ON THE SUITABILITY OF NATAL SUGARS FOR THE MANUFACTURE OF SWEET DRINKS

By G. S. MOBERLY, E. P. HEDLEY and B. E. BEATER.

The following investigation was initiated as a result of a report received from refiners in Canada to the effect that certain manufacturers of soft drinks had complained that sugars refined from South African raws were unsuitable for this purpose, owing to their giving rise to a gummy ring in the neck of the bottle, and to the formation of a flocculent suspension in the liquid.

Enquiries from local manufacturers elicited the fact that these phenomena were well known in the drinks manufactured in this country, but that they were observed only in the case of drinks manufactured from certain essences, in fact some manufacturers of essences advertised essences specially guaranteed not to produce a gummy ring in the neck of the bottle.

The following experiment was carried out by the writers at the Experiment Station.

Eight different types of sugars, including two imported sugars, were selected. For the purposes of the experiment these were numbered as follows:

S1. Tate & Lyle's Cubes.
S2. Hulsar M.
S3. Hulsar No. 1.
S4. Hulsar B.
S5. Illovo Suchar Refined.
S7. Cuban Refined.

From each of these a syrup containing 6 lbs. of sugar to the gallon was made (this being the strength used in manufacture). In each case a certain amount of suspended matter was observed in this syrup, S1, S2, and S5 showing the least. S6 though nearly clear, showed more colour than any other sugar except S8. S1 showed a number of fine hairs in suspension, and these could be seen under a magnifying glass embedded in the cubes.

Six typical essences and colouring matters were selected and numbered as follows:

E3. Ringless Orange Essence.
C7. Red Colouring Matter.
C8. Orange Colouring Matter.

E3 was one of the above-mentioned essences guaranteed to give no gummy ring.

A number of specimen drinks were then made in ordinary lemonade bottles, each type of sugar being made up with each type of essence or colouring matter. Eight sugars and nine essences, etc., thus made seventy-two mixtures. In addition each sugar was made up with water only (without essence), and each essence was made up with water only (without sugar), thus making an extra seventeen bottles, or eighty-nine in all. The mixtures were made up as follows:—In each bottle was placed 1½ oz. of syrup containing from ½ oz. to 1¼ oz. of essence per gallon, as prescribed by the manufacturers. The whole was then filled up with water and aerated with CO2. The colouring matters were powders dissolved in glycerine according to manufacturers’ directions. No citric or tartaric acid was added.

The bottles were arranged in order as shown in the accompanying diagram. In a couple of days, rings began to show in the necks of certain bottles. These increased from day to day, but little change was noted after one week. The rings took the form of a gummy or “gelatinous” precipitate round the neck of the bottle, together with a slight floating scum which appeared to consist of an emulsion of essential oils from the essences.

The above diagram shows the order in which the bottles were arranged for the purpose of the experiment. The letter R denotes the mixtures in which the gummy ring was found.

It will be seen that in the case of the Essences E1, E2 and E4, rings were formed with every sugar, whereas no sugar showed a ring with any other essence or colouring matter. No rings were formed in any of the water series. The above would appear to indicate that the fault lies with the essences rather than with the sugars. S8 was a mill white sugar, i.e., a sugar still containing an appreciable amount of the colloidal matter of the juice. Even in this case the ring was formed only in the case of Essences E1, E2 and E4. It is interesting to note that of the essences giving the ring, two were of the cloudy type and one was clear. Of those giving no ring, one, (E3), was cloudy and the rest clear. The cloudy principle in the essence cannot therefore be blamed for the phenomenon. The
ring would appear to consist of a fine emulsion of essential oils. Minute drops of which could be observed in the floating scum on the surface. No rings were formed in the case of E6, which was a synthetic preparation, nor in any of the colouring matters. The presence of sugar was apparently necessary to produce the ring, but the type of sugar seems to be immaterial.

In no case was any colloidal suspension observed beyond that already present in the syrups or the natural cloudiness of the essences E1, E2 and E3. It was observed, however, in the commercial product made from E5 and S5. It consists of fairly well dispersed flocculations, which however, disappear as soon as the liquid is poured out of the bottle.

