You, Mr. President and Mr. Chairman, have referred in far too flattering terms to the part which I have been privileged to take in this movement. But, after all, the efforts of the individual do not count for much. To change the metaphor, all he can hope to do is to play his part as a unit in the great army he serves, and if he does his best, well, he can do no more.

I trust before the night comes that there may still be a short time left for me in which I may perhaps be able to prove myself a little more worthy of this great honour. The magnificent testimonial from my friends at the Institute which you have been good enough to present to me to-night could not have taken a form more acceptable to myself and to my family. It is the work, as our President has said, of one of the greatest masters of portraiture of this or any other age, and will always remain as one of our most prized possessions. But, best of all, it will always remind us of the debt of gratitude which we owe to my friends for the expression of kindly feeling towards myself which prompted and accompanied the splendid gift.

Gentlemen, I thank you with all my heart.

Reminiscences of Fifty Years' Experience of the Application of Scientific Method to Brewing Practice.

By HORACE T. BROWN, LL.D., F.R.S.

THE origin of this paper may be very briefly stated. It is the result of a retrospect and a process of mental stock-taking, suggested by the fact that I have now been closely connected for half-a-century with one of the most interesting industries in the world. Like most other industries it had its beginnings in the remote past in supplying certain material wants of the people, but it differs from most others insomuch that in its later developments it has suggested problems which in the solving have extended the boundaries of natural knowledge beyond all expectation, and have indirectly conferred incalculable benefits on the human race.

I am fully aware that there is a certain section of the public, ever ready to influence popular prejudice against us, which has no knowledge of these matters and would deny any such claims as these for the brewing industry, but I can confidently assert that the ever-diminishing abuses of our national beverage, on which these well-meaning but mistaken people rely for their arguments, weigh but little in the balance as compared with the life-saving application of knowledge which has flowed solely from a study of beer brewing and the other allied fermentation industries. I shall have occasion to develop this thesis during the course of my remarks; it is the more necessary since the real source of this knowledge is seldom comprehended even by those whose special business it is to make daily use of it in the domains of preventive medicine, modern surgery, or sanitation.

I have been privileged to live through the whole evolutionary period of this great movement, from its inception to its present fruition, which is even yet far from complete, and have followed each step in the development with ever-increasing interest. The source from which all this knowledge sprang is still capable of yielding further supplies to those willing and able to explore and develop it. How often have the endeavours to get a rational and complete explanation of some simple brewing operation resulted in the enrichment of science and the linking-up of some biological or physical problems which at first sight seem to have no connection with the original observation!

Looked at from the point of view of the far-reaching nature of its principles, brewing is an industry which is capable of yielding in kind a very high rate of interest on the scientific knowledge sunk in it.

Whilst dwelling on this aspect of the subject, I hope at the same time to give you my matured opinion on some practical questions which are still more or less *sub judice*, because they depend for their solution on experience and extended observation rather than on the application of the more rigid experimental methods of the laboratory, where conditions can be varied at will and disturbing factors successively eliminated.

My very earliest recollections are closely associated with breweries and brewing, for I was born and received my early education at Burton-on-Trent, a town which then, and even later, occupied a position in the brewing industry quite unchallenged by any other town or locality in the United Kingdom. It was there that the famous Union system had its origin, a system of fermentation particularly well adapted to the higher branches of the export trade of that time. Burton was then a Mecca to which brewers from all parts of the world made their pilgrimages in order to study the latest developments of English brewing.

Influenced strongly by my environment, I made up my mind at a very early age indeed as to what my career should be, and, very fortunately, I had developed strong tastes and inclinations towards natural science, which, fostered and encouraged by a wise father, later on I endeavoured to turn to good purpose.

At the time of which I am speaking, the early sixties of the last century, there was already springing up in the minds of a few of my more enlightened townsmen a feeling, perhaps more or less vague, that the science of chemistry was destined to come to the aid of the brewing art. Amongst these few there was one man, a friend of my father's and a frequent visitor at his house, who had clearer visions of this possibility than the others. This was Dr. Henry Böttinger, a pupil of Liebig's and a friend of most of the well known chemists of the day. Böttinger, who was then Manager of Allsopp's brewery, had in 1860 written an article on brewing for the first edition of Muspratt's *Chemistry as Applied to Arts and Manufactures.* Reference to this article, which is a very interesting one, shows very clearly how small a part science played in our history in those days.

Böttinger, much to my sorrow, left Burton in 1866, the very year I entered on my duties as a practical brewer, but, on looking back, I can very well trace the influence of the wise counsel and advice which he gave me as a lad. He used often to express a firm and certain belief that the principles of brewing, which at that time were based, like those of many of the sister arts, on mere empiricism, would in time to come admit of a rational and scientific explanation, and become more systematised.

Böttinger did not live to see his prophecies fulfilled; had he done so, I think it would have surprised him to see how the greatest advances to which he had looked forward only became possible after the complete overthrow of the preconceived ideas of the exciting causes of fermentation as laid down by his great master, Liebig.

These conversations with Böttinger had the effect of arousing in me a great desire and ambition to help on the good cause, and if I have been able in any way during the last 50 years to assist in this, it is in great measure due to the sowing of good seed by an old and valued friend in ground which I trust has not been altogether stony and unproductive.

In my studies in general chemistry, both prior and subsequent to my student's year of 1865 at the Royal College of Chemistry under Hofmann and Frankland, I also received much help from another friend, the late Peter Griess, who, after acting as Hofmann's private assistant, had been induced by Böttinger to come to Burton, where he became chemist to the firm of S. Allsopp and Sons, a post which he continued to hold up to the time of his death in 1888. Griess was a very eminent organic chemist, and had already made a name for himself by the discovery in 1860 of a new class of bodies, the diazo-compounds, which have played such a great part in the dye industries of the world. Although much interested in the application of science to brewing, which developed so much during the last 15 years of his life, Griess did not take a very active part in contributing to those developments; his heart, in fact, was elsewhere, and he devoted most of the time which could be spared from his routine duties to those organic researches with which his name will always be associated. It is not too much to say that if the services of Griess had been secured by an industry which was languishing in this country for lack of the higher scientific research, the whole history of the manufacture of dyes in England would have run a different course.

When I entered on my duties as junior brewer at Worthington's in January, 1866, there was no laboratory in the brewery. My chief, whom I shall always hold in affectionate remembrance, was one of the old school of practical brewers, and distinctly discouraged any suggestions I threw out as to the desirability of fitting up a small room for the purpose, so for a time I had to be content with the laboratory which my father had provided for me at home, in which I managed to do some fairly good work, but only remotely connected with brewing. I soon found out that the real objection on the part of my chief was due to a fear that the display of any chemical apparatus might suggest to customers who went round the brewery the horrible suspicion that the beer was being "doctored"! This objection was not really so absurd as it may appear to us now, for I distinctly remember on more than one occasion having to defend the existence of laboratories in the Burton breweries against those who failed to see that they could have a legitimate use. However, in my case no objection was raised against the purchase of a balance and the accessories for determining original gravities, and, later on, apparatus far ahead of theory, and has defined the main features of the industries solely by long and painful processes of trial and error. *Empta dolore docet experientia* is indeed true of the past, and to a certain extent must always remain true, but we now possess means, methods and instruments (often alas! too much neglected) which were undreamt of by our forefathers, and which can minimize those painful efforts and the wasteful expenditure of human energy which accumulated experience demands when it is the sole guide to progress. We are no longer exploring without stars, compass, or map to guide us, and are therefore in a better position to appreciate the merit of those unknown geniuses who from time to time struck and followed new paths which led them, they knew not how, a little nearer to the desired goal.

With all our present advantages derived from the application of the methods of science to brewing practice there is still as much, nay, perhaps more scope for the exercise of those qualities which characterised the exclusively practical man in pre-scientific days. For successful brewing there is a certain indefinable "touch" required which can only be attained by daily exercise and contact with practical operations, and which becomes in time part of one's subconscious self. It takes long to acquire it, but when once acquired it is never lost; without it a man can never become really proficient, no matter what his other qualifications may be. Those of you who possess this almost instinctive "touch" will understand perfectly well what I mean. A good example of its application to daily needs is afforded by the manner in which a brewer manages his fermentations. If he has the faculty of which I speak he seems to recognise at once that his yeast is a living thing with a distinct character of its own, and it is not too fanciful to assert that he feels the necessity of getting into sympathy with it, and of controlling its behaviour more by coaxing and gentle treatment than by stronger measures which it is apt to resent. If he has any imagination it will also appeal to his æsthetic sense; perhaps some day a brewer Maeterlinck will arise who will be able to do full justice to the changing beauty of those fleecy flocks and their shepherding.

But this is somewhat of a digression, and I must take you back once more to those days of the middle sixties of the nineteenth century to which I have referred.

Of brewing literature, either on its theoretical or practical side, there was little of any value to one looking out for light and leading at that time. The most popular English work was a *Practical Treatise on Brewing*, by William Black, a new edition of which appeared in 1866. This is a curious work, worthy of a place in a museum of fatuous works on Brewing. According to the author, all the ills to which beer is liable are due to atmospheric electricity and earth currents, and the one essential to success is to insulate the fermentation vessels, and to arrange them and the attemperator pipes in such a manner as to avoid the possibility of electro-chemical action. This appears to have been a mere guess of the author, and not one in any way based on experiment, or even on close observation, but we must remember that the view was put forward at a time when the explanation of any occult phenomena was conveniently found in the mysterious agency of electricity, just as physiological phenomena were explained by recourse to a supposed "vital force."

These statements of Black induced me at a very early period of my career to take up the subject experimentally, for there seemed to me no valid reason why the alleged effects should not be equally well reproducible on a small scale. You will not be surprised to hear that my results were of an entirely negative character, provided I used platinum electrodes and guarded against the possible introduction of any disturbing cause other than that of the electric current itself. Whatever might be the immediate cause of beer going acid it was, at any rate, not ascribable to electric action.

In the course of my later experience of brewers and their ways of looking at things I have occasionally come across traces of a survival of this old belief, and in one respect there is no doubt a grain of truth in the idea, since it is quite possible, especially in these days of metal-lined fermenting vessels, to have an unsuitable arrangement of attemperators and their connections which will give rise to local electro-chemical action, and the formation of traces of soluble copper salts, to which yeast is particularly sensitive.

Further to realise the little help afforded by the current literature of that day one has only to refer to the various articles on Fermentation and allied subjects in Muspratt's *Chemistry as Applied to the Arts and Manufactures*, published in 1860; and to the first edition of Watts's *Dictionary of Chemistry*, published in 1863. I have a very distinct and painful recollection of my endeavours to derive from these supposed for water analysis, but the little office in which these were placed for my use had its windows carefully obscured, so that no one could see what was going on inside! For the development of my complete scheme I had to wait patiently and bide my time, which came in due course.

