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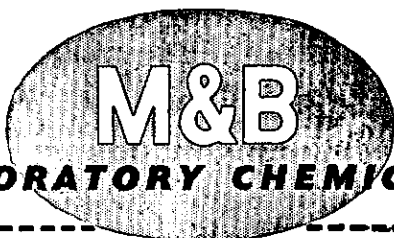
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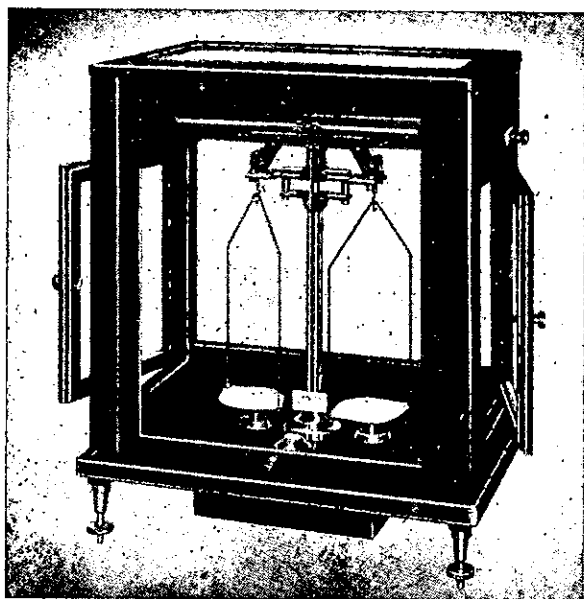
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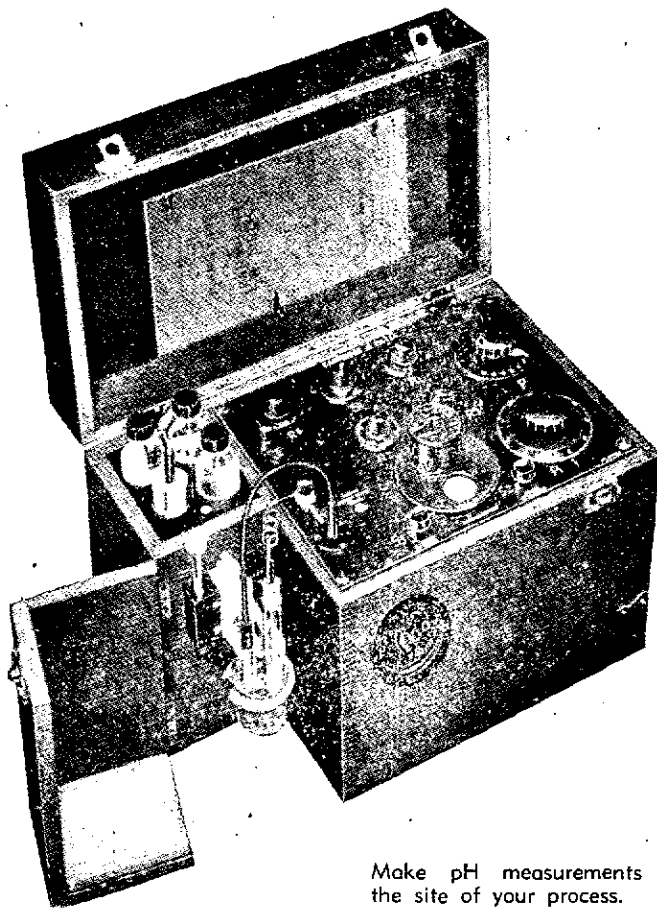
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IMPRESSIONS OF CONFERENCE, 1950

The first impression of Conference was of the cold weather with which Christchurch greeted us while we waited in the cloisters of Canterbury University College for the official opening and photograph. One could almost tell, by their reactions, from which district members had come, from the frozen Aucklanders to the comparatively comfortable Dunedinites. However, as Mr. Siemon, Chairman of the Canterbury Branch, and Sir Ernest Andrews, Mayor of Christchurch, expressed the hope in their welcoming addresses, our hosts made up in the warmth of their welcome for the coldness of the weather.

It seemed a particularly happy choice that Professor Evans, first President of the Institute, should open Conference. His address, particularly his reminiscences of the early days of Canterbury University College, and his remarks on the excrescences now appearing among the buildings of the College, were well appreciated.

The matter of papers varied in quality, but compared with previous conferences the delivery of speakers was good. There was the usual fault to be found that many speakers had more material than they could deliver in the twenty-five minutes allotted. Full use was generally made of the ten minutes allowed for discussion after each paper, but one felt in some cases that the discussion was just warming up when time was up. Credit must be given to chairmen of sessions for the time-table being rigidly adhered to so that one could attend selected papers in both of concurrent sessions.

The projection apparatus was good, and efficiently operated, but the writer agrees with the opinion he heard expressed, that projection of material by epidiascope should be discouraged at future conferences.

At a meeting held during Conference of representatives of past and present conference committees, to discuss the conduct of future conferences, it was surprising to hear the variety of views held on this matter. The decision of this meeting that conference committees have a free hand, as in the past, should, over the years, give sufficient variety to cater for most tastes.

The two presidential addresses and the well-balanced symposium on Isotopes were the highlights of Conference. It was a good Conference, and one has returned from it stimulated, not only by the papers read, but by friendships renewed and contacts and discussions with workers in similar fields.

G.W.S.

SOME ASPECTS OF PHOTOSYNTHESIS

Presidential Address delivered to the Annual Conference, Christchurch, August, 1950, by Dr. J. Melville, Plant Chemistry Laboratory, Palmerston North.

The duties attaching to the honourable position of President of the N.Z. Institute of Chemistry are neither numerous nor onerous—your General Secretary sees to that in a most efficient manner. But there is one task which the Secretary, regardless of how efficient he may be, cannot remove from the President's shoulders, and that is the preparation and delivery of the Presidential address. The incumbent has all the world to choose from for his subject, since there is no phase of our material existence in which the chemist is disinterested or to which he cannot make some contribution. With that background it is only natural that he should talk on some topic in his particular field of scientific endeavour, and it would be rather surprising if I did not talk on some chemical aspect of plant activity. It is here that the strictly logical sequence breaks down, since in choosing photosynthesis I shall be talking on a subject which has not been investigated in my laboratory and on which no work is contemplated. My reasons are two: it is the most important process in this world of ours, and some very exciting work has been done, and advances made, during the past ten years.

Now it is obviously impossible in the course of an hour's lecture to deal with the whole subject and describe other than in barest outline, the present state of our knowledge of this very complex process. Moreover, the physical chemists and the physicists have been adding an ever-increasing quota to the sum total of our knowledge and their work is such (or rather my limitations are such) that I could give no more than generalised conclusions. Hence when I first considered the matter in the knowledge that I had to get something down on paper, I had thought to restrict the discussion to one aspect only, viz., the path of carbon in photosynthesis. That is still my intention, and the major part of the next hour will be spent in a consideration of the steps through which a molecule of carbon dioxide must go before appearing in the leaf as a recognisable carbohydrate or near relative of a carbohydrate. But I think that in addition it is desirable to paint, even though very sketchily, a general picture of photosynthesis which will form a background into which can be fitted the more detailed consideration of the reduction of carbon dioxide.

To that end I should like to state quite categorically what I have already briefly mentioned, viz., that the word photo-

synthesis is used to describe the most important chain of reactions in the world. I could give the reasons for that statement in my own words, but I would not succeed in being so graphic and so brief as is Rabinowitch in the opening sentences of his book on the subject, "Photosynthesis."

"The chemical reactions which constitute the material aspect of life all take place on a precariously high level of potential chemical energy. Like acrobats performing their complicated exercises high above the circus crowd, the molecules of proteins, fats, carbohydrates, vitamins, enzymes and other constituents of the living organisms combine, exchange, or dissociate in the midst of an ocean of oxygen which continuously threatens them with breakdown and extinction. Oxygen atoms, reluctantly united in diatomic molecules, are ever ready to break away from each other and to seek stability in the union with carbon, hydrogen, phosphorus, iron, or the other elements contained in organic matter. The inorganic world has long since succumbed to a similar attack—so completely that now an average atom in the earth's crust is held in the grip of two atoms of oxygen. Living matter, however, has escaped the same fate by its remarkable capacity for regeneration. Every day, almost a thousand millions tons of organic compounds are destroyed by oxidation, finally to pass into the air as carbon dioxide and to return as water to the universal moisture which surrounds and permeates all living things on the earth. At this rate of destruction, all organic matter now present on this globe will be consumed in the next ten to twenty years; but during the same period, an equal quantity of organic matter will be created, i.e., oxygen will be expelled from its stable union with carbon and hydrogen, and the liberated atoms knitted together into the intricate patterns which spell the secrets of organic growth, propagation, heredity, sensitivity and mobility—all the properties which distinguish living organisms from inanimate objects of the mineral world.