No preservative was used in the experiment, so that by the end of three weeks fungus growths were present in all the bottles containing sugar. The experiment was therefore discontinued, but as no new rings had formed after the first few days, it seems probable that they would not have formed at all.

It would be seen to be clear from the above that the formation of gummy rings is due, not to the sugar used, but to the essence. No further light was thrown on the formation of a colloidal suspension observed by the Canadian manufacturers. Local manufacturers express themselves as entirely satisfied with Natal sugar for their purposes.

The thanks of the writers are due to Messrs. Dalys, Ltd., of Canada Road, Durban, for their interest and assistance, which included the supply of essences, etc., the loan of bottles and the aeration of mixtures.

Experiment Station,
South African Sugar Association,
Mount Edgecombe,
Natal.
March, 1931.

Mr. BLACKLOCK: The experiments made in conjunction with this paper are on very similar lines to those carried out by Mr. Barsdorf at the St. Lawrence Sugar Refinery, in 1930. The results however, are somewhat different. The Natal investigators give the comforting assurance that other sugars besides those of South African origin produce flocculation. Whilst Barsdorf showed that four sugars out of ten did not produce this condition, of the six remaining those refined from Natal Raws were in the worst class as regards flocculation. The paper before us does not bear out this statement as regards comparison, but certainly proves the production of flocc. No conclusive data have been found in either case to suggest the cause of this phenomenon in beverages of this kind and Barsdorf makes the statement that "general excellence of a sugar does not guarantee freedom from flocculent tendencies in the products." The whole thing is very indefinite and there is evidently opportunity for more research work in this connection.

I notice that in each case when making the syrup a certain amount of suspended matter was noticed. My own work at different times has led me to believe that this slight haze or opalescence caused by suspended matter is a sure indication of the after separation of flocc in sugar solutions or filtered liquors. Very often this haze consists of reversible colloids which have become desiccated at the high temperatures attained during drying the sugar, and which afterwards gradually absorb water and become more highly dispersed, eventually agglomerating into clots or flocs owing to some change in their electric charge—or some pH change in the solution.

It would be interesting to know if any comparative pH tests were made on the beverages experimented with.

CHAIRMAN: Unfortunately these pH determinations were not made on these tests. As Mr. Blacklock has pointed out our results do not at all bear out those made by Mr. Barsdorf in Canada, which makes the matter rather more interesting and makes one wonder what the particular differences in conditions of experiment were. But our results were quite definite and undoubted.

Mr. DODDS suggested that in view of the discrepancy between the results obtained here and those published by Mr. Barsdorf he should be sent a copy of the paper and asked for his comments, and that if possible further work should be done in collaboration with him to get the matter cleared up.

CHAIRMAN agreed this would be a very good thing to do. He had been informed that the Soft Drink Manufacturers in America maintained a Chair of Research at the Iowa State University, and he thought it would be a good thing if the Association got in touch with them and brought the problem to their notice and asked if they could assist.

Mr. BECHARD stated that about ten years ago the same problem cropped up in Mauritius. A leading soft drink manufacturer there complained about one of the sugars supplied to him. It would probably be very interesting to get in touch with the Agricultural Department at Mauritius and find out the result of the investigations made.

Mr. BIJOUX stated that he believed the case in question was due entirely to the sulphur content of the sugar, and he asked if the Committee investigating this matter had any idea of the sulphur content of the different sugars used in the tests.
DISCUSSION ON REPORT OF CHEMICAL CONTROL COMMITTEE.