There was another thing which I discovered very early in my career, and one which required some worldly wisdom and tact to deal with satisfactorily.

Fifty years ago it was a distinct disadvantage from the point of view of advancement to have too enquiring a mind, and one had to be extremely wary in asking too many questions about the reasons for doing this, that and the other in brewing operations. Certain rigid rules had been laid down for the guidance of the brewer and maltster which were assumed to have been so far sanctified by experience and tradition that to throw the least doubt on their general applicability laid one open to the terrible charge of being a "theorist," and, save for the supposed extinction of "frightfulness" at that time, one's fate might have been that of a disbeliever of the Aristotelian philosophy in the Middle Ages. I remember cogitating over this word "theorist" and trying to get some clear idea of what this modern Council of Inquisitors meant by it, and I could only come to the conclusion that it signified a man who wanted to know something of the underlying principles of his practice, and, above all, one who asked inconvenient questions which exposed the ignorance of those to whom they were addressed.

I used to wonder how it was possible to apply an unvarying code of rules to manufacturing processes like malting and brewing, which deal with raw products exhibiting such seasonal differences of quality, and yet expect constant results.

But my belief in the divine origin of some of these empirical guides to perfection received a still more severe shaking in the first year of my experience by a brewing catastrophe of considerable magnitude which somehow or other had eluded all rule-of-thumb experience and set tradition at defiance. It was really due, as I learned long afterwards, to a serious invasion of the *Saccharobacillus Pastorianus*, but in the year 1866 the lactic acid trouble to which this organism gives rise in stock beers, which then represented the greater part of the output of a Burton brewery, was as little capable of explanation as were epidemics of enteric fever, plague, or cholera. How well do

I remember the hopeless flounderings of the practical men in authority in their attempts to explain the breakdown of all their cherished ideas, and how every impossible agency was invoked, including water contamination, and even obscure electrical agency. We are a little more tolerant of "theory" in these days, in the sense of the application of scientific knowledge to the control of practical operations, but the supposed opposition of practice and theory still lingers in the minds of some brewers who have no scientific insight into the meaning of their operations. This is due in the first place to the somewhat extravagant pretensions which are sometimes made by the man versed only in the scientific side of the business, who fails to appreciate the extreme complexity and interdependence of some of the problems; and secondly, to the very defective education of some of our brewers, who have little or no scientific equipment and no appreciation of scientific method.

I am here tempted to quote the words of the President of the British Association in Manchester last year; he says: "The most fatal distinction that can be made is the one which brings men of theory into opposition to men of practice, without regard to the obvious truth that nothing of value is ever done which does not involve both theory and practice; while theory is sometimes overbearing and irritating, there are amongst those who jeer at it, some to whom Disraeli's definition applies: 'The practical man is the man who practises the errors of his forefathers.'"

Well, I am not one of those who jeer at the exclusively practical man, and with good reason. All our older industries have been built up by successive generations of such men, exploring in all possible directions, observing, sifting and selecting such modifications as appear to be distinct improvements, and transmitting them to the next generation to do its part also. It always fills me with wonder to realise how, by the accumulation of small improvements, often going on for ages, industries have been established entailing most elaborate processes, which at times appear to have little or no relation to each other. The brewing industry and all that it involves, from the selection and growing of the grain to the final product, is an excellent example of this, and it is only surpassed by agriculture, inasmuch as in this latter instance the results of experiments have to be waited for so much longer. In all such cases practice has been authoritative sources some clear ideas which could in any way help me in my quest for some rational explanation of ordinary brewing processes.

I must make an exception, however, in favour of one work which I found of real value: G. J. Mulder's treatise on beer. Mulder was Professor of Chemistry at the University of Utrecht. The work is not a practical treatise, but it was I believe the first book to give any scientific account of beer and the brewing processes. My copy is dated 1865 and is a French translation of the third Dutch edition, the first having appeared in 1861.

It contains a great deal of information, well up to the times in which it was written, on the chemical composition of hops, malt, barley, and grain generally, and even at the present day it may be consulted with advantage on some points. That part of the work, however, which is devoted to the alcoholic fermentation and the acetic and lactic changes in beer is dominated by the *a priori* ideas of Liebig, which up to that time, owing to the genius and commanding authority of their great apostle, had acted as a bar to progress and prevented a dispassionate consideration of the new vitalistic theories of fermentation which were destined to revolutionise all our conceptions of such phenomena. The author refers indeed to the researches of Pasteur of 1857 and 1859 on these subjects, but he attacks most mercilessly the fundamental idea of the French chemist that the vegetal growth and functions of yeast and of the lactic organisms are correlated with the decomposition of the sugar in special directions.

It was in the year 1869 or 1870 that I first began to turn my attention to the systematic use of the microscope in my brewing work, an instrument with which I had already made myself familiar several years before in certain youthful investigations of the pond life of my neighbourhood. The immediate determining factor in this case was a desire to discover the reason, and if possible the remedy, for that abominable flavour and odour which certain casks contribute to the beer, and which is known as "caskiness" or "fustiness."

All that was known at that time about the origin of this flavour, which resembles so closely that of a "corked" wine, was that casks were wont to acquire it through ill-treatment, and especially by not being properly closed after the contained beer had been consumed. Owing to certain special conditions of the Burton trade the annual loss in beer and casks from this cause was very considerable.

In the "casky" beer itself I could not detect by means of the microscope any organisms which were not also present in samples of the same brewing free from this flavour. The case was different, however, when thin shavings of the wood from normal and infected casks were compared. The latter always showed evidence of a mycelial growth penetrating the wood to a depth which varied from a millimetre or two to anything up to a centimetre, and the extent to which this growth extended corresponded pretty well with the intensity of the "casky" flavour which the wood gave when immersed in beer. The whole cause of trouble was ultimately traced to the growth of the well-known *Penicillium glaucum* or blue-green mould, whose hyphæ penetrated the wood to a certain extent.

It was during this work, which I carried to a successful issue, that I was led to examine more fully than I had ever done before the appearance and nature of the organisms adhering to the inner surfaces of casks, and of those occurring in beer sediments of different ages.

It led me also to look into the very scanty literature, and the exasperatingly confused accounts of ferment organisms which then passed as current knowledge, and, what was still more important, it induced me to study the earlier publications of Pasteur, including his memoirs of 1857 and 1858 on the alcoholic and lactic fermentations, and his more recent contributions on similar subjects communicated to the French Académie des Sciences.

It is well known that it was during his researches on the connection between the crystallographic form and the optical activity of the tartaric acids that Pasteur's attention was first drawn to biological questions, owing to a chance observation that the blue-green mould, *Penicillium glaucum*, exercises during its growth a selective preference for the optically right-handed variety of the acid. It is, however, doubtful whether this observation would in itself have directed his footsteps along the path which ultimately led him to those far-reaching conclusions with which his name will be ever associated. It was his appointment in 1854 as Dean to the newly established Faculté des Sciences at Lille which gave the real directive impulse towards the great work of his life.

Lille was then, as now (or rather, I should say, as it will be once more after the war), a great centre of industrial activity, and one of the objects of the Faculté des Sciences was to train the sons of manufacturers, and to fit them for their industrial work, by giving them a thorough grounding in the scientific principles underlying their practice. A very important industry in the town and neighbourhood was distillation, and it was but natural that Pasteur should direct his attention to the various processes involved in the conversion of starch or sugar into alcohol. We are told by his son-in-law and biographer that the stimulus towards the study of fermentation was given in the first place in 1856 by a Lille distiller named Bigo, who was most fortunately led to consult the young Dean on certain disappointments and difficulties which he had experienced in the manufacture of alcohol from beetroot. At that time, the views of Berzelius and Liebig as to the cause of fermentation were all powerful. According to Berzelius, fermentation was due to an obscure catalytic force, and the cellular nature of yeast, which had been observed by Cagniard-Latour and Schwann, was a mere precipitation of a vegetable principle during fermentation. Liebig regarded the ferment as acting by virtue of its being an extremely alterable organic substance, which under the influence of oxygenation was able to communicate molecular motion to the sugar and thus induce its decomposition.

Pasteur in his investigations in M. Bigo's distillery very soon noted that the appearance under the microscope of the living cells in the "wash" differed very much from time to time, and these differences corresponded uniformly with the successes or failures as regards the yield of alcohol in the distillery. These observations were soon linked up with those made by Pasteur on the souring of milk and the lactic fermentation of sugar. He recognised that the gray deposit from such fermentations was made up of organisms much smaller than the cells of ordinary yeast and of a totally different shape; moreover that a trace of these organisms introduced into a solution of sugar to which had been added a decoction of yeast and a little chalk, readily multiplied and induced the lactic fermentation in the sugar, and further that these same effects could always be produced by further inoculation of a similar solution with a drop of the fermenting liquid. In order to meet any possible objections that albuminoids or any organic nitrogenous bodies played a part in the fermentation, nutrient liquids were employed both for the alcoholic and lactic yeasts which, besides the necessary mineral salts, contained their nitrogen only in the form of ammonium salts.

After long and laborious work on these lines Pasteur was able in 1860 to state his conclusion that the alcoholic fermentation is an act correlative with the life and organisation of the yeast cell, and is not a consequence of the death and putrefaction of some of the cells. Nor is it a mere contact phenomenon, whereby the ferment transforms the sugar by its mere presence without adding or subtracting from the substrate.

The idea was already in Pasteur's mind that this principle must be of general application, and that every fermentation has its specific organism by which it can be recognised; but he was always extremely cautious in drawing general conclusions from a limited series of observations, and it still required years of patient work to establish this thesis beyond all reasonable doubt.

Just about the time when Pasteur had concluded his earlier researches on the alcoholic and lactic fermentations, the great controversy on spontaneous generation, after being in abeyance for many years, was revived by Pouchet in a paper read before the Academy at the close of 1858, in which it was maintained that micro-organisms could arise *de novo* in certain organic solutions which were initially free from pre-existent germs, and had been under conditions which negatived the idea that any germs could have gained access from the air. Two years afterwards, in 1860, we find Pasteur taking up the gauntlet thus thrown down by Pouchet, and bringing forward experimental proof of the inaccuracy of his observations and conclusions. Pouchet was supported in his views by Joly and Musset, and more than 15 years afterwards the question was once more revived in a somewhat different form by Dr. H. C. Bastian, who consistently supported the idea of abiogenesis up to the time of his death only a few months ago.

