in place of double polarisation in that case.

Seconded by Mr. Jacobs.

Mr. DYMOND proposes an amendment, that the two methods be optional.

Mr. BIJOUX points out that they must have one or the other.

Mr. MOBERLY agrees that there should be one method only.

Mr. JACOBS stated he could not see anything to stop Mr. Dymond doing Clerget or the other.

Mr. BECHARD proposed that before taking a vote on the matter they should have a look at the Fahey Agreement.

Mr. JACOBS then read paragraph 15 of the Fahey Agreement, after which further discussion took place.

Mr. DYMOND withdrew his amendment.

Mr. FOSTER considered it would be a retrograde step to abolish double polarisation. He saw no reason why they should have any appreciable losses in the sampling. It should be a simple matter.

Mr. BECHARD suggested that instead of abandoning the Clerget method they should adopt a middle course; they could carry on the Clerget every two hours.

The proposal made by Mr. Bijoux, seconded by Mr. Jacobs, that the sampling of juice be carried out hourly instead of four-hourly, which implied single polarisation instead of double polarisation, was then put to the meeting.

Voting 11 in favour, 7 against.

Motion declared carried.

At this stage the Conference adjourned for lunch.

Resumed at 2.30 p.m.

CHAIRMAN: We were discussing before lunch the case of single versus double polarisation, and a suggestion has been made that we should add under the definition of Clerget sucrose on page 2 of the Report (second column towards the end), the following: "Where sucrose is mentioned in the Fahey Conference Agreement the above definition applies." That will account for the great majority of cases where the difference between single and double polarisation is very small. In such cases apparent sucrose may be considered equivalent to true sucrose. So if we add sucrose as mentioned in the Fahey Agreement it will probably meet the case.

Mr. DYMOND: Without wishing to obstruct the progress of the meeting might I ask whether people present who are not members of the Technologists' Association can vote on motions put by this body?

CHAIRMAN: I have not the rules before me, but I should take it for granted that those who are not members are not entitled to vote.

Mr. DYMOND: I beg to state that three non-members voted on the last motion.

CHAIRMAN: If that division is seriously challenged we will have to repeat it. Do you move that we take another vote on that matter?

Mr. DYMOND: This is a serious matter which we decided in a hurry. I contend that the subject should be discussed more fully before we take any drastic step.

CHAIRMAN: Do you suggest postponing a challenge on this division?

Mr. DYMOND: I propose a challenge now; take a re-vote. Seeing that there is a meeting of chemists to-morrow morning at nine o'clock this matter might come up for discussion after that.

CHAIRMAN: The meeting to-morrow morning has been called for a special purpose, and I think the time available will be fully occupied. It seems to me to put the matter in order that you should move the re-opening of the subject.

Mr. DYMOND: I propose that the subject be re-opened. A matter of such importance should not be passed by a mere majority of one vote here.

Seconded by Mr. Bechard, and agreed.

Mr. DYMOND: I have really no objection to either method: hourly polarisation and four-hourly Clerget. Actually from the scientific point of view the Clerget method is more accurate, and I consider that if the samples are adequately preserved, they will keep for four hours even under the worst conditions of infection with mercuric chloride, but certainly not with formalin. Therefore I contend that the Clerget method, which is accepted to be a more accurate method than single polarisation, provided the juices are properly preserved, should be used as our standard method.

SECRETARY: Might I just put in a word here not on the technical side at all but on the political side. An alteration of this kind reflects on the Fahey Agreement, and one ought to be very careful in approving of any change unless that change has so much in its favour that there can be no doubt at all as to its advantages. I need not tell you that the people most concerned from the Fahey Agreement point of view are the planters, and once they become accustomed to a method they will accept that method, but if you change it there is every chance of them thinking that somewhere or other there is a catch in it. It is only for the purpose of cautioning the meeting against making a change which may not have any really substantial advantages, though scientifically it may have. In the particular case in point the difficulty is that in certain cases the method recommended cannot conveniently be applied, and there the direct polarisation method would be much more convenient, but after all the thing has been working, people have got into the way of doing things, and if you are going to change it now you have to remember that you have not only the chemists to keep in view but the planter who is paid under the Fahey Agreement. If there is a very substantial advantage, either in convenience or cost or in time, for making the change, then it is worth while considering; if there is not it would be far better to leave it alone.

Mr. BECHARD: I move that the matter be referred back to the Committee for their consideration.

Mr. JACOBS: I hardly see how you can do that. The Committee have already referred it to the general meeting.

Mr. BIJOUX: As the one who proposed the hourly direct polarisation method, I should say that so far as the Clerget is concerned it does very well in certain places, but I think that very often one notices differences that should not arise, and we have to accept these as normal, especially at night when checks are out of question. It is very difficult to obtain concordant results, and I think we should carry on with the hourly direct polarisation as it is easier, and as there is not much difference between the results of the two methods. With even careful preservation, it is very difficult to keep the juice

from going sour at certain times of the year, and this is what leads me to propose shortening the time of analysis in adopting the hourly direct polarisation method.