Dr. HEDLEY: This report, as is the case every year, consists of such a mass of figures and formulae that it is an exceedingly difficult report to read and an impossible report to understand when you hear it for the first time. Nevertheless I venture to say that no other Committee has sat so often during the course of this last year nor has devoted so much time to the subject which it has under its control. The Committee had 8 sittings, totalling 30 hours in all, and at the end of those sittings there were many hours of work for the Convenor, Mr. Moberly, who did his work very well I think, and apart from the convenor there were other members who carried out work at home drawing up appendices, specifications, etc., which they submitted to the Committee. Some explanation of the changes which have been made will help to make this report more interesting and intelligible. You will see that for the first time we give diagrams of flasks on page 13. I don't propose to say much about them. Mr. Dymond has more to say about it from his personal experience than it is possible to say here. Two flasks have been recommended. Specifications have been drawn up which will help you to get the correct type of flask. The reasons for this are really more in the hands of Mr. Bechard and Mr. Dymond because they discovered, as we also discovered at the Experiment Station, the inadequacy of the flask which was sold by certain makers at Home. I make a point of that because we have been accused of dealing with trivialities too often, and that is not true. In the report under the heading of "General" it is stated "It has been decided to make a series of comparative tests of the hydrometer, refractometer, and pycnometer methods at the Experiment Station." Mr. Hayes has been occupied with this work and when this work was undertaken by the Experiment Station it was expected that we should be able to find a constant difference between the 1-1 dilution and the refractometer readings. Such a constant difference does not exist. For instance one refractometer reading gave 81.94% of solids and the pycnometer reading was 66.0. Another pycnometer reading was 74.8 and the refractometer 80.9. In other cases the reverse is true. This will show you that we are not in a position to give you any constant factor between the two methods. Further work will be done and a report submitted to this Association. Now under the heading of "Volumetric Glassware" we include what may look like an academic change but this is really an important matter. It says "The unit of volume adopted is that volume occupied by the mass of one kilogramme of pure water at its temperature of maximum density 4°C. and under normal atmospheric pressure; this volume is termed the litre. The recognised international metric units therefore become—the litre (1) and the millilitre (ml) or thousandth part of the litre." Now the use of the term "millilitre" (ml) to replace cubic centimetre (c.c.) has been recommended by three bodies—"La Troisieme Conference Generale de Poids et Mesures" in Paris 1901; The National Physical Laboratory, at Teddington, and lastly The Joint Committee for the Standardization of Scientific Glassware. These Commissions are in historical order; the French drawing attention to discrepancies first, being followed later by the English. On the English Committee sat representatives of the great scientific Societies of London—The Chemical Society, the Society of Chemical Industry, the Institute of Physics, the Medical Research Council, British Medical Association, British Engineering Standards Association, the Institute of Chemists, and there were also represented Manufacturing concerns such as British Chemical Ware Manufacturers Association, British Laboratory Ware Manufacturers Association, Association of British Chemical Manufacturers, etc. and lastly a Government Department—the Board of Trade. Altogether 21 scientific bodies and trade interests were represented on this joint Committee for the standardization of Scientific Glassware. They considered it necessary, quoting their own words, "to give much careful consideration to the question of units of volume, and as a result of their deliberations they unanimously recommend:

"That the recognised international metric units—the 'litre' (1) and 'millilitre' or thousandth part of the 'litre' (ml)—shall be used as the standard units of volume, and that standard..."
volumetric glassware shall be graduated in terms of these units and marked “ml” instead of “c.c.”

They issued a pamphlet of six pages dealing with the question and we see a further result of their work in that glass apparatus is being marked “ml” and no longer in “c.c.’s.” The reasons for the change are briefly as follows:—The definition of the litre is:—the litre is that volume of pure water occupied by a mass of 1 kilogramme of water at 4°C. The cubic centimetre is:—the volume of a cube the edges of which are one centimetre in length.

These two definitions are independent of each other and the relationship must be determined by experiment. It works out that 1 litre = 1,000.027 cubic centimetres, or a difference of 27 parts in 1,000,000. So small a difference would not matter for most purposes were it not for the fact that the cubic centimetre has also been extensively misapplied to denote the volume of a quantity of water having an apparent weight in air of 1 gramme (Mohr’s system) and when a “1,000 c.c.” flask is graduated on this basis at 15°C. it would actually contain 1,002 true cubic centimetres. The inscription “1,000 c.c.” has thus lost its original precise significance. The inscription “1,000 ml.” on the other hand is perfectly definite. It is the volumetric glassware of foreign manufacture that is causing this confusion, as the glass may have been calibrated in either of the two “c.c.’s.” Further confusion is occasioned by the fact that Mohr’s litre is the amount of water at 17.5°C., weighed in air with brass weights, it has the apparent weight of 1,000 grammes.