It is not unprofitable to consider for a moment why Pasteur apparently went out of his way to investigate and attack, with all the force of which he was capable, the views of Pouchet and his school, much against the advice of his friends Biot and J. B. Dumas, who considered that in doing so he was forsaking his legitimate work and risking disappointment in a fruitless struggle. Even so late as 1898 we find one of his biographers "disposed to regret that his great powers should have been so long absorbed in this work of exterminating a mere superstition."

Fortunately Pasteur saw that the future application of his new ideas to the etiology of infectious and contagious diseases, which even at this early period he regarded as within the bounds of possibility, would be greatly limited if spontaneous generation were an established fact. He had shown there is a constant nexus between the growth and reproduction of distinctly recognisable organisms and the particular kind of fermentation which they induce, but if it could be shown that these organisms can originate *de novo* and independently of any seeding or infection from the outside, then the whole outlook of the vitalistic theory of fermentation and its further extension to disease would be completely altered. We find him stating in his studies on the silk-worm disease: "Man has it in his power to cause parasitic diseases to disappear off the face of the globe if, as is my conviction, the doctrine of spontaneous generation is a chimera." We may even go further than this, and say that the whole vast superstructure of modern bacteriology which has been erected since Pasteur wrote these words, has been founded on shifting sand and quaking morass if spontaneous generation, in the sense given to it by Pouchet, could be experimentally demonstrated. It was considerations of this kind which led Pasteur to throw himself heart and soul into the fight in 1860 and later, for he, and he alone, saw the great issues at stake, and both then and thereafter he used his "vorpal blade" with good effect whenever the heterogenists ventured to come out into the open.

Pasteur, as we have already stated, owed much to the fermentation industries for the suggestion of many of his greatest problems, and the first twenty years of his active scientific life were occupied in their investigation. He recognised this indebtedness to the manufacturing industries and was ever desirous of liquidating that debt by applying the extended knowledge gained in the laboratory to the improvement of processes carried out on the large scale. For him there was no difference between pure and applied science, and he did not think it beneath his dignity to make suggestions to the practical man as to the conduct and control of his manufacturing operations, or even to take part in the work himself when occasion offered.

In 1863, mainly, he tells us, owing to the encouragement he received from Napoleon III, he began to turn his attention to the great wine industry of France. Since these investigations occupy a very important place in the continuous evolution of Pasteur's ideas, I propose to deal with them somewhat in detail, more especially as they had a great influence on the course of my own work from about 1869 onwards.

He approached them fully convinced by his previous studies that he would be able to throw some new light on the obscure maladies to which wine is liable, maladies which up to that time had never received any rational explanation. This important research occupied about three years, during which time he brought two papers on the subject before the Académie des Sciences, in 1863 and 1864, and in 1866 he incorporated his results in a work entitled *Études sur le Vin*.

Up to this time the general opinion, based on the statements of Colin, Liebig, Frémy, Berthelot, and others, was that wine is a liquid the constituents of which are ceaselessly reacting on each other in virtue of their molecular activity, and that when the wine contains nitrogenous substances of an albuminoid nature these undergo modification or alteration from some unknown causes, and *ipso facto* excite different kinds of disease in the wine.*

Pasteur, on the other hand, showed by his observations and experiments that wine does not undergo any spontaneous changes other than those due to the ordinarily well-known chemical reactions of its contained acids and alcohols, which result in the course of time in the production of esters, but he draws a marked distinction between these purely chemical changes in a maturing wine and those which are characteristic of disease. Whilst admitting that the ordinary ageing of a wine is essentially due to oxidation phenomena, induced by access of atmospheric oxygen, he maintains for the first time that the maladies of various kinds are not due to albuminous substances undergoing certain obscure and unknown changes, but that they are always brought about by the agency of visible but microscopical organisms having an extraneous origin, which multiply when the conditions of growth are favourable. These organisms alter the flavour, odour and the appearance of the wine, either by taking something from it, or, as is generally the case, by the formation of new products which are the direct effect of the multiplication of these parasitic organisms in the bulk of the wine.

Direct and convincing proof of these statements is given in his famous and epoch-making book, and this is accompanied by a series of

* Doubtless exactly the same reason would have been given for the changes going on in beer.

beautifully executed illustrations of the various micro-organisms responsible for the particular maladies of wine, whose symptoms are minutely described.

In the first place Pasteur describes the acetic change in wine, which he has also dealt with in a subordinate work, his *Études sur le Vinaigre*, and which he connects with an organism of surface growth, the *Mycoderma aceti*, the active agent in transferring atmospheric oxygen to the alcohol of the underlying liquid, with the production of acetic acid. His studies then extended to another film-growing organism, the *Mycoderma vini*, which unlike the *Mycoderma aceti* has the power of oxidising alcohol completely to water and carbon dioxide.

One of the most interesting sections in the book is that devoted to "Maladie des Vins tournés, montés, qui ont la Pousse." This deals with an organism which is, I believe, identical with one of the varieties of *Saccharobacillus Pastorianus*, well known to brewers. It produces a spring "fret" in wine affected by it, and an unpleasant flavour, accompanied by a curious "silky" appearance when seen in a strong transmitted light. This appearance is generally the first indication of there being anything wrong with the wine, and is due to the filaments of the bacillus setting themselves parallel to the convection streams induced by shaking the liquid. Pasteur refers to this malady as being of very frequent occurrence at the time he wrote, and as being responsible for considerable losses to the vigneron and merchant.

The proof adduced of the causal relation between this organism and the malady of *vin tourné* was not so satisfactory as it was in the case of *Mycoderma vini* and the acetic fermentation, where a trace of a pure culture of the organism re-introduced into a suitable sterile solution reproduced acetification at will. In the former case, Pasteur relied rather on "experiential" methods for his proof of the connection between organism and malady. Such methods, it is true, can establish a high degree of probability of a causal connection between the two sets of phenomena, but modern bacteriology demands more than this. At the present time, before it can be considered *proved* that any particular disease in animals or plants, or any particular chemical change observed in a fermentable or putrescible liquid, is brought about by the agency of a specific organism, the following conditions must be satisfied:

1. The particular disease or change must be shown to be *invariably* accompanied by the presence of the specific organism in a living state.

2. The organism must be isolated in pure culture.

3. The re-introduction of a little of the pure culture into a healthy animal, or plant, or sterilised nutrient, must be shown to reproduce the particular malady, or the specific chemical change in the nutrient, as the case may be.

Another disease of wine which was investigated by Pasteur was one which is familiar to us as "ropiness," a peculiar viscous change which causes the wine to pour out like oil. The organism which he associated with this disease in the case of the red wine of the Loire district is apparently one of the viscous ferments which is not very common in ropy beers, but one which I have occasionally noticed.

Still another disease which is described is the *Maladie de l'Amertume*, but Pasteur was in this case not so successful in proving its connection with an organism which he figures somewhat tentatively as the immediate exciting cause of the trouble.

In the second part of the *Études sur le Vin* the author discusses the influence of atmospheric oxygen on vinification, and concludes that whilst the action of parasitic organisms always results in deterioration of the wine, the influence of oxygen, apart from the *rôle* which it plays in acetification, is beneficial provided the access of oxygen is regulated: in fact he goes so far as to state that it is oxygen which "makes" the wine, and produces by slow processes of oxidation those qualities which we associate with a well-matured product.

On this question, which is one of extreme difficulty and requiring far more investigation than it has yet received, I do not propose to dwell. I have of late had occasion as a practical vigneron to study some of these problems in connection with the white wines of South Africa, and have recently given this Institute the results of these investigations (this Journal, 1914, **20**, 345). When Pasteur wrote his work on wine, and even long afterwards, there was nothing known about the *oxidases* which are present in some wines and act as "oxygen carriers," or of the *rôle* which iron salts can play in conjunction with certain *tannoids* in producing unpleasant changes in a wine. There are undoubtedly some diseases of wine such as the *casse ferrique*, manifested by a darkening of colour, a distinct precipitation, and the development of a bitter flavour and an earthy odour or "terroir," which are not in any way ascribable to the growth of micro-organisms. They are changes of a purely chemical nature induced by the indirect oxidation of certain tannoid constituents of the wine, under the catalytic influence of traces of iron salts.

The consistent association which Pasteur had observed between a definite organism and a definite malady of the wine opened up a completely new vista as regards the early diagnosis of the malady, and its mode of treatment.

There could certainly be no hope of predicting the future history of a wine as long as the maladies were regarded as being caused by the presence of minute quantities of albuminous matters undergoing an occult and mysterious kind of decomposition which, according to the particular phase of molecular vibration, could produce this, that, or the other form of disease. But the outlook became entirely changed when it was once established that the chemical changes of the substrate were invariably correlated with the appearance of definite organisms which could be seen, isolated, cultivated, and experimented with at will. The exact manner in which the organism functioned in bringing about the special change might still remain an open question, but it was one which might be left to future research, meanwhile for all practical purposes it was sufficient to know that the organism and the particular phase of fermentation stood in the relation of cause and effect. Pasteur never attempted to indicate the particular mechanism by which this was brought about, and was content to include it in the general life functions of the organism. His arguments and conclusions are not in the slightest degree weakened, as some people have imagined, by the more recent discoveries of Buchner and others that, in the case of the alcoholic fermentation at any rate, the actual transforming agent is of an enzymic nature, which, after separation from the living cell, can split up sugar into alcohol and carbon dioxide.

Pasteur, with his practical way of looking at things, soon applied this new found knowledge to the wine industry by using the microscope to differentiate those wines which would be likely to change in time, and also by devising a process which would prevent that change from taking place even when the seeds of the malady were present. His first attempts in the latter case were in the direction of adding to the wine harmless antiseptics having de-oxidising properties, such as the sulphites, but he soon abandoned these in favour of the application of *heat.* He found that by raising the temperature of the wine for a short time to from 50° to 60° C. the contained organisms were destroyed and the wine became practically inalterable. This is the well known "Pasteurising" process which is now so largely employed in various ways in the fermentation industries.

As I have already stated, the first edition of Pasteur's *Études sur le Vin* was published in 1866, but so little attention did it receive in this country that it only came under my notice in the latter part of 1870 through a study of the contemporary communications of its author to the Académie des Sciences. That a treatise of this character and with such great potentialities should have received practically no notice here when first it appeared is somewhat surprising, but I imagine it must have been regarded as a technical work dealing with a subject having no direct connection with British industries.

In one way I think it was an advantage that I did not come across the *Études sur le Vin* until I had had from three to four years' experience in practical brewing, for during that time I had learned much about the direction in which scientific control was most needed in practice.