"The continuous renewal of life on earth requires that its chemical elements, scattered by the decomposition of organic matter, be brought back into combination; and that the energy which was thereby converted into heat be replaced. The regeneration of organic matter can occur by a cyclic process—the same carbon dioxide, water and nitrogen which were liberated by organic decay, can be used again for synthesis. The liberated energy, however, is lost beyond recovery. Living organisms no less than inanimate nature, are subject to the laws of thermodynamics. These laws decree that once the energy liberated by oxidation has been dissipated in the vastness of the atmosphere and the hydrosphere it has become practically unavailable for the reverse conversion into chemical energy. Thus, the energy required for organic synthesis must come from an external source—and the only external energy which continuously reaches the surface of the earth is sunlight. No thermodynamic restrictions stand in the way of a complete conversion of light into chemical, electric or mechanical energy; but it requires a mechanism able to intervene immediately after the light strikes the absorbing surface, and to prevent the energy of this impact from being converted into heat. Man has not yet solved this engineering problem; but he would not be here if other organisms had not solved it for him long ago. Those organisms are the green plants, i.e., the chlorophyll-bearing plants. The process by which these plants synthesize organic compounds from carbon dioxide and water, with the help of sunlight, is, beyond doubt, the most fundamental of all biochemical reactions."

I think you will agree that Rabinowitch makes the point

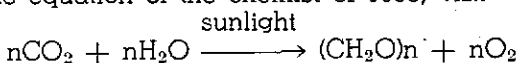
both forcefully and concisely, and in doing so provides the background which I mentioned earlier. Hundreds of chemists have written thousands of papers on the properties of the reduced carbon compounds which form the basis of living matter; the whole science of biochemistry relates to the way in which these substances are oxidised, reduced, aminated and deaminated, methylated, and demethylated, and so on *ad infinitum*. But it must always be remembered that without that one process, viz., the replacement of the oxygen atoms in carbon dioxide by hydrogen atoms and hydroxyl groups, through energy derived from sunlight and applied through the leaves of plants, then such investigations would be impossible both through lack of materials and of course through lack of biochemists to investigate them. And as to the desirability of that latter consequence, I am not prepared to make a statement.

Now let us go on to the main topic of this address—a consideration of what happens to a carbon dioxide molecule after it enters a leaf cell. That is a question for which an answer has been continuously sought over the past one hundred years. Already by the turn of the century, when sugar chemistry was receiving its greatest stimulus in the shape of Fischer's classic researches, a number of investigators were interested in the soluble carbohydrate levels in similar leaves which were kept in sunlight and in the dark respectively. Already the idea was current that the simple mono- and disaccharides, e.g., glucose and sucrose, were the first products of carbon dioxide assimilation; and that from these relatively simple compounds the plant went on to construct its cell walls and hence its structural components by some sort of polymerisation process. Simultaneously by another set of reactions nitrogen was incorporated into the molecules with the subsequent synthesis of proteins which were the basis of new protoplasm. As a further illustration of the scientific acumen and technical skill of our biochemical forbears, they had established at least fifty years ago that there occurred in the overall process a carbohydrate deficiency; that is, that the soluble sugars found by analysis in a leaf which had been depleted over a long dark period, and then had been briefly exposed to bright sunlight, was always less than the carbon dioxide absorbed by that leaf. And the conclusions they drew were what we still believe to be the correct ones, viz., either that in the processes whereby sugar is synthesized in the leaf, some intermediate is drained away from the system for other synthetic purposes; or that even in the short period of the experiment some of the sugars which were formed had already been

used for the synthesis of compounds which did not appear in their sugar analyses.

And there the matter rested for nearly forty years. That is not to say that no further work was done on the first product of photosynthesis. On the contrary dozens of papers appeared giving sugar analyses of leaves and of the green chlorophyllous algae under a wide variety of environmental conditions. Considerable arguments developed as to whether glucose or fructose or sucrose was the first sugar to make its appearance. Some investigators came to the conclusion that starch was the first recognisable product because of the way in which starch granules appeared in and disappeared from the chloroplast in light and in darkness, and that the soluble mono- and disaccharides resulted from its subsequent hydrolysis.

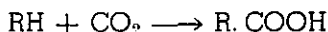
Insofar as this phase of the investigation of photosynthesis is concerned, I think it may be fairly said that the organic chemist of 1935 would have agreed without too many reservations to the equation of the chemist of 1900, viz.:



(Just as digression at this point it is necessary to remember that although on this fundamental question little progress was made, enormous strides were being made during this period with other problems of the photosynthetic process. For example, Hans Fischer during the '20's and '30's was making his classical experiments with the porphyrins and gradually building up the knowledge on which the structure of chlorophyll is based. Warburg was also making his contribution to the efficiency of utilisation of incident radiant energy in the production of organic compounds.) What was required before a new fundamental step could be taken was that new tools should become available or that some advance in another field of science could be applied to this particular problem. Actually both factors were operative. The new tool was a gift from the physicists and consisted in the application of isotopes to chemical reactions. As is already known to every member of this audience, and as will be abundantly illustrated in a symposium later in this conference, isotopes may be used to mark particular atoms and to follow those atoms step by step through complicated processes. But isotopes would have been relatively useless if during the thirty years between 1910 and 1940 enormous strides had not been made in the science of biochemistry. Those three decades saw a spectacular increase in our knowledge of the mechanisms through which carbohydrates, fats and proteins are degraded and synthe-

ised and of the catalysts through which the multitudinous reactions of living cells are channelled and controlled. One result was the realisation that in the total oxidisation of sugar to carbon dioxide and water by a reversal of our original equation, a whole host of reactions are involved. Glucose does not just oxidise: it breaks down step by step through phosphorylated derivatives in a series of alternate pathways, some involving the evolution of energy and some not, but all of them under the control of an equal number of enzyme systems. Conversely it was tacitly agreed that the overall process of photosynthesis could be broken up into a series of intermediate reactions, and many investigators during the first decades of this century tried to establish the nature of these intermediates.

Then in 1939 Ruben, Kamen and their co-workers used the tool provided by the newly-constructed cyclotron, viz., the radio-active carbon isotope of mass 11. By allowing leaves or preferably the unicellular algae, **Chlorella**, to photosynthesize in carbon dioxide in which some of the carbon atoms were C^{11} , they hoped to be able to get unequivocal information on the first organic compounds formed. They were working under major difficulties. C^{11} has a half life of only 20 minutes and this meant that experiments must be limited to four or five hours. In that short period they had to carry out the photosynthesis experiment, make a suitable extract of the algae, carry out the necessary fractionation and perform their final radioactivity measurements. Naturally their conclusions were highly tentative ones, but they considered that the first detectable product of photosynthesis was a relatively complex compound with a molecular weight of the order of 1000. They concluded, too, that the primary reaction is a carboxylation:



Before the work was interrupted by the war Ruben and Kamen discovered another isotope of carbon, C^{14} , which has a half life of about 5000 years and whose use would obviously remove the time element as a limiting factor. These investigators were unable to exploit their discovery: firstly, the cyclotron which was the sole source of radioactive carbon isotopes gave only low yields of C^{14} ; secondly, scientists with a knowledge of radioactive isotopes, whether from the point of view of utilisation or production, were allotted a very different task during the years between 1941 and 1946. Hence it was mid-1946 before any serious attempt was made to continue this highly promising line of investigation, and it was only fitting that it should be continued in California. Ruben was killed during

the war, and it was left to a former associate, Melvin Calvin, to carry on the good work. My story from this point on is devoted almost entirely to a consideration of the work of Calvin and his associates at Berkeley since they have produced the lion's share of results in this field over the past four years. Calvin had one great advantage over Ruben in that C^{14} could be produced easily and with high specific activity in the atomic pile.

Calvin's approach was both simple and direct. He worked primarily, with the unicellular algae **Scenedesmus** and **Chlorella**, which can be grown under far more standardised environmental conditions than can green plants. A standardised suspension of algae was allowed to photosynthesize in air containing 4% ordinary carbon dioxide, using a standardised source of artificial illumination. The algae received this treatment until a steady state of photosynthesis, as measured by gaseous exchange, was reached—usually between 30 and 60 minutes. Gas flow was interrupted, and a solution of $NaHCO_3$ containing a known percentage of its carbon atoms as the C^{14} isotope was injected rapidly into the flask. The flask was shaken in the light beam for an accurately measured period of time and the organisms were instantaneously killed by running into boiling alcohol. This technique, which is familiar to all plant chemists, cytolyses the cell, inactivates the enzymes, and brings all low molecular weight metabolites into solution. High molecular weight components such as cellulose and proteins are insoluble and can be centrifuged off. Removal of the alcohol from the supernatant renders insoluble the waxes and pigments and gives a concentrated aqueous solution of the sugars, organic acids, phosphate esters and amino acids with which we are particularly concerned, with only minimum contamination with inorganic salts, peptides and more complex organic compounds. Most important was the finding that for experiments of short duration, i.e., up to 90 seconds, not less than 90% of the radioactivity absorbed by the cells is soluble in 80% alcohol.