It must therefore be evident to you all, gentlemen, that the introduction of the term millilitre is not an academic point but one rather of serious import, especially since many sugar mills buy their glassware out of England. Were the matter of small import it can hardly be imagined that so distinguished a Committee would occupy its time on the question and publish a long pamphlet on the subject.

In connection with the Chemical Control Committee’s report, the 13th Annual Report of the Bureau of Sugar Experiment Stations, Queensland, is of interest. Firstly it is worthy of notice that this report is issued as a Parliamentary White Paper. It is the report of the Director to the Secretary for Agriculture, which shows that the Australian Government has an interest in the station’s work. In Queensland, too, they have found it necessary to seek after greater accuracy in their chemical work. A Standards Laboratory has been established “in conjunction with the mutual control scheme.” Here brix spindles, flask capacities, etc., are checked, the object being to have uniform standards in all the mill laboratories in this control scheme. This control scheme embraces 21 factories who co-operate with the division of fortnightly distribution of manufacturing results. This control is very similar to our scheme and they report “the results of the control to date have been very satisfactory.” They say further: “This marked increase in Mill efficiency is most gratifying, and can be wholly ascribed to the general realisation of the fundamental essentials required for the attainment of better results.” This realisation has been born at Conferences of Mill Managers, Engineers, and Chemists, convened by the Government.

I might mention that I had a talk with Dr. Guthrie, the Chief Science Officer of the Colonial Sugar Refining Co., Sydney. This Company has eight factories and six refineries, also one distillery, employing altogether 100 trained chemists. Each year they select two of their best men and give them a full University training. Further, the Company has always followed the policy of having one man a year travelling the sugar countries of the world picking up information for them. Sometimes as many as three men are travelling at once. This gentleman himself has visited at least once for his Company, India, Java, Honolulu, America, Holland, Germany, England, and Fiji. The Colonial Sugar Refining Co. further recruits annually from the Sydney schools one or two of the best pupils. They never consider it good policy to cut down salaries to economise, but look to their large research staff for advance and progress. They have also got a travelling staff which is continually visiting the factories, keeping an eye on the scientific work and rendering special assistance when called for. Dr. Guthrie laid special stress on the value of the visiting of other sugar countries as a means of keeping up-to-date and abreast of progress.

CHAIRMAN thanked Dr. Hedley for his interesting remarks, and said it was very good to hear that South Africa had been able to give Australia a lead in regard to the formation of a Technologists’ Association. He then asked for expressions of opinion on the proposed changes in the determination of sucrose in juice as set out in the report. This, he said, was a very important change. Last year they were unable to come to a decision and the Committee had now brought a definite recommendation and wanted to know if it was acceptable?

Mr. DYMOND stated that with regard to the addendum to the report which had been read, he thought it should be mentioned that special care should be taken in pipetting off the proportional amount of juice weighed. Pipettes should of course be washed out with the liquid hourly, and that was a point that should be stressed.

Mr. BECHARD stated that at Darnall three years ago they had had a lot of difficulty with the mixture of juice and had eventually come to the conclusion that it was due to the type of glassware in use at the time. Since then they had discarded everything which was not at least 5 centimetres
above the mark. He preferred 7 centimeters as 5 was a little bit short.

The CHAIRMAN then exhibited types of new glassware recommended and explained the advantages to be obtained.

Mr. CHRISTIANSON stated that while the direct polys were being made temperatures varied enormously and a very careful control was required in doing the direct poly, to try and get it to the same temperature as the indirect poly.

Mr. BECHARD agreed that the query brought out by Mr. Christianson was quite a sound one; but he suggested that the temperatures should all be recorded, and the average temperature of the 8 hourly direct polys be the temperature at which the invert reading should be done.

CHAIRMAN asked if it was proposed that temperatures corresponding to that average should be applied, to which Mr. Bechard replied in the affirmative.

Mr. FOSTER said he did not know how far it would apply, but he thought it would be possible to get a check reading on the composite filtrate, a direct poly check. If that could be done, and the sample was more or less representative of the 8 hourly work, it should do away with any objection owing to varying temperature. On the other hand, as the method stood, they were only going to use the Clerget applied to each hourly sample; therefore he did not think any variation of room temperature would affect the results.