In those days the Burton breweries were almost completely shut down during the summer, the main brewing operations being carried on between the months of October and May. This practice of course entailed the carrying of very large stocks of season-brewed beer for use in the summer. The export ales, which at that time formed by no means an inconsiderable proportion of the total output, demanded still greater keeping properties than those for home consumption. The risks attending this class of trade were naturally considerable, and the system was only commercially possible with high gross profits to carry such risks and all the other expenses incidental to it.

It goes without saying that under such conditions great financial losses occurred at times through spoiled beer, and, as I have already said, any explanation which was forthcoming was always more or less vague, founded on guesswork, and of no real value as a guide for the future.

At a very early period it seemed to me that if one could obtain some early warning of the trouble it would at any rate be of great service, since a process of selection of the various brewings would have enabled us to get the doubtful beers consumed before the malady had advanced sufficiently far to be apparent.

I tried many plans in endeavouring to discover some practical means of early diagnosis, including comparative determinations of the nitrogenous bodies left in the beers, for according to then current ideas the disturbing factors must reside in the residual albuminoids. These nitrogen determinations were all made by the somewhat laborious process of combustion *in vacuo* in the Frankland–Armstrong apparatus, as it was long before the introduction of the Kjeldahl process. As might be expected they led to nothing, and I had just turned to microscopic methods when Pasteur's *Studies on Wine* came into my hands in 1870. The immediate effect was that of a ray of light piercing the darkness and illuminating a new path into the unknown. It is true that the work dealt only with wine, but it was at once evident that the new principles must be equally applicable to beer brewing, and from that moment I turned my attention with renewed energy to my microscopical work, fully confident that I should thereby obtain an answer to the many questions I had been asking myself for the past three or four years.

In a short time, by a comparative study of the sediments of normal and faulty beers of various ages I had worked out, and could recognise, the particular organisms which produce most of the irregularities of bacterial origin. On looking back at my notes and microscopical drawings made in 1871 they show that by that time I had obtained a complete knowledge of the life-history and after effects of a common filamentous organism, now known as the *Saccharobacillus Pastorianus*, which in its early stage of development gives rise to the "silky" turbidity of beer, and in its later and segmenting stages to a slow acidification of the beer from the production of lactic acid. This was the organism which gave rise to nine-tenths of the trouble experienced with the Burton stock beers of that time, and I well remember the immense pleasure which this discovery gave me, for I saw that it would have far-reaching consequences. The years 1872 and 1873 found me busily occupied in extending these researches to beers obtained from all possible sources. Where a definite bacterial organism could be detected an endeavour was always made to isolate it, to cultivate it in a suitable medium, and as far as possible to determine the chemical nature of its fermentation products, and the general character of the changes in appearance and flavour which it induced in beers infected with it.

The experimental difficulties which I had to contend with were great, for it must be remembered that I am speaking of a time prior to the appearance of Pasteur's work on *Beer*, which was published in 1876, and long before the modern technique of present-day bacteriology had been developed as regards the isolation of organisms on solid culture media, and the applications of staining methods for their differentiation. I had to work out these problems for myself, guided only by the general principles which had been laid down by Pasteur in his various researches on the alcoholic and lactic fermentations, and in their application to wine and vinegar. But the result was well worth all the labour expended on it, for I was soon in a position not only to recognise the particular micro-organism associated with a given malady, but also to exercise a certain amount of prediction as to the ultimate history of a beer, since the first appearance of the determining organism could by suitable means be detected long before any of the ordinary symptoms of the disease had made any perceptible progress. The means adopted for this method of prediction ultimately took the form of the "forcing" system which is now in such common use in breweries exactly in the form in which I originally devised it.

Since there seems to be some misunderstanding as to the scope and object of this process, it is perhaps desirable that I should state briefly what it was intended to do, and what its limitations are as an aid to modern brewing practice.

The immediate object was to put representative samples of beer under such conditions of temperature as would hasten the development of any of the adverse bacterial changes to which the beer was liable when stored under the ordinary conditions which rule in practice. By a long series of trials, including comparisons of "forced" samples with beers stored and matured in cask in the ordinary way, it was found that all the information required at that time could be obtained from small samples of 100 c.c. or so of the newly racked beer, taken with proper precautions and with the normal sediment in suspension, if these were submitted to a temperature of 80—85° F. for a period of about three weeks. Microscopical examinations at the end of this time gave information as to whether there had been any infection of the beer with lactic organisms sufficient to overcome the resistance to their growth and reproduction which all beers possess in a greater or less degree. In this way valuable information was gained as to the future history of a beer in certain directions, and generally in sufficient time to be of service in determining its disposal. The indications of the "forcings," coupled with microscopical control of the fermentation processes at all other periods, also gave a good idea at any time of the general state of affairs in the brewery, and constituted a check on the plant and processes which I soon found to be invaluable. As my experience of the forcing system increased I found it applicable to bacterial changes other than those induced by the *Saccharobacillus*, for which it had been specially devised, and in these directions there can be no doubt that the indications afforded by the "forcing tray" are in skilful hands as valuable to-day as they were 40 years ago.

But times have changed, and with them fashions and the public taste, in beer as in many other things, so that we now experience difficulties of a different order from those which had to be met in my early days.

It was not until long after the time of which I am speaking that brewers began to recognise that their hidden foes were not confined to organisms of a bacterial nature; in fact there is no evidence that Pasteur himself ever realised this. Some few of us at Burton had observed that the finer qualities of a fully matured beer depended to a large extent on the nature of the secondary yeasts which appear in cask or bottle, and as early as 1877 or thereabouts I had proved by continuous observations on single cells of these secondary yeasts, using the Dallinger and Drysdale moist cell as an adjunct to the microscope, that they were not genetically connected with the primary pitching yeast, but must have had an extraneous origin. It was further observed that during the early stages of the development of these secondary yeasts the beer was invariably turbid, and was not amenable to the action of the usual fining agents.

These intermediate stages in the maturation of a beer were in the old days regarded as necessary and normal episodes in its history, and a beer was expected to pass through such a period of "sickness" before it could acquire the highly matured flavour which was at that time demanded.

When, however, the public taste gradually altered in the direction of newer beers devoid of the "old" flavour, and of unimpeachable brilliancy, it became a matter of great importance to postpone the incidence of these secondary changes as long as possible, for whilst they are in progress the beer is unsaleable, and if they are allowed to run their course it acquires certain properties which are no longer in demand.

It is, perhaps, not too much to say that secondary "frets" of this nature are to-day responsible for more brewing troubles than those due to bacterial infection. I shall have occasion to dwell upon their origin later on. At this point I am only concerned in considering how far the ordinary "forcing" system is capable of determining the liability of a beer to changes of this kind, and of speeding up these changes sufficiently to be of any diagnostic value.

The question could be answered satisfactorily as it had been with regard to bacterial infection only by comparing the indications of the forcing flask with the actual behaviour of the beer in cask. Using this criterion, I came to the conclusion many years ago that the forcing tray method is not applicable in this case. Whilst, at a given temperature, the parallelism between forcing flask and cask is very complete as regards bacterial growth, there is no certain correspondence of the indications as regards the multiplication of the secondary yeasts which cause the "frets." Although the true explanation of this was suspected long ago, it is only comparatively lately that I have been able to make out the whole story as a result of some recent work on the reproductive power of yeasts, which, within certain limits, is directly proportional to the free oxygen which the cells have initially absorbed, or which has been subsequently available.

Let us consider for a moment the essential differences of the conditions which exist in a newly racked beer containing the usual small amount of yeast in suspension when a sample is, on the one hand, placed in a "forcing flask," and, on the other, remains in cask. I will assume that the cask has been rendered as perfectly sterile as the flask, and that the problem is not further complicated by the addition of dry hops in either case.

In both cases, the yeast cells present will very rapidly absorb all the free oxygen which has been introduced into the beer during its transference from one vessel to another. This oxygen will be shared by the predominant primary yeast, and by any traces of secondary yeast cells which may be present. As my recent experiments show, the future power of reproduction of each one of these two classes of yeast cells will be strictly limited by the actual amount of oxygen absorbed, provided there is no further access of air.

It is evident that in the case of the glass forcing flask further access of oxygen is precluded, but the same does not hold good with regard to the beer in the cask, since an ordinary cask affords far more facilities for the diffusion of air through the wood than is commonly believed. The conditions for the continued reproduction of the yeasts in cask are therefore much more favourable than they are in the forcing flask at equal temperatures, and the two sets of phenomena are consequently never found to correspond with any exactness. With bacterial organisms capable of anaërobic reproduction, the case is entirely different. The yeast cells are so greedy of oxygen, and absorb it with such avidity, that the bacteria are always practically under anaërobic conditions in the beer, no matter whether this is in cask or forcing flask, a fact which explains why there is always a good correspondence between the bacterial indications.

I have made many attempts to modify the forcing-tray process to render it applicable to the early diagnosis of wild-yeast troubles, but although a certain measure of success has attended these efforts, the process becomes too complicated for everyday use, and I have fallen back on another method which gives all the necessary information in a very practical way. This consists in forcing samples of each brewing in small casks in a room maintained at a temperature of 75—80° F. These are kept under daily observation for from 10 to 14 days, a daily record being kept of any visible alterations as regards brilliancy, etc. At the end of the prescribed time the casks are rolled over, and a sample is centrifuged for microscopic examination, a note being made of the appearance of the sediment, especially as regards the relative proportion of primary and secondary yeasts of various types. If in the intermediate daily examinations the beer has shown any turbidity after initial fining, a centrifuged sample will always show whether this is due to actively budding and resuscitated primary yeast, in which case spontaneous clarification will almost certainly take place again in a short time; or whether the turbidity is due to secondary yeasts, which are the invariable harbingers of wild-yeast frets. Such a method of forcing in cask is an invaluable guide if it is carried out systematically under fixed standard conditions for a period which varies somewhat according to the special requirements of the individual brewery. It is a very necessary adjunct to the forcing tray, and the two methods combined are capable in the hands of an intelligent brewer of giving timely information of a positive kind which will enable him to forestall some of his difficulties. He will, for instance, be in a position to ascertain whether such irregularities in the after-fermentation of the finished beer are in any way associated with certain materials or methods of working which may have encouraged the reproduction of certain undesirable ferments, or whether their incidence is sporadic, independent of variations of materials, and conditioned only by the intensity of the original infection.

But this by no means exhausts the possibilities of such methods, since they can often be used to locate more or less exactly the particular infective centres in a brewery, especially if they are supplemented with some of the ordinary culture tests which modern bacteriology has put into our hands. Moreover, since the comparisons of the samples are all made under conditions of a high standard temperature, defects may often be discovered at such times of year when they are perhaps giving rise to no outside trouble merely because the mean temperature is low.