Now just as this type of approach would have been impossible without the advances made in nuclear physics, so the next stage in the investigation would have been incredibly arduous and time-consuming without advances which had been made in the microfractionation of complex biological fluids. You will appreciate that the total weight of algae which can be reasonably dealt with in such experiments is of the order of hundreds of milligrams. Hence the amount of carbon dioxide assimilated in the course of a minute or two must

necessarily be of the order of micrograms. Standard techniques of analysis which find their expression in fractionation of the crude extract, concentration and finally preparation of crystalline metabolites or their derivatives are obviously ruled out. The information which is available and the deductions which can be made regarding the path of carbon is derived almost wholly from the technique of paper chromatography developed by the British chemists Consden, Gordon, Martin and Synge during the early '40's. I doubt if ever in the history of biochemistry has a single innovation been so rapidly and widely adopted by investigators in as many fields as has paper chromatography. It has been reported on at Annual Conferences over the past three years and I would not describe it, even in most summary terms, if an understanding of the process were not essential for the subsequent discussion. Very briefly, a tiny aliquot of any concentrated biological fluid is placed near the corner of a large sheet of suitable filter paper and the water allowed to evaporate. The sheet is then supported by one edge in a shallow trough which contains one of a series of mixed solvents, such as phenol-water, butanol-water and the like. The solvent passes slowly through the paper by capillarity and in the course of 16 to 40 hours will have reached the lower end of the paper. In the original solution will be compounds with differing degrees of solubility in the solvent and differing partition co-efficients between aqueous and non-aqueous components of the solvent. These compounds therefore travel at different rates, and at the end of the run will be distributed along a line vertically beneath the original spot. For many purposes this is quite sufficient, and Messrs. Bathurst, Mangan and Butler in Plant Chemistry Laboratory have used unidimensional chromatography quite extensively. However, if a greater degree of resolution is required, the paper is removed, dried, turned through 90° and run again with a different solvent mixture. Instead of a spot as starting point we now have a series of points of origin, and at the end of this second run the individual components of the mixture are scattered over the paper as a series of discrete spots each of whose co-ordinates with respect to the original spot is characteristic for that particular compound. By running known compounds as controls under identical conditions it is possible to identify unknown materials in the original fluid with considerable precision.

Of course it's not quite so simple as that. It is obvious that some method must be available for identifying the individual spots, such as, for example, the ninhydrin colour reaction for amino acids. For the Berkeley workers this pre-

sented no difficulty, since the radioactive carbon could be made to act as a very convenient means of locating the compounds they were seeking. In order to determine how the radioactivity was distributed, they laid the paper on an X-ray film and left the two in close contact until good exposures had taken place. The film was developed and wherever there was a radioactive compound on the chromatogram, there was a corresponding dark spot on the film.

Now at the risk of boring you, let me just summarise what Calvin was trying to do before going on to a consideration of these radio-chromatograms. He brought a standardised suspension of algae into a stable photosynthetic state using ordinary CO_2 . That is, all the mechanisms whereby CO_2 is transformed into organic compounds were fully operative and all the intermediates in the process were present to the extent that represented a balance between their synthesis and degradation. Then the supply of ordinary CO_2 was stopped, and a source of CO_2 which was labelled with C^{14} was substituted for an accurately measured period of time. During this period the labelled carbon distributed itself among the intermediates. Finally the tissue was killed and the intermediates were extracted with alcohol. By determining the compounds into which C^{14} is incorporated, and by determining the particular atoms in each molecule which are radioactive after varying periods of photosynthesis in labelled CO_2 it should be possible to construct a series of curves, compound by compound, showing differential increases in radioactivity as a function of time. By this means an overall flowsheet can be built up showing the path of carbon, as it progresses step by step through the metabolites within the plant.

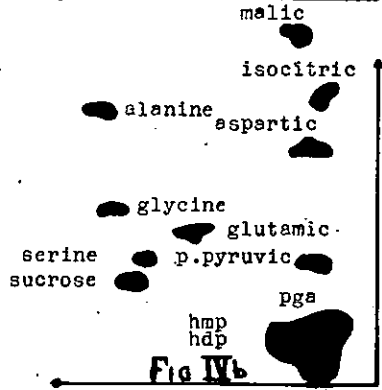
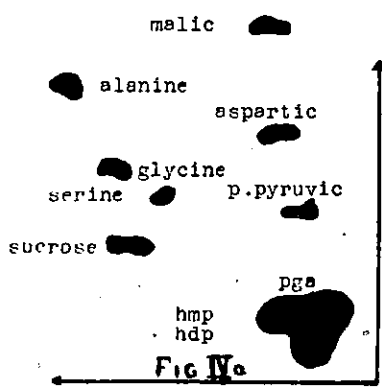
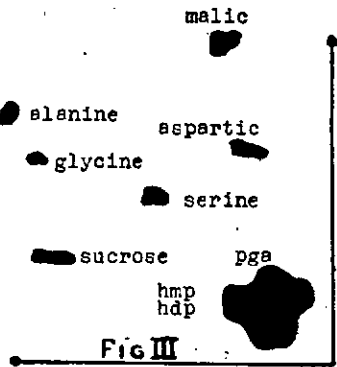
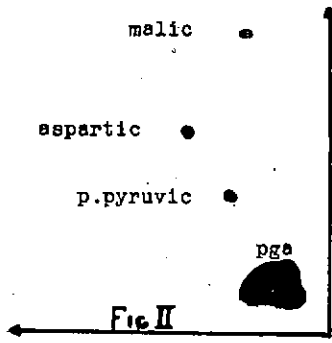
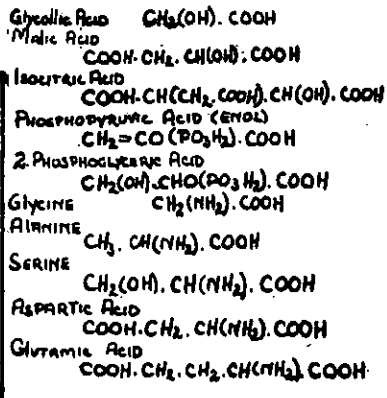
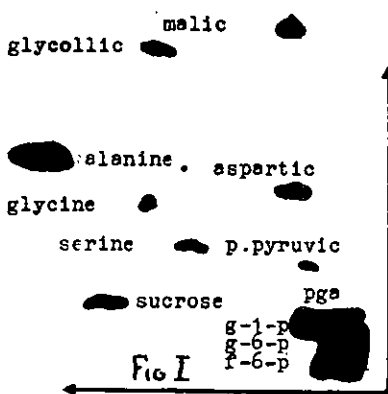
That would be the ideal position: let us have a look at what Calvin has actually achieved to that end. Figure I is a representation in rather idealised form of the radio-chromatogram which was obtained when *Chlorella* was allowed to photosynthesize for 90 seconds in labelled CO_2 . You will note first of all a number of spots scattered over the area of the sheet with a dense irregular spot near the point of origin. The total radioactivity of the sheet represents about 90% of the radioactivity of the washed cells before extraction, so that within 90 seconds some 10% of the assimilated carbon is already in compounds which are insoluble in 80% alcohol. Within the sheet itself, the irregular spot in the bottom right hand corner contains about 60% of the total radioactivity, the remaining 40% being distributed among the rest of the spots on the paper. Considering the latter first, the compounds

identified are alanine, malic acid, serine, glycine, glycollic acid, aspartic acid, sucrose and phosphopyruvic acid. Except for sucrose, I have shown the structural formulae alongside each spot, so that inter-relationships can be clearly seen.

There are several points of interest about these isolated spots. Firstly, sucrose has been synthesized during the course of 90 seconds' photosynthesis, whereas there is no trace of either glucose or fructose. This does not mean that these hexoses are missing from the alcohol extract; on the contrary they are almost certainly there in significant concentration. The important point is that whereas during 90 seconds of tagged photosynthesis no radioactive carbon appears to find its way into these mono-saccharides, a substantial amount does find its way into the disaccharide sucrose.

Secondly, alanine, pyruvic acid, aspartic acid and malic acid are all compounds whose formation would be predicted by reversing the known steps in the biological degradation of the hexoses. Thirdly, nitrogen and phosphorus enter into the chain of reactions at an early stage of the process. In other words the path of carbon in photosynthesis cannot be considered in relation to compounds containing only carbon, hydrogen and oxygen.