CHAIRMAN: That is not quite the case that we are going to apply a Clerget difference to direct poly reading, the average is going to be used, together with the invert reading, in working out your Clerget sucrose. In the formula D - 1, D is going to represent the average of those readings, and 1 the invert reading: Therefore this temperature difference is a significant one if D represents one temperature and I another. You have an error coming in there, but as far as taking a direct reading on the composite that can be done as a check, but it was not adopted as the method because even though the samples are preserved with lead we want to guard against any deterioration which would set in during the eight hours. Any inversion on the sample used for the invert reading does not matter; but any inversion which took place would affect the direct reading. We were therefore not proposing to use the direct reading on the composite but on the original.

Mr. BECHARD: Before we go further I would like to stress the opinion of the Committee that this method was only a temporary expedient to get over the difficulty of the insanitary conditions obtaining at certain laboratories at the present moment, and we are looking forward to the time when it will not be necessary to do samples every hour. The results of Mr. Dymond's experiments have completely shown to us that you can obtain conditions under which 4 hourly sampling is a deftently good one, and I think I represent the opinion of all the Committee when we say the Mills where efficiency is carried out are not prepared to carry for any length of time the "lame dogs."

Mr. POUJNET stated that if the hourly poly is accepted it would mean that all other things would have to be done in the same way—crusher juice, last mill, and all the rest of it. It was going to increase the work in the laboratories considerably.

CHAIRMAN: This change was only intended to refer to mixed juice, and was necessitated by the very great degree of accuracy which is necessary in mixed juice as one of the constituents of sucrose in the cane. As far as extra work goes it is true that you will now do four hourly polys, where you previously did one. However, the hourly poly is not a very lengthy business, and to level it in some way we have half the number of invert readings. An invert reading is a lengthy business, so that the extra work put in is very largely countered by the decreased work at the other point. It is not necessary to apply this to all other juices, but we do feel in view of the importance of getting absolutely accurate figures for sucrose in cane, and therefore sucrose in mixed juice, we could not have anything better than the method explained.

Mr. H. M. JACOBS: It seems to me that this temperature difference is going to present a very serious obstacle in adopting the method of determining sucrose in mixed juice. Allow me to read from page 18 of the report—"Invert (Saccharimeter) Reading." It says definitely that the direct and invert readings should be made at the same temperature, a maximum difference of 1° C. being permissible in routine work. I do think that would be a very difficult thing to accomplish in all the readings performed in these nine tests.

CHAIRMAN: It is very hard to say anything definite on a point like this when one has not one's text books handy to refer to. It certainly is a point which will want very serious consideration before we adopt the method finally, as it just occurs to me that the variation in temperature may prove on study not to be such a serious matter because the temperature variations would affect mainly the direct reading and the direct poly is not subject to the same temperature fluctuations as in the invert reading, and as the invert reading is the one done last the temperature is accurately taken and allowed for in the inversion formula. Whether a variation in the direct reading is going to make an appreciable difference I can't say offhand. With reference to this 1° C difference mentioned I don't think it was inserted because we found that 1° C was the limit of accuracy beyond which the formula broke down, but we considered that was a degree of accuracy
which could be quite fairly demanded when two tests were made at the same time and two temperature readings taken at the same time. However, it was proved on examination that this variation is a matter of considerable importance. The method I presume that we would adopt would be to take the average temperature readings of the direct polarisation and try to do the invert at the same temperature. That unfortunately is not a very easy matter. It is quite easy to cool juice a bit in running water, but to get an exact standardisation of temperature without a thermostat requires a little care, and we don't want to make this method too complicated. So I would like you to give your consideration to this important point raised by Mr. Jacobs and to hear any observation which any member has to bring up, and see whether we can overcome it or whether further investigation is necessary before we can say whether it is feasible.

Mr. BECHARD: This point of temperature variation is closely bound up with the question of the Herzfeld factor. At present we have had to leave the factor more or less in abeyance awaiting the results of the experiments at the Experiment Station. If any difference is registered on account of temperature this difference would affect the factor.