These are facts and considerations which cannot be ignored by the practical man, and a day must come, one I shall not see myself, but which I hope will arrive within the lifetime of many here, when every operative brewer will regard his microscope not merely as a pretty ornament to be put under a glass case in the brewing room and to be used occasionally, but as an instrument capable of looking into the future, and as essential to his daily needs as the thermometer and saccharometer.

A year after I commenced my practical training as a brewer at Worthington's an addition was made to the staff of one of the Burton breweries which was destined to have a very great influence on the development of the scientific side of brewing.

Cornelius O'Sullivan, whose acquaintance I had made in 1865, when he was demonstrator at the Royal College of Chemistry, had followed A. W. Hofmann to Berlin in that year and came to Burton in 1867 on his appointment to the brewery of Messrs. Bass & Co., where he ultimately became the head of the brewing and scientific departments, a post which he occupied up to the time of his death in 1907. This was a very fortunate appointment, since O'Sullivan was a very capable and enthusiastic chemist, and he very soon discovered that there was plenty of scope for his talents in the new career on which he had entered.

Whilst fully appreciating the biological side of the industry which I was then cultivating, he elected to devote himself to the chemical problems connected more especially with the mashing process, which were then in dire confusion. It is to O'Sullivan that we owe all the pioneering work into the true nature of the transformation of starch under the action of diastase, the influence of temperature in modifying the reaction, and a right understanding of the chemistry of the mash-tun. For the purpose of these investigations he had to devise new and original methods of attack, which have been employed by all subsequent workers in this direction; he opened up in fact a perfectly new field of research, which has revolutionised many of our ideas about the mode of action of enzymes and has had a far reaching effect on carbohydrate chemistry generally.

At the time of which I am speaking the polariscope was a comparatively unknown instrument in this country, and was rarely used, except perhaps for occasional estimations of cane-sugar. It so happened that in the laboratory of Messrs. Allsopp & Sons at Burton-on-Trent, there was an old Soleil polariscope of an antiquated pattern, which I recollect having been shown to me as far back as 1863 by Dr. Böttinger, who explained to me as a boy the general principles of the angular rotation of a beam of plane-polarised light when passing through a solution of sugar placed in the instrument. I little imagined at the time that this identical instrument was destined to be in after years of much historical importance. By a fortunate chance this polariscope, which had never been put to any practical use by its owners, was lent in or about the year 1870 to O'Sullivan, who at once saw its possibilities with regard to the special line of work which he had mapped out. Hitherto the sugars, apart from their crystalline habit and empirical composition as determined by their combustion, had been mainly differentiated by their power of reducing Fehling's solution, and this as we shall presently see had sometimes led to actual error. Here, however, in optical activity was another and quite independent characteristic, which could be used in conjunction with the chemical properties, thereby giving a precision in the differentiation of the sugars otherwise unattainable. This may be compared with the location of a point on a curve, to establish which requires a knowledge of at least two co-ordinates. Such I imagine must have been the idea in O'Sullivan's mind when he threw himself heart and soul into this polarimetric investigation.

I have often heard him describe the difficulties he experienced at the outset in trying to understand the construction of this ancient instrument—difficulties which he only overcame by taking every part of it to pieces and re-constructing it over and over again until he had obtained complete mastery of its principles and capabilities.

With the excellent manuals and monographs on the polarimeter now in existence, it is impossible for the student of to-day to realise how truly formidable were the difficulties which my old friend had to surmount nearly fifty years ago before he could fairly start on the applications of the polarimeter to his special line of work. But we were young in those days and both of us would have agreed with Schopenhauer that "to overcome difficulties is to experience the full delight of existence."

There are no indications of these early struggles in O'Sullivan's first paper on the Transformation Products of Starch, contributed to the Chemical Society in 1872. It was in that paper he established the all-important fact that the crystallisable sugar which is one of the transformation products of starch under the action of malt-diastase is not *dextrose*, as had been almost universally assumed, but a sugar of the $C_{12}H_{22}O_{11}$ or saccharose type, having a higher optical activity than dextrose, and a less power of reducing Fehling's solution. He isolated this sugar and showed that it is identical with that prepared from starch by Dubrunfaut as far back as 1847, and described under the name of *maltose*. These observations of Dubrunfaut had been entirely overlooked by chemists, who still went on regarding the starch product as dextrose until this rediscovery of maltose in 1872.

It was the especial merit of O'Sullivan that he saw at once the full bearing and high importance of this discovery, and that, having once determined the specific rotation and cupric-reducing power of maltose, he, for the first time, held the key to a complete investigation of the non-crystallisable or dextrinous portion of the products of starch transformation and the quantitative relation of the maltose and dextrin under varying conditions of temperature.

The results of these researches were given in a series of papers which certainly laid the foundation of all our knowledge on the transformation of starch by diastase, although opinions still differ as to the mechanism of the reaction and the molecular complexity and constitution of the dextrins and malto-dextrins which appear simultaneously with the fermentable and crystallisable maltose.

The commencement of my own work on the chemistry of starch dates from the year 1877, about three years after I had succeeded to the outside management of the brewery with which I was connected. With the increasing responsibilities of a large and growing business my opportunities for continuing my work on the scientific side unaided naturally became less, and I had to find someone to assist me in the laboratory.

In 1877 the late John Heron was appointed to the post, and remained with me up to 1883. Besides continuing and further developing the bacteriological methods of control as applied to the brewing processes, we were naturally led to examine for ourselves the starch problem which had been so brilliantly reopened by O'Sullivan, but at first only with the idea of becoming thoroughly acquainted with the details of the work and its possible applications.

It was not long, however, before we found ourselves somewhat dissatisfied with some of the views expressed by O'Sullivan, another example of the disturbing influence which this puzzling colloid starch always exercises on those who are drawn into its viscid toils. The results of this earlier work were published in 1879, and my further investigations on the same subject were continued up to 1899 with the successive co-operation of G. H. Morris, Spencer Pickering, T. A. Glendinning, and J. H. Millar.

I have no intention of inflicting upon you any account of the different phases through which these researches went, or of the controversy to which they gave rise. There seems to be every probability that differences of opinion will continue to exist until some new methods of attack are devised which mark as great a departure from existing methods as O'Sullivan's did from those of his predecessors.

The "starch question" had its birth in 1814, when Kirchof observed that starch under the influence of the "vegetable albumin" of grain yields a crystallisable sugar, and that during the malting process the potential action of this transforming agent is much intensified. That is just over a hundred years ago. So complex and difficult is the whole subject that I venture to predict that chemists will not be united upon it even in its bicentenary in 2014 A.D., and that the starch controversy will still be in existence then, and perhaps giving rise to as much frictional heat as has been displayed in and before our time. Of it may be truly said, "Quot homines tot sententiæ; suus cuique mos."

During the seventies and eighties of the last century, a period which may be regarded as the halcyon days of my native town of Burton-on-Trent, there had been assembled gradually a small but very active colony of chemists, all of whom were attached to the various breweries in the town. In those happy days, when Limited Companies were few, and before excessive competition and trade jealousies had marred the good relations which used to exist between the various firms, there was a free interchange of ideas amongst the scientific members of the staffs with regard to new discoveries in bacteriology and chemistry. This resulted in the inauguration of a small and quite informal dining club, which ultimately received the name of the "Bacterium Club." It had no rules and no subscription, but met at regular intervals at the houses of a few of our Members, and included two local medical men interested in science. This Club was started about 1876, the original members being C. O'Sullivan, Peter Griess, P. B. Mason, H. E. Bridgman, E. A. P. Forster, Adrian J. Brown, W. Odling, and myself. We were subsequently joined by James O'Sullivan, G. H. Harrow, F. E. Lott, John Heron, and G. H. Morris.*

There were no formal papers read at our meetings, nor were any subjects for discussion pre-arranged, but these generally turned upon the more recent scientific discoveries in chemistry and biology, especially those which directly or indirectly had any bearing on our own particular industry. These discussions were often very far reaching, for there were those amongst us who saw that in the by no means distant future some of the problems we dealt with must sooner or later have a great influence on the well-being of mankind and the

* Of these 14 members four were, or subsequently became, Fellows of the Royal Society.

whole attitude of the medical profession towards the etiology of infectious diseases. It is true that the reasoned discussions on these points often went far beyond the strict evidence at that time available, but on looking back I am struck with the fact that so many of the forecasts of the future development and applications of bacteriology which were made by this small scientific community have been so fully realised.

Occasionally our symposia were attended, much to our advantage, by some of our scientific friends from London and other places, and amongst these I must specially mention my old friend, contemporary, and fellow student, Dr. H. E. Armstrong, who with his wide knowledge and sympathies, his infectious enthusiasm, and his well-known powers of trenchant criticism, used to enliven our discussions, encourage our efforts, and give us a stimulus and mental tonic which was occasionally needed by dwellers in the somewhat relaxing and enervating climate of the Trent Valley.

I had been studying the bacteriology of beer for about five years, instigated thereto, as I have already said, by the *Études sur le Vin*, publishing nothing, but freely discussing all the points with my friends, when Pasteur's *Études sur la Bière* appeared in 1876, at once producing a profound impression on the brewing industry throughout the world.

I had the pleasure of finding in that book a full confirmation of my own observations on the principal disease organisms of beer, which I had been able to recognise at a somewhat earlier date by an application of the principles already laid down by its illustrious author. I take no credit to myself for this anticipation, which might have been made by anyone who had made himself familiar with the new ideas on fermentation, which Pasteur had been teaching for years, and which now received a new, and, to many people, an unexpected application.

It is only necessary to compare the brewing literature prior and subsequent to 1876 to see the immediate and far-reaching effect produced by the *Studies on Beer*.

The work was translated into English in 1879 under the title of *Studies on Fermentation*. How far this change of title was made with the consent of the author does not appear in the translator's preface, but, be that as it may, the alteration was a very unfortunate one, since it has helped to obscure the very close connection which exists between the subsequent developments of Pasteur's work in preventive medicine and these earlier studies on beer. This is a phase in the history of bacteriology which apparently is never dwelt upon in the education of our medical students, with the result that it is rare at the present time to find a medical man who has any clear conception of the true sources of that mighty and fertilising stream of applied science which supplies his daily needs. We, who know these facts, may be justly proud as brewers that our industry has played such an important part as a stepping stone to great things, and we shall in consequence be the better able to meet those specious arguments and wild assertions which would try to convince the world that we are to be classed amongst the enemies of the human race. That these ideas are not shared by the countrymen of Pasteur is indicated by the inscription on his tomb in the Pasteur Institute in Paris. There we find inscribed in due chronological order all the links in the unbroken chain of his achievements, including the *Études sur le Vin* and the *Études sur la Bière*, which immediately precede the record of his work on infectious diseases, vaccines, and hydrophobia. If this country had been fortunate enough to claim this illustrious man, cannot we imagine the fanatical outcry which would have been raised had the words "wine" and "beer" appeared on his monument wheresoever erected? Our Allies on the other side of the Channel take a more sane and philosophical view of such things.