Mr. MOBERLY: I think we have got to put out of our minds all ideas as to which is going to give higher results and which is going to benefit one side or another. It has to be done entirely on the matter of accuracy, and as the Secretary has said, to consider whether the change is well worth while. I have been in favour of this change, not being so much concerned with the comparison between the two polarisations as the advantage we are going to get from hourly analyses. If it were feasible to do hourly double polarisation that would be the ideal. That being in most laboratories out of the question, we have to pick the better of the two alternatives, and I am inclined to believe that the error which you get from deterioration during the last three hours is greater than the difference between the two methods, which experience has shown us in the great majority of cases is very slight and hardly material. It is to be admitted that under ideal conditions of cleanliness the four-hourly sample would give as good a test as the hourly one; unfortunately we cannot rely on ideal conditions being attained at all places. Very conscientious work is done at some factories. I myself am quite convinced that a good deal of sucrose is lost through deterioration in polarising samples. We can't get away from the fact that we have to contend with conditions which in many cases are not as good as they ought to be. The only thing we can insist on and lay down the law on is that these methods outlined here should be followed. If the methods allow of hourly testing instead of four-hourly, we can insist on the hourly test. You say you should insist on cleanliness, too; that is a very dubious thing. You can insist on it, but you can't always get it. Purely from the point of view of accuracy and getting figures nearest to the truth the advantage lies in having the hourly test.

Dr. HEDLEY: Why not put the matter to the vote now?

CHAIRMAN: If the meeting is agreeable we are certainly in order in having another division since the first one has been challenged on the ground that they were not all members who have voted.

Mr. BECHARD: Before you do that I would like to propose an amendment that you combine the two methods: that two-hourly samples will be done by the Clerget method.

CHAIRMAN: Does that compromise meet the case, doing the sample every two hours and continuing the double polarisation?

SECRETARY: May I draw attention to this point? The position seems to be that certain in-

formed opinion says that in the four-hourly tests with double polarisation the deterioration is of no importance; on the other hand, it is said that in the four-hourly test deterioration is of importance, and that it is corrected by an hourly direct polarisation. I don't see how a scientific body are going to decide that question by a show of hands. It seems to indicate further enquiry, and a body like this should take that course and enquire further.

Mr. BECHARD: I think the enquiry has already been made on that subject. The question is one of practicability. On the one hand we know that in general laboratories we can do the four-hourly Clerget. But the contention is that the amount of error by fermentation in certain cases in four hours is greater than the hourly amount of error without Clerget. On the other hand, the ideal would undoubtedly be to do the Clerget every hour. Well, why not compromise, and, as I have suggested, do the Clerget every two hours? We cut down the possibility of fermentation by more than half and we keep the Clerget method, which is the correct method. There is another difficulty, and that is our position with the International Sugar Technologists' Association. It would look rather funny on our part to drop the Clerget after advising it. We don't want to make ourselves a laughing stock.

Mr. FOSTER: The whole thing seems to boil down to preservation of samples. The majority of those proposing polarising every hour emphasise the fact that you can't rely on samples keeping over four hours. I can't say that I have found that to be the case. Personally I have found that you can keep the samples. I therefore think you should give this question of preservation of samples further consideration before you take any steps.

Mr. DYMOND: I wish to support the Secretary in saying that an enquiry should be instituted on the question of deterioration of samples in this country.

CHAIRMAN: Although the Chairman of the Committee on Standardisation of Chemical Control has told you that it cannot be referred back to them, it is difficult to see any other alternative at present. Would you not be prepared, Mr. Jacobs, to consider it again after having heard the very conflicting opinions of this meeting?

Mr. JACOBS: I think it is throwing a lot on the Committee. When we started on this we felt it was something bigger than we could handle, and we referred it to the general meeting. Now that the general meeting refers it back to us it places us in the same position.

CHAIRMAN: It is a technical point which is very difficult to handle in a general meeting. It is essentially a point that can be better dealt with by a committee.

Mr. JACOBS: I have no objection to the methods standing as they are for another year pending investigation by the Chemical Control Committee, which will be reported next year.

Mr. BECHARD: If the Chemical Control Committee felt it was rather too big for them to handle, the obvious thing for them to call in extra help and get the opinion of a more representative body of chemists.

Mr. JACOBS: What I meant to say is that at the moment we feel it is more than we can handle, but if we are given another year I think we can collect enough information to enable us to express an undivided opinion at next Congress.

Mr. BECHARD: Then I will put my motion to refer it back to the Chemical Control Committee.

Seconded by Mr. Foster, and on being put to the meeting this was agreed to, the decision on the previous motion being thus cancelled.

Mr. MOBERLY: I would just like to mention something in connection with this change to the use of the word "saccharimeter." The word "pol." remains; it is only the name of the instrument itself which is being altered. The use of it is still polarisation. The reading obtained will be known as the "saccharimeter reading."

CHAIRMAN: The polariscope, strictly speaking, is an instrument which measures angular rotation in degrees and not on the scales such as

we have in the saccharimeter. Consequently we have decided to follow international practice and refer to the special type of polariscope as a saccharimeter. As Mr. Moberly says, it does not affect the use of the term "polarisation," which is the per cent. of sucrose obtained by the instrument. There is another matter I would like to call attention to, that is with regard to the analysis of molasses. In some factories they are determining the brix by diluting with five volumes as in the methods for control. The method was intended purely for sugar-house control and not intended to give a direct or accurate reading of the brix where it is a question of sale of the molasses outside the factory. This is a question which has arisen between Mr. Kloot, the Borough Analyst, representing the buyers of molasses, and certain factories, and although it is not a matter bearing directly on chemical control, we thought it as well to include this clause. Of course even dilution with an equal weight of water does not give a strictly accurate density of the mixture, but is sufficiently close for all practical purposes. The most logical way is to use the refractometer and calculate the results from that. Mr. Kloot said he would be here this afternoon to discuss this question, but unfortunately he has not turned up. The difficulty as you know is that molasses very often contains a large number of air bubbles which are very difficult to get rid of in the refractometer determination. We feel that the refractometer method is the best conveniently applicable.

Mr. BIJOUX: On the question of molasses I think the Committee should publish a table of concordance.