Mr. CHRISTIANSON: I find the temperature of tap water ordinarily does not change very much and if the direct polarisations are made in a water jacketted tube it should be possible to maintain a general temperature.

Mr. WATSON: This is proposed in the same paragraph in the report.

Mr. CHRISTIANSON: Yes, but the point is that room temperature varies enormously and it would be better to work to the water temperature.

CHAIRMAN: For room temperature there you really should use “tap water temperature.”

Dr. HEDLEY: Would it not be better to alter the wording to this and say circulating the jackets with water? Only “filling them” is demanded; but if the water is circulated it brings the juice down to that temperature.

CHAIRMAN: What action do you propose should be taken about this? It comes to you as a recommendation from the Committee and it is up to you to pass it, revise it or refer it back. I would remind you that this has already been referred to the Committee twice, so don’t refer it back because you have doubts in your mind. If you feel there is further information necessary then your course would be to refer it back, but if it is just a matter of difficulty in deciding what to do I would ask you to make your decision now and not ask the Committee for the third time to do it.

Mr. H. M. JACOBS: I propose that this paragraph be amended so that the last sentence reads “the most practical way is to work at room temperature using water-jacketted tubes circulating water through the jackets at tap water temperature.”

Mr. DODDS: If we specify circulating tap water in the jackets of the pol. tubes it will involve in some cases having water connections laid on to the saccharimeter room or chamber which might involve some extra work and expense in some laboratories.

Mr. DRAEGER: We have quite a lot of factors coming in to this, and I think it would pay us all to stick to No. 3 as originally put in and eliminate all these factors.

Mr. DODDS: I think we ought to keep in mind that the whole procedure of inversion in this determination is under revision and the present methods are merely tentative until our new conclusions are arrived at. The position might be simplified considerably by the new method of inversion we hope to work out.

Mr. H. M. JACOBS: In my opinion I don’t think it is a matter that any mills would find serious difficulty in overcoming, to put tap water in the dark room. I think that is an easy matter in most laboratories.

Mr. BOOTH: Arising out of Mr. Draeger’s reference, have you any idea how the voting went from the outside chemists on these three proposals?

CHAIRMAN: Those voting papers were sent out to nearly all the chemists, including members of the Committee, and they were sent back, most of them unsigned. I could give you the aggregate result, but that includes the opinion of members of the Committee, but, however, were divided at that time. Thirty papers were sent out, 26 were returned, of which one was not filled in in the proper way, but of the 25 there was a strong preponderance for method No. 1, which forms the basis of what we have adopted. Of the other two there was a slight majority in favour of No. 3 rather than No. 2. The first method received 33 points, the third 21, and the second 19 points. The third method showed rather a preponderance over the second for first choice. We have adopted the first with certain modifications which were suggested.

Mr. CHRISTIANSON: I do not see that there will be any great difficulty in working out this thing in actual practice and therefore I move that this suggested method be adopted.

Mr. BECHARD: I would like to make a small addendum—“the invert test to be made at the temperature approximating the average of the eight hourly direct polarisation.”
Mr. CHRISTIANSON: If you have circulating tap water you should have no difficulty in getting the same temperature. As a safeguard it is all right.

CHAIRMAN: Mr. Jacobs has already proposed the medium of circulating tap water and the proposal would then be to adopt the method as laid down in the report, with the rider that the invert reading should be done at the average temperature. If circulating water is used the invert reading will automatically come out at the average. There is a slight difference in the two readings. We have to decide whether we simply stick to circulating water and assume that is going to correct your temperatures, or definitely going to bring the invert temperature up to the average of the direct. That point had better be settled first before voting on the main proposition.

Mr. BECHARD: I propose my rider.

Mr. CHRISTIANSON: I think that would be more suitable.

Mr. JACOBS: I second the proposal for circulating tap water.

Mr. WATSON: In this you use the word "should" and not "shall." If you make a definite recommendation we have to stand by it, but here you don't. I reckon we should let this stand as it is in view of the difference of opinion as to arriving at a definite relationship in temperature. Why not let that stand for another year and see if the different laboratories can prove which is the best? I think the wording is very clear.

CHAIRMAN: Do I understand that you propose not adopting this method?