This becomes more apparent when we turn to a consideration of the major source of radioactivity near the point of origin. By virtue of the fact that the compounds responsible have not moved very far on the paper, it can be immediately deduced that they are relatively insoluble in the organic solvents employed and are probably very soluble in water. Moreover, earlier studies had led Calvin to the conclusion that sugar phosphates and other phosphate esters were present among the first products of photosynthesis, and he set out to show that the spot near the point of origin was in reality due to phosphorylated compounds. He was able to prove his point very neatly by use of the radioactive isotope of phosphorus, P^{32} . He allowed photosynthesis to take place in the presence of both C^{14} and P^{32} , so that a radiochromatogram would give all the compounds into which either C^{14} or P^{32} had been incorporated. Then in order to distinguish between the two he made use of the greater penetrating power of the P^{32} radiation which penetrates an X-ray film as compared with that from C^{14} which is stopped by the film. The emulsion on both sides of the film will therefore be affected by P^{32} , while C^{14} affects the emulsion on only one side. By experiments such as these, by running control chromatograms with various phosphorylated compounds, by eluting the spot, hydrolysing and re-



C^{14} RADIOGRAMS of extracts of plants exposed to C^{14}O_2 under selected conditions.

pga = Phospho glyceric acid. hmp = hexose mono phosphates
 hdp = hexose di phosphates. q-1-p = glucose 1 phosphate.
 q-6-p = glucose 6 phosphate. f-6-p = fructose 6 phosphate.

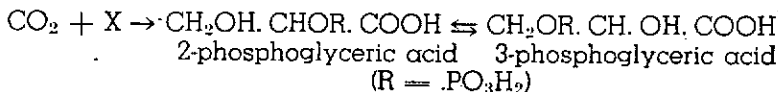
chromatographing on paper, Calvin was able to show the presence of glucose-1-phosphate, glucose-6-phosphate, fructose-6-phosphate, 2-phosphoglyceric acid, 3-phosphoglyceric acid and triose phosphate.

Thus we may sum up this first chromatogram by saying that of the CO_2 assimilated during a period of 90 seconds, only a small proportion appears as carbohydrate, and that as the disaccharide sucrose. Glucose and fructose appear only as their phosphate esters. The simplest phosphorylated compounds are 2- and 3-phosphoglyceric acid, and these with the sugar phosphates represent the major proportion of the assimilated carbon. Simple amino acids and organic acids are present, while there is no compound with one or five carbon atoms.

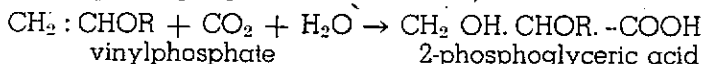
It is therefore obvious that this experiment gives no information about the first compound formed and Calvin progressively decreased the time of photosynthesis in order to answer this question. It was not until he had reduced the time to five seconds that he obtained radioactivity at only two points on the paper and a representation of his results is shown in Fig. II. There were traces of radioactivity in the aspartic and malic acid spots, phosphopyruvic acid was definitely present, but as you can see, radioactivity is virtually confined to the right-hand bottom corner. The compound responsible now becomes a very important one and Calvin went to considerable trouble to establish its identity. His methods cannot be detailed here, but I think he showed beyond doubt that it was phosphoglyceric acid.

Now there are two possible formulæ for this compound according to the carbon atom to which the phosphoric acid radical is attached, and I have already mentioned both 2- and 3-phosphoglyceric acid as being present. But if we are really getting down to first principles, which one is formed first? Calvin got at least a partial answer by allowing photosynthesis to take place at 4°C for 10 seconds. Under these conditions 2-phosphoglyceric acid was the only substance detected, and that is reasonable evidence that 2-phosphoglyceric acid is indeed the first detectable product of carbon assimilation. This finding is a far cry from the formaldehyde which Baly, Dhar and others vigorously upheld as the first product of photosynthesis. Moreover, it is really not a very satisfactory finding, because the relationship between CO_2 on the one hand and phosphoglyceric on the other is not nearly so clear as that between CO_2 and formaldehyde. It is impossible to write a

nice simple equation relating the two and the best we can do at this stage is:



Hence the question is not so much: "What is the first product of photosynthesis?" as "What is the metabolite, already present as a result of previous photosynthesis, which acts as CO_2 acceptor?" Calvin postulates that it is the unknown material vinyl phosphate, and on that hypothesis we can re-write our primary equation this:



His argument is based on inference rather than experiment. The obvious way to prove the point experimentally would be to show that vinyl phosphate can be carboxylated with CO_2 under the influence of an enzyme present in the chloroplast. Then tying the whole thing up neatly would be its demonstration on the chromatogram of the soluble extract from actively photosynthesizing tissue. It is doubtful if such unequivocal proof will be forthcoming simply because of the presumed lability of vinyl phosphate, but there are probably indirect ways of proving or disproving that vinyl phosphate is the primary CO_2 acceptor. At the moment Calvin offers the following in support of his theory:

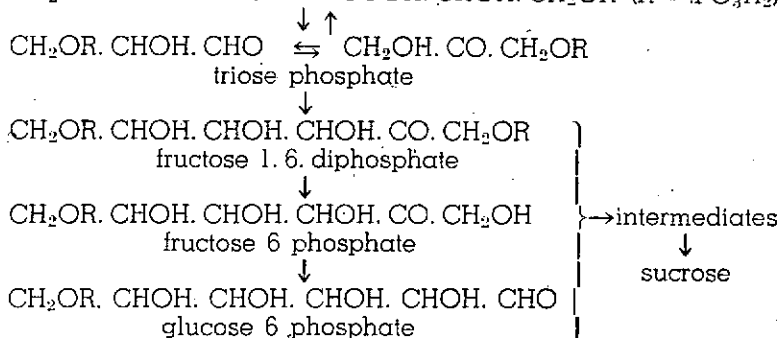
1. It is reasonable to assume that the unknown material should be a 2-carbon compound, since the first product of photosynthesis contains 3 carbon atoms.
2. There is good reason to believe that the 2 carbon atoms in the unknown are not equivalent.
3. The unknown must be capable of continuous production during photosynthesis.
4. Calvin showed experimentally that extracts of algae which had been in active photosynthesis for periods of one minute or longer contained a non-volatile compound which on relatively mild acid hydrolysis yielded radioactive acetaldehyde. In Calvin's own words, "It is difficult to visualise many things which would behave in this manner other than a non-volatile vinyl ester, presumably of phosphoric acid."

I shall return to this question at a later stage, but there are some other simple biochemical matters which have to be cleared up first. We have already seen the difficulties inherent

present in the chromatogram. The reason is not far to seek: oxalacetic acid is a very labile material and does not survive the chromatographic process. In short there can be no doubt of the inherent probability of the way in which malic and aspartic acids appear among the early products of photosynthesis. Incidentally, although this does not enter into the discussion, well-characterised systems lead from malic to fumaric and succinic acids.

Now while we are dealing with materials containing a limited number of carbon atoms, there are three other compounds which put in an appearance on the 90-second chromatogram, viz., glycine, glycollic acid and serine. Here we have no clear-cut series of mechanisms worked out in other tissues to give a guide to the reactions involved. The interconvertibility of serine and glycine is well established, although the mechanisms whereby the transformations take place are obscure.

That disposes of the 2-3 and 4- carbon compounds which have been identified on the original 90-second chromatogram and there remains only the 6 and 12- carbon compounds. Again we see the desirability of phosphoglyceric as the first detectable result of carbon assimilation since it is a well-established compound in the biological degradation of glucose. Starting from 3 phosphoglyceric acid, the transformations are as follows:



The steps in the reaction between sucrose and phosphoglyceric acid are among the most intensively investigated reactions in biochemistry, and there can be little doubt as to their validity in photosynthesizing tissue.

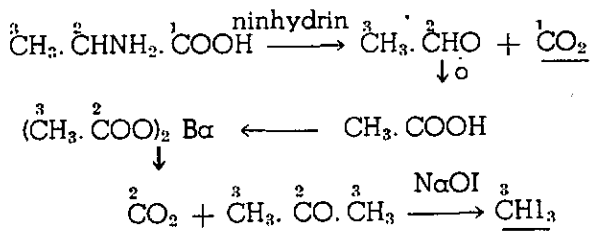
I think that at this stage it would be desirable to give an interim summary of the experimental evidence and of the inferences which have been drawn from it. The first detectable material formed within 5 seconds of photosynthesis in

labelled CO_2 is a 2-phosphoglyceric acid and it is postulated that this is formed by direct carboxylation of the unknown compound vinyl phosphate.

Practically simultaneously 3 phosphoglyceric and phosphopyruvic acids are detected, and their production from 2-phosphoglyceric acid are the result of well-characterised reactions catalysed by known enzyme systems. The synthesis of the 4-carbon atom acids, malic and aspartic, is presumed to occur through the reduction and reductive amination respectively of oxalacetic acid, which in turn is formed by the direct carboxylation of pyruvic acid with CO_2 . Similarly alanine is formed through the reductive amination of pyruvic acid, and the only obscure reactions are those leading to the early synthesis of glycollic acid, glycine and serine. It should be noted also that there are still a number of unidentified spots on the early chromatograms.