It is far from my intention to give you any detailed account of the contents and scope of the *Études sur la Bière*, a work which I assume is well known to all of you, but, since it marks an important turning point in the career of Pasteur, leading him straight on to the study of infectious diseases, it is desirable to consider how it originated, and the part it played in the general scheme of his life's work.

The book may be fitly described as the "war child" of its author, begotten in misfortune, but destined for a great and unexpected future.

The applications of his discoveries to industrial needs were always in the mind of Pasteur, who saw perhaps more clearly than any of his contemporaries the importance of founding industrial practice on a solid substratum of science. Moreover, he had always advocated scientific instruction in the schools, and the introduction of scientific methods into the administration of the country. That these representations fell on deaf ears need not surprise us in the light of our own experience a generation and a half afterwards. The misfortunes which fell upon his beloved country in 1870—71 almost overwhelmed him, but he was still mindful of the cause for which he had striven so hard.

Soon after the disaster of Sedan we find him writing to his old pupil Raulin words which have a strange significance for us at the present time. He says, "We *savants* were indeed right when we deplored the poverty of the Department of Public Instruction! The real cause of our misfortunes lies there. It is not with impunity—as it will one day be recognised too late—that a great nation is allowed to lose its intellectual standard. But, as you say, if we rise again from those disasters we shall again see our statesmen lose themselves in endless discussions on forms of government and abstract political questions, instead of going to the root of the matter. We are paying the penalty of fifty years' forgetfulness of science, of its conditions of development, of its immense influence on the destiny of a great people, and of all that might have assisted the diffusion of light."

It was with this certainty that the short-comings of his nation were due to the neglect of science and its applications that Pasteur, immediately after the war, began to look around to find some further industry which could be advanced by scientific research. He had already placed his mark on wine making, and on the silk industry, and this time his choice fell on the manufacture of *beer*, mainly, he tells us, because this was an industry wherein France was at that time undoubtedly surpassed by Germany. He was also no doubt influenced in his choice by the recollection of his earlier experiences at Lille, which had brought him into intimate contact with brewers and distillers, and had affected the whole current of his subsequent work.

The object with which he set out, as explained in the preface to his *Études sur la Bière*, was to discover a process of manufacture "independent of season and locality, which should obviate the necessity of having recourse to the costly methods of cooling employed in existing processes, and at the same time secure the preservation of its products for any length of time."

His great desire was to teach his compatriots how to produce "une bière de la revanche nationale" which should entirely supplant the imported product of the hated Teuton. These researches on beer occupied Pasteur's attention from 1871 to 1876.

It is true that the particular aims and object he had in view were not realised, mainly owing to his proposed new methods not being very practical; on the other hand the influence which his work had on all the various existing processes of brewing throughout the world was immense, and by the irony of fate the enemy nation was one of the first to benefit by it.

But far greater consequences followed from this five years' work on beer. We have it on the authority of his son-in-law and biographer that it was this particular work which opened out before him glimpses of light on human pathology, whilst new and unexpected visions rose before his sight. As he continued to study the maladies of beer and their causes, the general ideas on the origin and nature of infectious diseases, ideas which had been floating in his mind for years, began to take more definite shape and directed his footsteps along a new path of experimental enquiry, which led him to discoveries of incalculable benefit. He was like Saul, the son of Kish, who went out to seek his father's asses, and founded a kingdom.

The conviction in the mind of Pasteur had always been very strong that there was more than a mere analogy between the phenomena of fermentation and those of the contagious and infectious diseases of men and animals. We find him remarking even as far back as 1860, when describing to the French Academy some of his early work on fermentation: "What would be most desirable would be to push those studies far enough to prepare the road for a serious research into the origin of diseases." Again in 1863, during an interview with Napoleon III, who was encouraging him to turn his attention to the maladies of wine, Pasteur tells us that he assured the Emperor that it was his only ambition "to arrive at a knowledge of the causes of putrid and contagious diseases"; and further, in the preface of the *Études sur la Bière*, he hazards the prediction that these, his latest researches, will throw more light on their etiology.

A less cautious man would have enlarged on these ideas and promulgated the germ theory of disease long before its requisite proof was forthcoming; but that was not the way of Pasteur, who abhorred hasty generalisation, and demanded that each link in the chain of evidence should be separately forged and experimentally tested. Although he had great prophetic insight and the vivid imagination of genius, he never let these qualities over-ride his reason; "ever insisting on experimental proofs, he constrained his exalted imagination, so as to follow calmly and patiently the road of experimental method." His great endeavour at all times was to curb the hasty generalisations and interpretations of his results which the over-zeal of some of his adherents gave rise to.

Recognising in 1876 that the world was on the threshold of great discoveries and advances in medicine, he thus sounds the needed note of warning in the *Études sur la Bière*: "Unfortunately there is amongst physicians a tendency to generalise by anticipation. Many of them are men of rare natural or acquired talent, endowed with keen powers of intellect and the art of expressing themselves fluently and persuasively; but the more eminent they are the more they are occupied by the duties of their profession, and the less leisure they have for the work of investigation . . . . They eagerly seize on easy and plausible theories, readily adapted for statement, which is general and vague just in proportion to the unsoundness of the facts on which they are based. When we see beer and wine undergo radical changes in consequence of the harbour which these liquids afford to microscopic organisms that introduce themselves invisibly and unsought into it, and swarm subsequently therein, how can we help imagining that similar changes may and do take place in the case of man and animals? Should we, however, be disposed to think that such a thing must hold true because it seems both probable and possible, we must, before asserting our belief, recall to mind the epigraph of this work: the greatest aberration of the mind is to believe a thing to be because we desire it."

From these cautious and wise words of Pasteur we can well imagine what would have been his attitude towards some of the popular medical theories of the present day, and how the vials of his wrath would have been poured out on those Schools of Medicine which dogmatically teach their students the latest theory of immunity without any warning that it is not a consistent theory which has withstood experimental tests and is explanatory of the true mechanism of the processes involved, but is a mere hypothesis or *memoria technica* of a particularly artificial kind, with a nomenclature increasing in hideous complexity with every newly discovered fact; useful, perhaps, to hang those facts on temporarily, but certainly not to to be accepted as a final explanation.

Pasteur suffered much from medical men throughout his career, especially from those of his own country. He speaks of the tyranny of the medical education of his day, which weighed down the public mind, and of a Medical Press in ignorance of the true principles of experimental method.

Nevertheless he often regretted that he had not himself received a medical education, a regret which we cannot share, since he, too, might have been involved in its toils. It was a perfectly free external agency, untrammelled by narrow teaching and professional convention, which the Medicine of that day required to stir it from its lethargy, and to direct its energies into the more promising channels of experimental science.

In 1873, whilst he was still engaged in his studies of beer, Pasteur was elected as a Free Associate of the Academy of Medicine, which brought him into closer contact with the great physiologist, Claude Bernard. This fact no doubt profoundly influenced the direction of his subsequent work. His quondam assistant and colleague, Duclaux, in his admirable *Histoire d'un Esprit*, justly remarks that Pasteur's physiological work was the development and complement of his work in fermentation. It had long been his desire to throw himself into pathology, and the studies on beer which he had just completed in 1877 had armed him with a special technique which he was now fully prepared to use in other directions. It was Davaine's contemporary studies in the bacillus of anthrax which gave just the required determining impulse.

Bacteria had already been discovered associated with erysipelas, hospital gangrene, puerperal fever, diphtheria, and some other maladies, but doubts were still expressed as to whether these organisms were the real active causes, or whether the associated organisms were mere epi-phenomena and independent of the true virus of the disease.

Pasteur's first attack was on *Anthrax*, which he was able by rigid experimental proof to show was caused solely by the invasion of the special form of bacillus which had been observed by Rayer as far back as 1850.

His further studies on chicken cholera and swine fever, his great discoveries with regard to the immunisation against infectious diseases by the attenuation of the virus, and finally his study of hydrophobia and its treatment, laid the foundation for all the wonderful developments of preventive medicine and of hygiene which we have seen during the past 35 years.

I have no intention of enlarging on this phase of Pasteur's work, which he continued to follow up to the time of his death, but what I do wish to emphasize is that in the unbroken chain of interdependent researches, extending from a mere crystallographic discovery to the most modern triumphs of medicine, there are two important and essential links represented by *wine* and *beer* respectively; and that the more one studies the evidence afforded by Pasteur himself and by his contemporaries the more one becomes impressed with the enormous importance to be attached to the fortunate chance which in 1871 directed the mind of this great man to *beer* and its improved manufacture.

Antiseptic and aseptic surgery, although not in the same line of descent as the one I have just traced, were the direct outcome of Pasteur's work, branching off from the main stem about the year 1865. The story has been told many times, but I have seldom if ever seen any reference to the fact that modern surgery, like preventive medicine, is a child of the fermentation industries.

Shocked by the prevalence and fatality of the hospital diseases which attended the surgery of that period, our countryman Joseph Lister, of immortal memory, had been searching for their causes and the power to alleviate them. His physiological observations on inflammation were the precursors of his pathological work, and about the year 1864 he became acquainted with the early studies of Pasteur on the alcoholic, lactic and butyric fermentations, in which the phenomena of fermentation and putrefaction were definitely connected with the presence of micro-organisms.

Musing on these facts, and combining them with his own observations, Lister by a process of analogical reasoning was led to associate the inflammation of open wounds with the action of micro-organisms introduced from without, and, inspired by this idea, he commenced a treatment which in his hands and those of others has gradually led to the triumphs of aseptic surgery. The fact of immediate interest to us is that the researches from which Lister drew his inspiration were those suggested to Pasteur by his observations in M. Biot's distillery at Lille, an episode to which reference has already been made.

Pasteur apparently heard for the first time of Lister and his work through a letter from Lister dated February 10th, 1874, which is quoted in the *Études sur la Bière.* This was about 10 years after Lister had commenced his researches, and at a time when he could already show a marked measure of success of the new surgical treatment.