Mr. WATSON: Oh no, I entirely agree with that method. But this temperature difference seems to bring up a great deal of work and attention. If we carry on for another year with experiments in this direction we might be able next year to have something definite.

Mr. BECHARD: If the wording of the section is to stay as it is, with just the alteration about circulating water instead of filling the jackets, I am prepared to withdraw my amendment.

CHAIRMAN: That would seem to reconcile some of these differences of opinion. Mr. Jacobs' proposals don't interfere with the point of that paragraph. The word "should" still remains; it is only a matter of substituting "circulating water" for-water stationary in the tube, and working at tap water temperature instead of room temperature. Otherwise the bulk of that remains the same, and if Mr. Bechard is prepared to accept that, we can adopt that as the method of working in connection with this new method. Does that meet with the feeling of the meeting?

Mr. CHRISTIANSON: It is very important that all should be made at approximately the same temperature.

Mr. WATSON: That substantiates my claim that we ought to leave it over for further experiment.

CHAIRMAN: Would there be any necessity for keeping the juice itself at any definite temperature? In the first stage taking the Brix you already make a tentative correction. The real material point is the polarization. That is covered by the proposal to use circulating water in the saccharimeter tube.

Mr. CHRISTIANSON: You have two factors; first of all the effect of temperature, and secondly the expansion of the liquid.

Mr. BECHARD: I think we would have to introduce "the average direct and invert readings should be made at the same temperature," bearing in mind it is still "should" and not "shall."

CHAIRMAN: Will anyone sponsor the proposal that we adopt the method in the addendum, reading it in conjunction with this paragraph on page 18 altered as Mr. Jacobs suggests? If we get that as a primary proposal then we could get any amendments.

Mr. JACOBS: Before we take that vote, I don't want to get the position further involved, but I must say I hate having a loose method for the determination of sucrose in mixed juice. I would like to see a straightforward clean-cut method, and therefore I propose we change that "should," into "shall." I know it seems funny, but I want you to realise the sucrose in mixed juice is the great thing, the basis for the payment of cane, and therefore we cannot have alternate methods. We must have a clean-cut method of determining the sucrose in mixed juice. Last year we had an argument on similar lines about sampling of bagasse, and we came to the conclusion we could only have one clean-cut method, because it was a determining factor in the payment for cane. I therefore feel I must make the proposal that we change that "should" into "shall."

CHAIRMAN: I must say I am entirely with Mr. Jacobs there; we should have no doubts on the subject. Once we have come to a conclusion that should be an unalterable way. Anything else means a tremendous amount of dispute and trouble afterwards. I think the first thing will be to take that point by itself.

Seconded by Mr. Dymond.

On being put to the meeting, carried without dissent.

Proposed that all the readings be carried out within the range of 1°.

Mr. CHRISTIANSON: I think we are covered by the previous sentence to that — "The direct and
inert readings shall be made at the same tempera­
ture."

Mr. HAYES: If the direct polarisation is taken at a certain temperature laid down as tap water
temperature, the Brix would also have to be taken at that temperature. If the pol. is done at a dif­ferent temperature I think that would lead to trouble.

Mr. JACOBS: I don't think the temperature difference in the Brix makes any appreciable differ­ence to this.

CHAIRMAN: As you notice in the Schmitz Table the Brix differences are very wide, the columns are as much as 0.5 difference from each other, so I don't think the small variations in temperature you would get would really affect the Brix reading or the utility of the Schmitz Table. This alteration now would seem to cover us so far as I can see on all practical variations. Mr. Golding says the ideal is to get a constant temperature. To get a constant temperature is rather difficult. But to say it shall be done at tap water temperature, which does not vary within very wide limits, in my opinion would give you a reading of very considerable accuracy. The temperatures on direct readings are not of very great importance, but the temperature readings on invert are, but they are allowed for in your Clerget formula. This method will obviate any wide vari­ations, and I think only small variations of that sort would really affect your readings. After all most of the direct pol work is done without very much consider­ation to the temperature of polarization, and my own opinion is that if we adopt Mr. Jacobs' suggestion of using circulating tap water in the direct and invert readings we will obviate this difficulty and get a method which will be sufficiently accurate for our purpose.