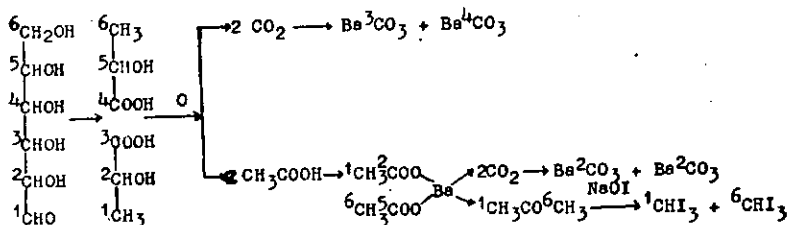
Then in another closely-linked cycle there is the conversion of 3-phosphoglyceric acid through triose-phosphates to fructose 1-6 diphosphate, glucose-6-phosphate and glucose-1-phosphate, all of which are present in the 30-second chromatogram. The interesting point here is that although radioactive hexose phosphates are present, there is no sign of either radioactive glucose or fructose. Finally the only radioactive sugar identified and probably the only one present after 90 seconds is the disaccharide sucrose.

Now let us turn briefly to the second objective in this study of CO_2 assimilation, viz., the differing degrees of radioactivity in the carbon atoms of the various metabolites. Such a study is possible only if each metabolite can be degraded by a series of reactions in such a way that a particular carbon atom and only that carbon atom will turn up in the end product whose radioactivity is to be measured. Two illustrations will serve to show the type of approach, the first being the sample case of alanine:

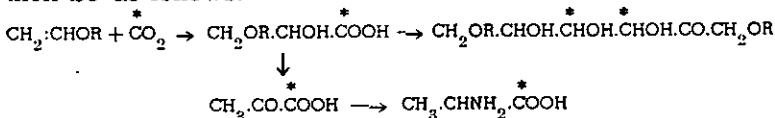


I do not think that any organic chemist would quarrel with that series of degradations for distinguishing among the 3 carbon atoms of alanine.

I have chosen glucose as the other example because of its importance in the mechanisms which we have been considering. Two separate investigations bear on the point. The first by Burr and Wood show that the hexoses resulting from the hydrolysis of radioactive sucrose formed in photosynthesis contains pairs of atoms with similar activities in the members of each pair. That is, there is the same activity in atoms 3 and 4, in atoms 2 and 5, and in atoms 1 and 6. Then in order to distinguish among the pairs Calvin used a biological method followed by chemical degradation:



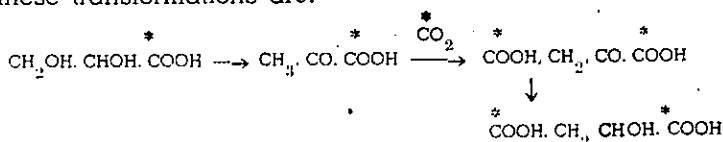
I do not propose to deal exhaustively with the result reported by Calvin and his associates. The argument is too lengthy and, moreover, experimental details are largely lacking in the work published thus far. It is sufficient to have an understanding of the way in which the technique is used and to summarise the findings. Taking the hexoses first, the pair of carbon atoms which first show radioactivity and those which are responsible for the appearance of the hexose phosphates on the 90-second chromatogram are the 3- and 4-carbon atoms. Continuing photosynthesis for a minute or two induces radioactivity in atoms 2 and 5, and finally after 5 minutes atoms 1 and 6 become active. This is interpreted as evidence that 2-phosphoglyceric acid is labelled in the carboxyl group which in turn fits in with the idea that this compound arises from the carboxylation of vinyl phosphate with labelled CO₂: Leaving out the intermediates, the flow of radioactive carbon would then be as follows:



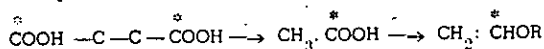
The transformation to alanine is also shown since Calvin was able to demonstrate that the radioactivity of alanine is

confined to the carboxyl carbon, which is further evidence for the distribution of radioactivity in phosphoglyceric acid.

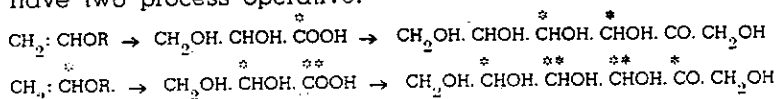
Next we must enquire as to the way in which the other carbon atoms in glucose or fructose acquire radioactivity and here we must return to the other part of the cycle whereby phosphoglyceric acid goes through pyruvic and oxalacetic to malic and aspartic acids. Leaving out the phosphate radicals, these transformations are:



Calvin has shown that malic and aspartic acids in the 30-second experiment contain their radioactivity exclusively in the carboxyl groups, but he has not to my knowledge differentiated between carbon atoms 1 and 4. I mention this because in the diagram I have shown pyruvic acid as condensing with another molecule of radioactive CO_2 supplied artificially to the system. But CO_2 is arising continuously as a result of respiration and there is no reason to suppose that in the 30-second experiment all the CO_2 that is fixed by pyruvic comes from extraneous sources. However, that is not so important as far as this brief discussion is concerned. What is important for the next stage of the argument is that both carboxyls are labelled, and this stage is that vinyl phosphate is formed by the splitting of a 4-carbon acid, either at the level of oxidation of oxalacetic or at the level of succinic or fumaric acids to form two molecules of acetic acid. The acetic acid will of course be labelled in the carboxyl group, and it is postulated that acetic acid is reduced, possibly in the form of acetyl phosphate, to vinyl phosphate labelled in the α -carbon atom. Schematically these transformations are:



This labelled vinyl phosphate would then lead to the formation of phosphoglyceric acid with radioactivity both in the carboxyl group and in the α -carbon atom, and now we have two process operative:



Another turn of the cycle would obviously produce phosphoglyceric acid with all atoms, radioactive, leading, of course,

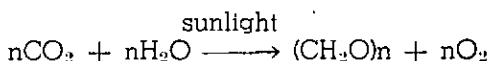
to a hexose with activity right along the chain. That forms a coherent picture and one which explains the available experimental data. But I would stress again that these possible mechanisms and pathways exist on paper only. An enormous amount of work is necessary to show whether they are right or wrong or, indeed, in many instances, whether they are probable or improbable. My object in discussing this phase of the work, viz., the ordered flow of assimilated CO_2 into one compound after another as determined by the distribution of radioactivity within individual metabolites, is to show how powerful a tool it can be, and the kind of results which can be expected from its use.

I hope, too, that I have given the non-biological members of my audience some conception of the interrelationships which are so marked a feature of biochemical transformations. During the last 45 minutes the word cycle has occurred again and again, and it is amazing how accurately that word portrays the sequence of reactions. Every single compound I have mentioned, together with dozens of others which are formed in living cells, are in a state of continuous degradation and synthesis. This applies not only to simple compounds such as malic and aspartic acids, but to molecules as large and complex as those of the proteins. Merely to take one illustration, some of the molecules of sucrose which caused a spot on the 90-second chromatogram would immediately be re-phosphorylated, hydrolysed and broken down again through the glycolytic reactions which have already been described. But in addition there are all sorts of side reactions through which the intermediates may go, with the result that those 3, 4 radioactive pairs may turn-up again in phosphoglyceric acid or in some substance quite outside the glycolytic cycle.

Interesting information is also being derived by varying the conditions under which photosynthesis takes place. The work I have been describing has been done on cells brought to a state of photosynthetic stability, followed by varying periods of photosynthesis in labelled CO_2 . At the end of this second period the cells are killed and the distribution of the labelled atoms determined. Obviously there are a number of changes which can be rung on the variables of light and darkness, presence and absence of CO_2 . The first experiment to be described is of interest not because of the information it gives on the path of carbon in photosynthesis but because of the light it throws on the general photosynthetic process. In endeavouring to get a line on the very first compound produced, Calvin cut the period in light down to zero: that is, he

supplied labelled carbon immediately after the light was turned off. Figure III shows the radioactive chromatogram which he obtained after 15 seconds' exposure to labelled CO_2 , and you will note that the pattern is very similar to that of the first chromatogram I showed. Now this is a very interesting observation. There are two factors which are indispensable to the process of photosynthesis—light and carbon dioxide. Yet here in this slide is indisputable evidence that CO_2 is assimilated through the same channels in darkness as in light. The conclusion is obvious: the process whereby CO_2 is assimilated into organic combination is **not** dependent on the mechanism whereby the energy of light is trapped by the chlorophyll complex and prevented from being turned into heat energy. Actually the finding was not new. In 1936 Van Niel who was studying carbon dioxide assimilation by certain chemo-autotrophic bacteria postulated that the processes of light absorption and CO_2 fixation were distinct and separate sub-reactions of the overall reaction. Ruben and Kamen were the first to test this postulate experimentally and they showed that assimilation of carbon dioxide does take place in the dark in chlorophyllous tissue.