The methods of these two great men in attacking a problem were so entirely different that it may be regarded as fortunate they did not come into contact at a much earlier period. The analogical mode of reasoning which gave Lister his first great impetus was always anathema to Pasteur, as he showed on several occasions, and more especially in his recorded conversation with Dr. Déclat.*

Had Lister consulted Pasteur at the very outset of his work I think he would have been discouraged by some such argument as this: "It is true that there are certain analogies between the phenomena of fermentations and putrefactions and those of infectious and contagious diseases, but up to the present these are only analogies, since it has never yet been shown that any single infectious disease is caused by a micro-organism. You have no right, therefore, to commence with an assumption which entirely begs the question. You are beginning at the wrong end of the problem, arguing from the general to the particular. In order to give that finality to your experiments and observations which will prove your case you must, in the first place, be able to show that the particular disease you are investigating is always accompanied with the appearance of a definite micro-organism; secondly, you must be able to isolate that organism and to cultivate it in successive generations in media rigorously shielded from the advent of other organisms; and finally you must show that these cultures and sub-cultures when introduced into a healthy subject have the power of again initiating the disease."

To this Lister, if he had not been permanently discouraged from following up his ideas and pre-arranged methods, would probably have replied as follows: "I admit all you say, that positive certainty is only attainable by the rigorous laboratory methods and controls you advocate; but to get this proof we may have to wait years; the

* See Vallery-Radot's *Life of Pasteur*, Eng. trans., 2, 1.

requisite technique has not yet been elaborated—meanwhile my patients are dying. Thanks to you a ray of hope has broken through the clouds and a possibility, nay, to my mind a great probability of alleviation has presented itself. I shall work on, things cannot be worse, they may turn out infinitely better. My test of failure or success must depend on 'experiential' methods: on constant repetition of treatment and close observation, using such controls as may be possible. With an extended clinic, plenty of time, and a careful statistical grouping of the results, I shall be able to prove or disprove my thesis. After all, Jenner and his successors had no better method than mine." In this spirit the great pioneer Lister went on his way and conquered still another kingdom.

When the Pasteur Institute was inaugurated in Paris in 1888 as a memorial of Pasteur, during the latter years of his life, its founders, acting no doubt on his advice, gave due expression to the bond of union between the higher branches of medical research and the fermentation industries by establishing a department of applied bacteriology, not, it is true, to be wholly engaged in technological work and teaching, but to keep the Institute in close touch with those industries of the country which are more or less dependent on fermentation and enzymology; such for instance as wine and cider making, brewing and distilling, vinegar making, bread making and tanning.

I imagine that this wise action of the founders of the first Pasteur Institute was directed by the certain knowledge that medicine and these industries have not only much in common, but that in the case of the industries the problems are often presented in a much simpler form, and one more amenable to investigation.

The same general plan of organisation has also been carried out in the Pasteur Institute at Lille, presided over by Dr. A. Calmette; the idea is essentially French, and the combination is one which has been particularly fruitful.

Unfortunately the only Institution of the kind existing in this country, the Lister Institute of Preventive Medicine, has not taken the same broad view of these questions. It is true that from the Biochemical Department of the Institute there has proceeded some excellent work on yeast and its zymases, but speaking generally the medical men and the biochemists, unlike their French confrères, have, at any rate officially, cut themselves adrift from the fermentation industries, which I fear they regard with a certain amount of disdain as being remote from their higher studies.

Having said so much I may be rightly challenged to descend from generalities to particulars, and to indicate certain directions in which there would be reasonable hope of brewing research being in the future directly or indirectly helpful to the physiologist and pathologist. Cases of this kind are so numerous that the real difficulty lies in selecting them for purposes of illustration.

Of ever-increasing importance to physiology is the study of *enzymes*, which are so largely used by living cells, either animal or vegetable, as agents in carrying out their metabolic life-processes. These enzymes may either be produced *ad hoc* in the cells in which they are required for immediate use, or, on the other hand, they may be manufactured in vastly larger amounts than are individually required by certain groups of cells differentiated from other parts of the organism, and then secreted for distribution to the parts where they are needed. As examples of the first class, we may take the auto-production of invertase and zymase in the yeast-cell, and the similar production of *diastase* in cells of the parenchyma of a green leaf for the dissolution of the minute starch-granules formed during the assimilating processes of the leaf under the action of sunlight. An example of the second class of phenomena is afforded by the secretion of diastase during the germination of a cereal grain, by the highly differentiated epithelial layer of the scutellum. This secreted diastase projected into the endosperm dissolves the starch, and fits it for assimilation by the young plant.

The number of recognised enzymes produced during the vital activity of plants and animals is considerable and a large amount of work has been done on the nature of the transformation products and the velocities of the reactions induced by diastase, invertase, pepsin, trypsin, etc.

Most of this work, however, has been carried out *in vitro*, and, therefore, under conditions far removed from those existing in a plant or animal, when the living cell and the new-born enzymes can immediately co-operate. Under these last-mentioned conditions the time-values of the reactions are of an entirely different order from those given by experiments made *in vitro*.

My attention was first drawn to this fact many years ago when working on the action of diastase on solid ungelatinised starch-granules, and I was at once struck with its physiological importance.

There are certain starches which in their solid granular form may remain in contact with a highly diastatic liquid, such as a cold-water extract of malt, for an indefinite time without showing any signs of disintegration or dissolution, even if the liquid is kept in constant movement. This resistance to the action of the enzyme at once disappears if a trace of *yeast* is added.

Although neither the enzyme alone nor the yeast alone can produce any effect, the combined action of the two agents working simultaneously results in immediate pitting and disintegration of the starch-granules, and the production of alcohol and CO_2. Exactly the same thing occurs in the degradation of the dextrins and malto-dextrins of the mixed products of hydrolysis of starch; the addition of yeast, *plus* a minute trace of diastase, results in this case in the complete fermentation of the whole of the starch-products, whereas without the trace of diastase the yeast exercises a selective action on the sugars present, leaving untouched the dextrinous portions of the transformation products.*

This activation of the enzyme diastase by the presence of a living cell appears to be a general phenomenon. I found abundant evidence of it when working on the germination of seeds, the chemistry of the malting process, and the physiology of foliage leaves. It is difficult for those who have not experienced it to realise how infinitesimal an amount of an enzyme is required to produce quite a large effect when it is in the presence of living cells capable of assimilating and using the products of change as fast as they are formed.

The most probable explanation is that the chemical changes induced by enzymes come under the designation of "reversible" or balanced reactions. In such cases when the products of the reaction are not allowed sensibly to accumulate by being removed as fast as they are formed, the "chemical gradient" is at a maximum and the highest rate of transformation of the substance is maintained.

But this is only one side of the question: the activation of the enzyme results in the increased supply of assimilable food material

* These observations at once give the key to the well-known differences in the fermentations of the distillers' and brewers' worts respectively.

to the cells, whose growth and reproduction are correspondingly influenced.

Excellent examples of this stimulated growth are to be found in brewing practice; for instance, in the effects produced by "dressing" fermentations with a little wheat or malt flour, or by the addition of very minute quantities of a cold-water extract of malt to a finished beer to excite an after-fermentation and to bring it into "condition." In both instances the effect is initially produced by the presence of a little diastase which, co-operating with the yeast, breaks down some of the dextrins of the worts or beers, and renders them directly assimilable by the yeast, which consequently continues its growth and activity for a longer period. These are all common every day phenomena in a brewery, but they are nevertheless worthy of the careful attention of physiologists, for they are intimately connected with and explanatory of some important problems dealing with animal nutrition, which are attracting a considerable amount of attention at the present time.

Up to recently it has been generally believed that an animal can be kept in a vigorous and healthy state provided we supply it with a mixed food containing suitable proportions of carbohydrates, proteïns, fats, and mineral salts. Recent investigation has, however, shown that before the animal can make effective use of such food materials and continue in healthy and vigorous growth something else is required. This "something," which is a necessary auxiliary to the dietary, need only be present to an extremely minute extent, but is essential to the animal for the due assimilation of the supplied food.

At the present time we have no certain knowledge either as to the true nature of these auxiliaries, or as to their mode of action in stimulating growth.

To one indeterminate class of such substances the term "vitamine" has been assigned, and in another case, where the activation of growth is exercised on yeast, we have the name "bios" introduced by a Belgian chemist.

What is certain is that these "vitamines," "auximones," or whatever we choose to call them, do not produce their effects by the actual amount of material they contribute to animal growth, for their direct food value is infinitely small; we must therefore look for an explanation to *indirect* effects, such as may be produced by an enzyme, or by those chemical messengers of the body, the "hormones."

Judging from the somewhat analogous cases which I have just quoted I am inclined to think that the activity of vitamines will find its explanation in the combined effect of a hydrolysing enzyme, working in conjunction with living cells, and that the physiologist will once more go the brewers' fermentation vats for light and leading.

Whilst on this question of "vitamines" I may state that it has been suggested by Dr. H. E. Armstrong that beer may be rich in these dietetic auxiliaries, and that the popular estimation in which it is held as a valuable adjunct to a dietary may be largely owing to this cause. A careful and systematic examination of this question by really competent workers is most desirable, especially at the present time.

A consideration of the combined action of enzymes and living cells leads us on to questions involving *symbiosis*, where we may have two distinct organisms living side by side, either one contributing something essential to the well-being of the other. We have several examples of this kind in the fermentation industries, none of which have yet given up all the information of which they are capable. The "amylo" process for the conversion of raw grain into alcohol is a case in point. This is based on certain discoveries of Dr. A. Calmette the Director of the Pasteur Institute at Lille.

It was my privilege about seventeen years ago to study this process in the distillery of M. Collette at Séclin, in company with the late Sir Henry Roscoe and the late Dr. Allen Macfadyen. It consists essentially in sowing in a sterilised mash of raw grain a particular fungus, capable at a suitable temperature of ramifying rapidly through the mass, and of producing, by means of its mycelia, small but sensible amounts of a diastase. This operation is then followed up by a seeding with a certain variety of yeast, which also reproduces itself with great facility. The result of the co-operation of the two organisms is to carry on simultaneously the conversion of the starch and its complete transformation into alcohol and CO_2.

This extremely beautiful application of the more refined methods of bacteriology to an industrial process is well worth the attention of physiologists, for in it is exemplified on a large scale all the phenomena of symbiosis in their simplest form, and free from many of the complications which occur in natural symbiosis.

Still another example is afforded by the manufacture of ginger beer under the action of the "ginger beer plant," a process which was investigated and thoroughly explained by the late Dr. Marshall Ward.