Mr. GOLDING: Tap water temperature means nothing of course. It has just occurred to two of us talking here that most of the mills derive their water from the rivers. What is going to happen when you start with the water warm in the morn­ing and later the river comes down with rain water from up-country and makes a difference of two or three degrees?

CHAIRMAN: I think there is an intermediate storage which would level out the temperature during an eight-hour period. During that period I think they would be drawing water from the same storage tank. I think this should be read with a certain amount of intelligence, and if wide vari­ations in the tap water were noticed allowance should be made for it. I think Mr. Jacobs' suggestion is one which would meet our needs at any rate for the present. We could try it, and if it proves impracticable then the Committee will have to get down to it again and give you further advice. I will now put Mr. Jacobs' proposal that we change this to read circulating water through the jackets at tap water temperature.

Agreed unanimously.

Mr. BECHARD then proposed, and Mr. JACOBS seconded, that the method of the Committee set out in the addendum to the report, be adopted.

Agreed unanimously.

Mr. BLUETT: I suggest that all hydrometers should be standardized.

CHAIRMAN: I would draw your attention to page 1 of this report, top of the second column. (Reads.) That I think meets your question. Of course all these instruments should be carefully standardized at the beginning of the crop, and if there is any dispute they should be referred to the Experiment Station for verification. If the Experiment Station standards show they are wrong they should be considered as wrong, even if some Bureau of Standards in Germany or other country says they are right. It does not mean that we throw much doubt on the work of those Bureaux, but we must have one central authority. We might get the position where one Bureau would certify a standard as correct and another as wrong. Our proposal is to have one central standard of working.

Mr. POUIGNET: On page 3 you define Bagasse. (Reads.) There are two definitions there. Does it mean that they are just the same?

CHAIRMAN: I take it the weights would be the same except that the only variation would be the amount of water left in the maceration bath at the moment you happen to take the reading. Other­wise all water put in to the bath would pass on to the cane in the same way as water sprayed on it. We have allowed for both, and in making those calculations you take the weight of the water.

Mr. DODDS: Should that not read "maceration or imbibition water"?

CHAIRMAN: It should be amended to read maceration or imbibition water.

Mr. HAYES: On the same page it says: "Im­bibition Water: The water applied to the bagasse during crushing. This may be either weighed or measured in bulk, making the necessary volume corrections where hot water is used." It would seem that the Technologists' Association had some say in approval of the various methods of weighing or measuring in bulk, and the Government Depart­ment of Weights and Measures have approved one method of measuring water which has been found in various mills to be not quite up to scratch, the Lee Recorder. I think it is up to the Technologists' Association having approved of these methods of weighing and measuring of water, to discuss the question as to the suitability or otherwise of the Lee Recorder.

CHAIRMAN: It is a matter which requires in­vestigation. Unfortunately there are criticisms.
which can be levelled at the Lee Recorder. I think it is a matter which will get further attention in the future.

CHAIRMAN: There is one point I would like to mention. We have considered the Report of the Committee on Chemical Control, passed the main items in the report, but the report as a whole was not formally passed. As this is an important thing and actually affects our working I would ask someone to propose that the report be adopted.

Mr. BECHARD proposed that the report be adopted.

Mr. RAULT asked what would happen in the case of a miller declaring that he had not the staff to do what was proposed and he still continued with the four-hourly test?

CHAIRMAN: That is a difficult point, but if it is a factory working under the Fahey Conference Agreement they have undertaken in that agreement to carry out certain things and to carry out the methods of this Association. If this Association's methods have been confirmed by the Millers and Planters then I take it that that factory would be obliged to follow those methods or else it would constitute a breach of the agreement. I stand open to correction on that.

SECRETARY: Might I suggest that this probably could be adopted by this Association and then it becomes a subject for representation to the Millers through the deputation that has been arranged, and the same course could be taken with the Cane Growers. Then any question of effect on practical working at mills would be the subject of discussion.

Mr. Foster seconded the adoption of the Report, and on being put to the meeting this was agreed to unanimously.

At 12.50 p.m. adjourned to 2.15 p.m.