Ruben, however, went a stage further. He argued that if CO_2 assimilation is not connected with the primary process of light absorption, then the oxygen which appears as a result of photosynthesis must come from the splitting of water. Let us go back to our idealised over-all equation.



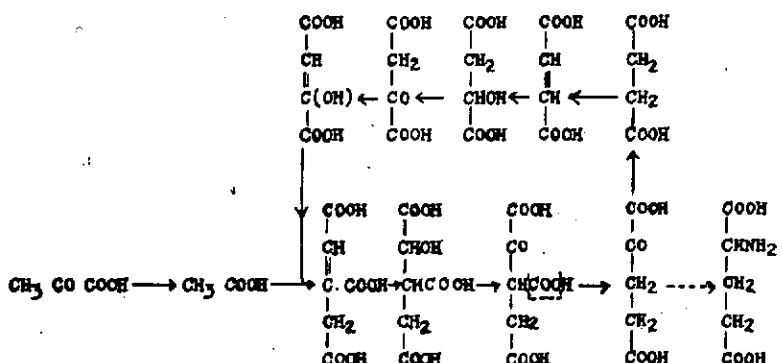
The origin of the oxygen given off by photosynthesizing tissue had long been a subject for speculation. Did it come from carbon dioxide or from water or from both? Ruben and his associates answered this experimentally, by allowing **Chlorella** to photosynthesize in water which had been prepared from oxygen containing an excess of the stable isotope O^{18} . On analysing the evolved oxygen with the mass spectrometer it was found that the ratio of O^{18} to O^{16} was equal to that of the originally enriched water.

You will have realised that this finding is implicit in Calvin's proposals as to the path of carbon. His whole scheme is based on carboxylation reactions in which all 3 atoms in the molecule of CO_2 are incorporated into the CO_2 acceptor. If the oxygen atoms in CO_2 could be shown to appear as free oxygen, then Calvin's proposals would require major modification.

It is reasonable to carry this matter a further stage by pointing out that the primary photosynthetic process, i.e., the one in which a unit of incident light energy is converted into chemical energy, must now be regarded as the splitting of a water molecule into its component parts. The oxygen is liberated and removed from the cell, while the hydrogen is presumably stored in some form which can later be used for the reduction of oxidised organic compounds. Such "reservoirs" of reducing power are well known in other tissues and have been much investigated.

I think you must have been impressed during this lecture by the rapidity with which the various reactions take place. The argument has largely been based on photosynthetic periods of seconds or at the most of a very few minutes. It is therefore of interest to enquire as to how long the cells must be illuminated in the absence of CO_2 in order to give maximum assimilation in the subsequent dark period. The answer according to Calvin is 20 seconds and a longer period of pre-illumination does not increase the amount of CO_2 fixed. Furthermore, this reservoir of reducing power, which can be pumped full by 20 seconds' illumination in the absence of CO_2 is almost entirely exhausted by 60 seconds' exposure to CO_2 in the dark.

There is time for just one more observation coming from this method of approach, and it derives from a different combination of the factors time, light and CO_2 . The cells are again brought to a steady photosynthetic state and treated with labelled CO_2 for an accurately measured space of time. Then instead of killing the cells, the CO_2 stream is switched off and the cell suspension swept through with helium in the light and in the dark respectively. The results obtained on **Chlorella** with 30 seconds' radioactive photosynthesis, followed by 150 seconds in helium in the light and dark respectively, are shown in Fig. IV. You will note that two new compounds appear in the dark, viz., glutamic and isocitric acids, whereas in the light the distribution is as in the original 30-second chromatogram. Here for the first time we have the appearance in our flow sheet of acids with more than 4 carbon atoms, and their presence immediately suggests the operation of a series of reactions which are known as the Krebs cycle. In the next figure the Krebs cycle has been taken directly from Baldwin's book, and I include it because it ties into one scheme of reactions all the metabolites which we have been considering other than those of the glycolytic sequence.



You will note that the initiating reaction in this cycle is the reductive decarboxylation of pyruvic to acetic acid. Calvin postulates that the presence and absence of isocitric acid in darkness and in light respectively is governed by the subsequent metabolism of acetic acid. His hypothesis is that in the light, acetic acid goes off to replenish the stores of vinyl phosphate which are continuously being depleted through carboxylation to phosphoglyceric acid. In darkness, on the other hand, acetic combines with oxalacetic to form aconitic and so on to isocitric and glutamic acids. Or put in another way, this indicates that not only does the light initiate a reduction process, but it also inhibits certain oxidation processes by preventing the freshly-formed photosynthate from entering the Krebs cycle with the formation of isocitric and glutamic acids. You must not think that these acids do not exist as such among the cell constituents. Glutamic certainly is present and isocitric is probably present. The point I wish to stress is that none of the radioactive carbon, assimilated during the period of seconds or at most a few minutes, finds its way into these compounds, unless a period of darkness is imposed on the system.

You may or may not accept Calvin's hypothesis regarding the formation of glutamic and isocitric acids. I introduced the topic because here we have an example of photodynamic action which is certainly not related to the primary photosynthetic process. In darkness certain reactions proceed which do not occur in the light and Calvin makes the tentative suggestion that this phenomenon is related to the complex phenomenon of photoperiodism.

That is the end of my story so far as Calvin and his associates at Berkeley are concerned, and although presidential addresses are not subject to ordeal by question, I think it is only right that I should spend a few minutes in criticism. Reading

back over what I had written, my own reaction was that the story was too simple, too cut and dried. Admittedly, words like "postulate," "hypothesis," "analogy," and "inference" occur almost **ad nauseam**, but the fact remains that they do not occur nearly so frequently as scientific honesty demands. I have demonstrated and discussed a variety of compounds, their degradation, their synthesis, and their interconversions. Calvin's approach is so direct and his findings so impressive that it is fatally easy to fall into the trap of identifying a plausible hypothesis with experimentally verified evidence. I do not think that I am doing him an injustice in saying that he has travelled about one yard along a road that is a mile long.

And in support of this I should like to describe very briefly the work of certain other investigators, in particular that of the group under Gaffron at Chicago. These people have also been working with unicellular algae, and have used techniques and methods which are similar to those of the Berkeley group. Yet their findings are entirely dissimilar, and Gaffron's work is much more closely in line with that of Ruben mentioned very early in the discussion.

When cells of **Scenedesmus** are allowed to photosynthesize in labelled CO_2 for periods of less than 40 seconds, the radioactivity is found in a small water-soluble fraction. The Chicago group believe that this radioactivity is vested in two compounds which can be separated by standard methods. Neither compound is a carbohydrate or phosphate ester of a carbohydrate—a direct contradiction of Calvin's discovery of sucrose, and the phosphates of glucose and fructose. They could find no amino acids, whereas Calvin found alanine, aspartic acid, glycine, serine, and under certain conditions glutamic acid. There were no keto acids, such as pyruvic and oxalacetic, no acids of the Szent-Gyorgi or Krebs cycle, such as malic, fumaric and isocitric, and no aldehydes, ketones, phenols or alcohols. They went to particular trouble to establish the presence of phosphoglyceric acid, and were completely unsuccessful.

In other words, the Chicago group could find not a single one of the compounds on which this whole discussion has been based. These workers have defined their products not by showing what they are, but by showing what they are not.

I suppose that had this address been constructed on strictly logical lines, I would have emphasised these contradictions as I went along. I wanted to show, however inadequately, some of the amazing transformations which all cells are capable of bringing about, and to that end I chose the other

course. Spectacular advances can be expected in this field during the next five years, and the results achieved will be of importance not only in the solution of the problem of photosynthesis but over the whole field of biochemistry as well.

CHEMICAL COMPOSITION OF BUTTERFAT

By R. P. HANSEN and F. B. SHORLAND, Fats Research Laboratory, Department of Scientific and Industrial Research, Wellington.

It has been shown by various investigators that butterfat contains normal straight chain acids both saturated and unsaturated and varying in molecular size from C_4 to C_{26} .

In the course of investigating the efficiency of the separation of the methyl esters of C_{18} acids of butterfat into saturated and unsaturated constituents by crystallization in acetone at $-30^\circ C$, the neutral product of the soluble fraction after oxidation with $KMnO_4$ was found to be a liquid. Similar investigations with pig fat showed that the acetone soluble fraction after such treatment with permanganate was solid and corresponded in properties with methyl stearate.

The presence of liquid methyl esters resistant to oxidation with acetone permanganate suggested that butterfat may contain branched chain acids which have hitherto been found only in tubercle bacilli lipids and in wool grease. The acids recovered from this fraction, representing less than 1% of the total fatty acids, showed the following characteristics:

m.p. $8^\circ C$. Saponification equivalent 287.0; Iodine value 2.4.

Specific rotation: $(d) = +0.9$ (chloroform).

Combustion analysis (Dr. Ma): H 11.56%, C: 74.62%.

Refractive index: $n_D^{17^\circ} = 1.4578$.

The analysis is consistent with the presence of branched chain acids contaminated with some oxygenated acids.

Further quantities of the material are being prepared in order to isolate a pure fraction.

ITEMS FROM THE MINUTES OF THE MEETING OF COUNCIL-IN-PERSON

HELD IN CHRISTCHURCH ON MONDAY, AUGUST 21st, 1950.

CONFERENCE, 1951, is to be held in Hamilton in August.

EXAMINATIONS COMMITTEE.—Syllabus for Microbiology approved, but Committee recommended to reduce the syllabus for Librarianship. Typing to be included as a separate optional subject.

NEW MEMBERS.—Nine new Associates were elected.

RESIGNATIONS of Messrs. I. A. Wilkinson and F. G. B. Brown were accepted.

FOOD PARCELS.—Branches are continuing with the distribution of food parcels to members of the R.I.C. Benevolent Fund during 1951.

NEW BRANCH.—It being reported that the result of the Postal Ballot was 155 in favour out of a total of 156, it was decided to set up a new Branch of the Institute, to be known as the Manawatu Branch, with headquarters at Palmerston North.

COMMITTEE REPORTS were discussed in detail.

During Conference a GENERAL MEETING of members was also held with an attendance of about 80 members. Reports from the various sub-committees of the Institute were received and discussed. There was a spirited discussion on a motion presented by a Wellington committee through Dr. F. B. Shorland, "That the Council approach the Senate of the New Zealand University asking if it would be possible to expand the scope of the Ph.D. degree to cover degrees outside the University." Mr. R. Elder presented the opposing point of view for the Canterbury Branch committee, and was supported by a number of other speakers. The motion was lost. It was reported to the meeting that the Government is to be asked for assistance in paying a subscription to the International Union of Chemistry. Life membership was further discussed, and the scheme of the Chemical Society may be investigated.

A further meeting was held during Conference of a representative committee to discuss any general procedure that might be laid down for the conduct of future conferences, an important item being the form of publication of the summaries of papers. The members of the meeting were asked to discuss the points raised with their respective branch committees, and forward any suggestions to the November Council meeting.

CONFERENCE BOOKLET

Those members unable to attend the Conference in Christchurch may obtain a copy of the Conference Booklet, containing summaries of the 25 papers presented, by writing to the Conference Secretary, P.O. Box 1290, Christchurch, and enclosing 2/6.

BOOK REVIEWS

COAL, COKE AND COAL CHEMICALS. By Philip J. Wilson and Joseph H. Wells. 509 pages. 1950. New York: McGraw-Hill Book Co., 8.00 dollars.

In view of the age and extent of the coal carbonising and by-products industries, the number of books available is, as the author points out, surprisingly small. However, in attempting to bring the subject up to date since the appearance of Lowry's two-volume work in 1945, it appears that the present authors have not been entirely successful. It is the reviewer's opinion that

this industry is long past the stage when it can be covered in a single medium-sized volume. The publication of a series of specialised treatises, written by a panel of experts, was contemplated by the British Institution of Gas Engineers some years ago and is now much overdue.

The specialist reader is disappointed to find the book principally a general survey containing little that is not already available elsewhere. Furthermore, in a work which claims to be up-to-date, some account of recent and important developments can reasonably be expected. The authors have neglected this aspect and make, for example, no mention of the British work on the catalytic removal of organic sulphur from town gas, or of recent British work generally. Also in view of the importance of the Fischer-Tropsch synthetic fuel process it is surprising to find that it is not mentioned, while the Winkler and other variations of the high-pressure steam-oxygen processes which supplied synthetic gas for the German war machine receive but seven lines. These processes are being extensively investigated by the United States Government and some important improvements have already appeared in the literature.

However, as a text-book for junior technicians or as a general survey for chemists in other industries, this book can be recommended. It conveys a good general impression of the subject clearly presented, and the reader can gain a good impression of the appearance of plant from a series of 221 excellent plates and diagrams and of working results and analytical data from 97 tables.—R.S.

The seventh edition of **LANGE'S HANDBOOK OF CHEMISTRY** is now available at \$7.00. A number of corrections have been made, there is an increased table of symbols, and a new table of motion-picture emulsions. On the other hand, the mathematical tables have been drastically reduced by more than 75 per cent., and the paper is not as good, so that the print shows through in places. This is still a useful volume, but not such good value as its predecessor. (See this Journal, Feb., 1949.) It is published by Handbook Publishers, Inc., Sandusky, Ohio.

Prof. Linus Pauling, the eminent chemist of the California Institute of Technology, has written two elementary text-books published by W. H. Freeman & Co. of San Francisco. **GENERAL CHEMISTRY** (618 pages, 1948) is intended for the "freshman" course, while "**COLLEGE CHEMISTRY**" (705 pages, 1950) covers the same ground with more descriptive matter and less mathematics, and is intended for students not advancing in chemistry. Both these volumes have the stamp of authority and are extremely modern in their treatment. The coverage is wide, including all branches of chemistry and biochemistry, without appearing sketchy. Notable features are the clear exposition of the various topics and the excellent illustrations, which are largely freehand drawings by Roger Hayward. The same publishers have issued a "**LABORATORY COURSE FOR PAULING'S GENERAL CHEMISTRY**," by Lloyd E. Malm and Harper W. Frantz.

LES HAUTES TEMPERATURES ET LEURS UTILISATIONS EN CHIMIE. (High temperatures and their use in chemistry.) Published under the direction of P. Lebeau, President of the Committee on High Temperatures of the C.N.R.S. 2 fr.; 1397 pages; 1950, Masson & Cie., Paris; 9000fr. This work consists of a number of essays by French leaders in the fields covered and as such breaks new ground, being the first to collate such information. It discusses exhaustively the various methods of producing high temperatures, and their control, as well as the various refractories both of laboratory and industrial significance and the application of high temperatures to ceramics. It is profusely illustrated with pictures of apparatus, phase diagrams and other graphs.

ITEMS OF INTEREST



We congratulate Mr. H. A. L. Morris of the Dominion Laboratory, Dunedin, on winning the Industrial Chemical Essay Prize for 1950. No award has been made for the previous three years and the value of the prize this year was £25. The entries were of a conspicuously high standard and the examiners had difficulty in deciding the winner. Mr. Morris in his essay explored the possibilities of the manufacture of furfural in New Zealand, a fitting subject for a member of the Otago Branch, as the raw material for this proposed industry is derived from the manufacture of oatmeal.

Mr J. Murray, Associate, has been awarded a National Research Scholarship, and will shortly proceed to Cambridge, England, to study under Professor A. R. Todd. Mr. Murray is a Junior Lecturer at Otago, and was until recently Branch Editor.

At the request of the Otago Branch of the Institute a post-graduate course of lectures in chemistry was given in Dunedin during the winter term. The course was well attended by some 35 members and was opened by Prof. Soper with two lectures on "Entropy and Chemical Change" and two lectures on "The Modern Theory of Electrolytes." Dr. M. Irwin discussed "The New Elements" and Mr. W. S. Fyfe "Crystal Structure and Physical Properties." Other lectures were "Chromatographic Analysis," by Mr. A. D. Campbell; "The Organic Chemistry of Petroleum," by Mr. C. L. Carter; "The Chemistry of Some New Chemo-therapeutic Agents," by Dr. R. E. Corbett, and "Some New Reagents in Organic Chemistry," by Mr. W. G. Edwards. The work of Prof. Soper and his staff in organising the course was much appreciated by the members of the Branch.

In Auckland a special series of three lectures on Thermodynamics was given by Dr. H. Bloom, but unfortunately this was not well attended. A very interesting evening was provided by Mr. R. Hicks, A.R.I.C., recently appointed Chief Chemist to the Auckland Drainage Board. A film of the very extensive sewage plant at Mogden, England, was first shown, and then Mr. Hicks gave a short address, which was followed by questions and a very lively discussion. In a recent newspaper report Mr. Hicks is stated to have commented unfavourably on the fact that there was no chemist on the commission set up to discuss drainage schemes for Auckland city and suburbs. At the July meeting of the Auckland Branch, Mr. G. Dingley spoke on the "Chemical Control of Brewing." Fortunately no samples were available and the meeting was quite orderly.

Dr. H. N. Parton, Associate Professor of Physical Chemistry at Canterbury College, spoke to the Otago Branch in July on "Chemical Education" and discussed various aspects of the teaching of chemistry in British and American

Universities. In the same month there was also a demonstration in the School of Mines by Professor G. J. Williams and his staff. Dr. R. Gardner, Branch Chairman, thanked Dr. Williams for a most interesting evening, and also the staff of the School of Home Science for a delightful supper.

In August a joint meeting of the Otago Branches of the Institute, the Royal Society and the Geographical Society, held a discussion on "Food and People" initiated by Mr. D. Cairns, Secretary of the N.Z. National Commission of UNESCO. The Otago Branch was also fortunate in that Dr. T. W. J. Taylor, Vice-Chancellor of the University of the West Indies, addressed members of the Branch as well as the staff and students of the chemistry department of Otago University, at the invitation of Professor Soper.

Mr. M. J. McDowell, of Dunedin, has been granted a Doctorate at Brown University, Rhode Island, and is now on the staff of the du Pont organisation.

At the July meeting of the Wellington Branch, Mr. T. A. Rafter, who recently returned from a trip to the United Kingdom and the United States, delivered the Mellor Memorial Lecture on "The Use and Problems in the Use of Isotopes."

Dr. A. Goldstern, Associate, has resigned from Brown, Barrett Ltd., Auckland, to undertake an extended trip abroad, including a visit to his home city of Vienna.

Mr. I. S. Hunt, Associate, has joined the staff of Hickson's Timber Impregnation Co., Auckland.

At the August meeting of the Wellington Branch a symposium on "Semi-micro Techniques and Apparatus" led by Prof. Slater provided a most interesting and thought-provoking evening.

Dr. C. J. Wilkins, of Canterbury College, left for England last month on a Nuffield Dominions Travelling Fellowship. He expects to be away for a year.

The promotion of Mr. E. W. Hullett to the position of Director of the Wheat Research Institute, Christchurch, has been confirmed.

Mr. E. F. Scott, Engineer to the Christchurch Drainage Board, addressed the July Meeting of the Canterbury Branch on "Modern Methods of Sewage Disposal," with special emphasis on its conversion to fertiliser. The lecture was illustrated with some interesting colour films.

AMENDMENTS TO LIST OF MEMBERS

Since being published, inaccuracies in the entries for certain members have been notified to the Editorial Committee. Where the committee has been at fault, we offer our apologies.

It is pertinent at this stage to point out that every member of the Institute should notify his branch Secretary or the Registrar of all changes in his qualifications, status, place of employment or home address, so that the Institute records may be kept up to date and early receipt of the Journal assured.

When the publication of the next List of Members is proposed, all members who have corrections to make in their entries in the present list should com-

municate the necessary information directly to the committee handling the project.

1. Cover page and title page:
Delete "of Great Britain and Ireland" after the "Royal Institute of Chemistry."
2. All members at Massey Agricultural College and the Dairy Research Institute are now served by a post office at the College.
Delete "P.O. Box 601 or P.O. Box 602, Palmerston North," where appropriate.
3. Ayling, H. S.: Delete "Mahia Road, Manurewa."
Insert: "105 Hutchinson Avenue, New Lynn."
4. Beath, G. B.: Delete "Bentley"; insert "Bentley."
5. Brown, E. W.: Add "C.O.P. (Bact. and Clin. Path.)."
6. Cawley, R. W.: Delete "Massey Agricultural College," etc.
Insert: "Wheat Research Institute, Christchurch."
7. Chapman, L. P. J.: Delete "24 Phillips Street, Auckland, C.2."
8. Chisman, J. A.: Delete "Morton School, Waikouaiti."
Insert: "Romahapa School."
9. Green, R. A.: Delete "Ronald, A., 1948."
Insert: "Rowland, A., 1938."
10. Holland, H. C.: Delete "Works Manager."
Insert: "General Manager."
11. Hullelt, E. W.: Delete "Chief Chemist."
Insert "Director."
12. Jurd, L.: Delete "Massey Agricultural College," etc.
13. McGillivray, W. A.: Insert "Ph.D."
14. Morgan, F.: Delete "Stevenson and Howell," etc.
Insert "27 Derwent Street, Wellington, S.2."
15. Neubauer, L. C. Delete "Temporarily Overseas."
Insert "Ph.D. (McGill)."
16. Perrin, D. D. Insert "Ph.D. (Lond.)"
17. Perrin, D. R. (Mrs.) Delete "Roberts."
Insert "Roberta."
18. Raeside, J. D. Insert "Temporarily Overseas (Washington)."
19. Ridley, A. A. Delete "Dominion Laboratory," etc.
Insert "32 Waipapa Road, Wellington, E.2."
20. Ross, D. J. Insert "Temporarily Overseas."
21. Siemon, S. R. Delete "262 Grahams Road."
Insert "252 Grahams Road."
22. Stonyer, C. L. Delete "P.O. Box 82."
Insert "P.O. Box 288."
23. Swanberg, E. D. (Miss). Delete "Enid."
Insert "Eda."
24. Swedlund, B. E. Delete "Eskill."
Insert "Eskil."
25. Wright, E. W. Delete "Bowen Street."
Insert "Brown Street."

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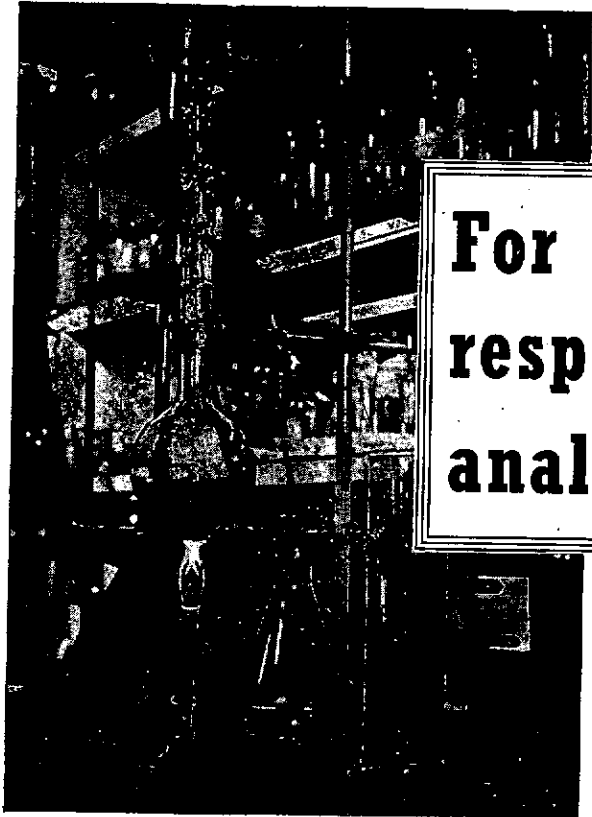
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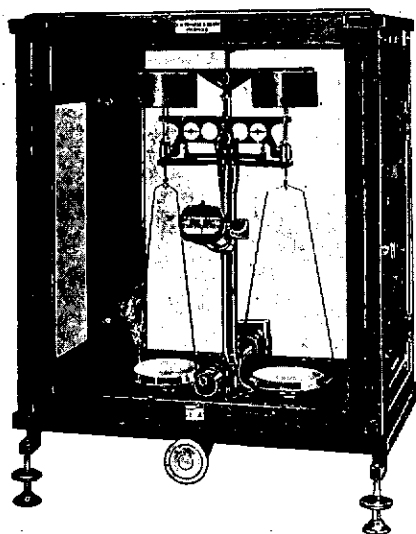
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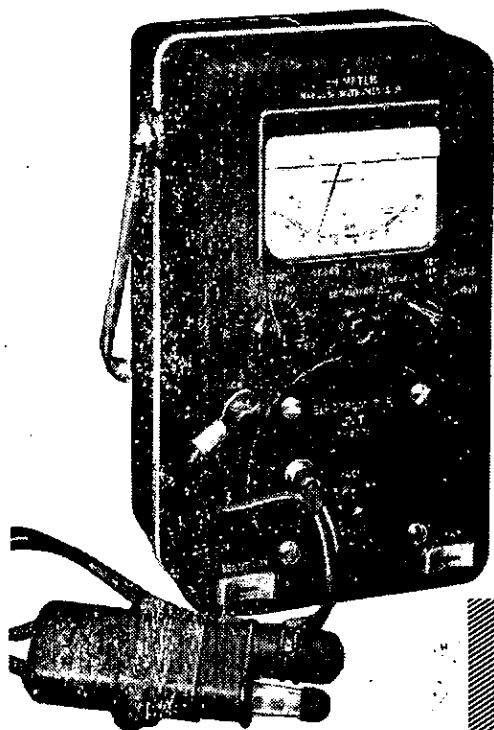
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Early research on penicillin was attended by great difficulties. At first it was only possible to produce minute quantities from the mould (*Penicillium notatum*) and the substance was easily destroyed by heat, acids, enzymes and air-borne bacteria. Imperial Chemical Industries Ltd. was the first industrial concern in Britain to make substantial quantities for chemical and biological investigation. The crude, unstable material then produced has since been superseded by an almost pure substance. Penicillin of I.C.I.'s manufacture is now a white crystalline product of known composition, which retains its activity for three years in all climates.



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