There is still a great deal of information of the highest interest to the physiologist to be obtained by a closer study of the conditions of yeast growth in brewery fermentations with regard to the influence of free oxygen in effecting cell reproduction. Problems of this kind, which are extremely difficult to deal with in multicellular organisms, become greatly simplified and much more amenable to rigorous experimental investigation if we start with a unicellular organism like yeast.

That free oxygen is necessary for the reproduction of the yeast cell was a fact established by Pasteur, who noted, however, that the fermentative function of the cell was still exercised in the absence of oxygen.

Owing to what we now know was a faulty interpretation of his results, Pasteur concluded that the two functions of the yeast cell, reproduction and fermentative power, are antithetic; that when the cell is supplied with plenty of oxygen reproduction occurs, but the fermentative faculty is in abeyance; whilst, when the yeast is placed under anaërobic conditions, reproduction ceases, but the fermentative faculty is at a maximum.

Following up this train of reasoning, Pasteur was led to regard fermentation as the expression of "life without air," and he put forward the idea that it was the abstraction of the needed oxygen from the sugar molecule which led to its fermentative decomposition.

This was the view generally adopted until Adrian Brown showed that, even during access of abundance of oxygen and when reproduction is at a maximum, the fermentative power remains unimpaired, and is always proportional to the number of cells present in unit volume of the liquid, all other conditions being the same.

My own investigations have further convinced me that the final reproductive power of a yeast cell, all other conditions being favourable, depends on the previous history of the cell as regards oxygen absorption.

When yeast cells are sparsely scattered through a suitable nutrient liquid containing oxygen in solution, the first recognisable action of the cells is rapidly to absorb this dissolved oxygen, which is equally divided amongst the individual cells. Each cell, therefore, receives an equal "oxygen charge," and direct experiment has shown me, by a series of cell-counts, that the final reproductive power of each cell, that is to say, the number of fissions which it will undergo, is, within certain well defined limits, directly proportional to the intensity of this initial "oxygen charge," provided there is no further access of oxygen during the process. This explains not only the somewhat puzzling fact, observed by Adrian Brown, that the maximal number of cells attained in unit volume of the liquid is independent of the initial seeding, but also why the *rate* of reproduction is linear instead of logarithmic, as one might at first sight expect it to be.

It is in the highest degree probable that the laws which express the quantitative relation between the reproduction of yeast cells and oxygen supply are of general application to cell growth of all kinds, and ought therefore to be of special interest to physiologists.*

The industrial process of *malting*, as we shall see presently, also involves many interesting scientific questions bearing on the dormant period of seeds, the chemistry of germination, and numerous phenomena of plant growth and metabolism; but for the present I have said enough to prove that the biologist cannot afford to ignore the various everyday processes of the brewer which cover such a wide range, and that the biochemistry of brewing and the lessons to be derived from it are by no means confined to the bacteriological field which was first explored by Pasteur.

In the years 1885—6 I began to ponder over the various chemical and physical changes involved in the conversion of raw barley into malt, and soon found that there was here a vast unexplored field awaiting tillage.

Besides being conducted on purely empirical lines, practical malting, up to the year 1881, owing to the incidence of the malt tax, had

* The phenomenon of "agglutination" or "clumping" of bacteria under the influence of hypothetical "agglutinins" is used largely by pathologists as an aid to diagnosis in immunisation. A similar phenomenon of "clumping" is sometimes observed in yeast, under conditions which are at present but little understood but which are amenable to experimental investigation.

suffered for several generations from a series of stringent legislative enactments which had retarded its development. Up to that time there was no real freedom of action on the part of the maltster as regards the selection of his barley and the time of steeping, unless he was prepared to submit to increased duty charges, and the regulations with regard to sprinkling the grain on the floors were of an absurdly stringent nature. The consequence was that the whole malting process had been developed in the direction of duty saving rather than on the lines of producing the best possible malt for the brewer. This handicap was removed when the excise tax was taken from the malt and imposed on the beer, a change which stimulated the malting industry and led to various experiments in the direction of improvement.

These early attempts were guided by no fixed principle. Widely divergent opinions existed as to the best treatment of the grain on the malting floors, and for not one of these opinions could any rational explanation be given.

Thanks to the pioneering work of O'Sullivan, and to the analytical methods which he introduced, we were beginning to know something about the respective composition of barley and malt, at any rate as regards the main carbohydrate constituents, but we were still absolutely in the dark as regards the nature and mechanism of the vital processes at work in converting the raw grain into malt.

During the long mental incubation period which should always precede any new departure of importance, there arose in my mind a feeling of certainty that no real progress could be made in such an investigation as long as we adhered to the old plan of analysing the grain *as a whole*, before and after germination. The grain of barley is highly differentiated into various organs and tissues, and to deal with it in a wholesale way is about as reasonable as it would be to analyse a whole animal or plant when investigating its life-changes. It was evident that no real advance could be expected until the functions of the various parts of the grain could be investigated separately.

As a preliminary to the work which was now mapped out, a knowledge of the anatomy and histology of the grain was requisite, and I found that in this respect a good foundation had been laid by Holzner and Lermer, who had studied and figured in considerable detail the minute structure of a grain of barley, and also to some extent its developmental history. It has always been a matter of wonder to me that, having gone so far, these observers should not have extended their botanical studies to investigating the physiological meaning of what they saw and drew so well.

However, here was a good histological foundation, and I soon learned my way about in a grain of barley by repeating and extending these observations, by dissections, and cutting sections for the microscope.

But I soon became aware that my knowledge of the morphology and physiology of plants was not sufficient to carry me through all the work I had planned, and therefore I had, in the first instance, to spend a considerable amount of time in a preliminary study of botany in its various branches before I felt sufficiently equipped for attacking the problem before me.

When fairly started I found many good and helpful friends who took a keen interest in my work and afforded me much encouragement both by the spoken and written word. Amongst these friends to whom I owe a debt of gratitude I would specially mention on the side of the botanists Sir W. Thiselton Dyer, Professor Marshall Ward, and Professor Reynolds Green; and on the physiological side Sir Michael Foster and Dr. Sheridan Lee. Although I am speaking of a time not more than 25 years ago, all these old friends, with one single exception, are, alas! no longer with us.

After four years of such work in the off time of my business, and with the constant assistance of the late Dr. G. H. Morris in the laboratory, we were in a position to give to the Chemical Society, in 1890, a reasoned account of these researches in a paper entitled "Researches on the Germination of Some of the Gramineæ."

I think I may safely claim that in this paper we gave for the first time an exposition of the principal chemical and morphological changes which go on in the barley grain during the early stages of germination, and the means of understanding, and, later on, even of controlling, some of the phases of practical malting operations.

This earlier work dealt mainly with the transformation and migration of the reserve carbohydrate constituents of the grain during germination. It was further elaborated in 1896 in conjunction with Mr. F. Escombe whilst working in the Jodrell Laboratory at Kew.

Still later some of these problems were further developed under my direction in the Guinness Research Laboratory, and afterwards in my own laboratory in London with the assistance of Mr. R. M. Filmer and Mr. F. M. Dyke, especially with regard to the transformation and migration of the proteïnic constituents of the grain.

One of the main conclusions arrived at in these investigations was that the relation of the embryo of the grain to the endosperm, which contains the nutrient necessary for the maintenance of the young plant in its earlier stages of growth, is that of a vegetable parasite to its host.

There is no actual structural connection between the embryo or germ and its endosperm, only one of close apposition which is maintained by the common integuments of the two. When these investing integuments are removed the embryo can be readily separated from the endosperm without any damage to the tissues. It was very interesting to find that the embryo when thus separated from its natural host can, by the application of suitable nutrients, be put out to "wet-nurse," and that perfect plants can be reared in this way without the aid of the natural endosperm. This of course gave an opportunity of carrying out experiments on the separated embryos and endosperms which threw much light on their physiological relations.

The portion of the embryo which is in close apposition to the endosperm, the *scutellum*, has on its surface a remarkable *epithelium* consisting of elongated cells much resembling in appearance the *villi* of the small intestine of an animal.

It can be readily shown, especially on the excised embryo, that these epithelial cells of the scutellum are able to secrete a diastatic enzyme which, projected into the endosperm, attacks, corrodes, and dissolves the starch granules, thus providing soluble carbohydrates (sugars) which are then absorbed by the epithelium and passed on for the general nutrition of the embryo as it develops into the young plant. There is good reason to believe also that the same epithelial cells secrete a proteolytic ferment, which, in a similar manner, acts on the proteïnic contents of the endosperm.

But here must be mentioned a very interesting fact which was found to have an important bearing on practical malting. The starch granules forming the main reserve-material of the grain are packed tightly in the endosperm cells, which are invested with a thin membrane, constituting the cell-wall. Until this is broken down by some means the diastatic secretion which is poured into the endosperm by the scutellar epithelium cannot gain ready access to the starch granules to dissolve them and render them assimilable by the young plant. We consequently find in the barley corn a special provision for breaking down the cell walls as a preliminary to the attack on the cell contents. This is supplied by the secretion, in part by the scutellar epithelium, and in part by the triple aleurone layer, of an enzyme capable of attacking and, under favourable conditions, of entirely destroying the cell-wall. By these means the whole solid contents of the endosperm can be broken down and utilised by the young plant. This in fact is what occurs under natural conditions of germination and growth in the field, the endosperm supplying sufficient food material to the plant until such time as by the development of its leaf and root system it can look after itself.

The art of the maltster consists in directing, co-ordinating, and limiting these natural physiological processes of interaction between the embryo and endosperm, and in making them as far as possible subservient to a particular end, which, briefly stated, is that of producing a highly friable and tender malt which when suitably dried and coarsely milled is capable under the infusion method of mashing of yielding the maximal amount of extract of the right composition.

Whereas the natural processes of germination in the field have for their final object the transference of the sum total of the reserve materials of the seed into the young plant, the aim of the maltster is to restrict the development of the plantlet to the lowest possible point consistent with a complete "modification" of the endosperm contents, by limiting the rate of migration of these reserve materials, and reducing to a minimum the respiratory processes which mean further loss of extract.

The means at the disposal of the maltster for carrying out his purpose are very few in number, and consist mainly in the careful regulation of water supply at all stages, in the duration of the process, in the proper control of the floor temperature, in judicious aëration by turning, and lastly in enlightened methods of drying and curing.

Experience can alone determine the exact treatment which a given barley requires for producing the best results, but we are no longer working in the dark. We now know the intimate scientific principles underlying the flooring and kilning processes, and their application to practice during the last 25 years or so has had a very salutary effect on malting in this country, whilst the analytical criteria which can now be applied have placed the whole industry on a very different footing. Malting has at last emerged from its empirical stage, and it is only fair to our English maltsters to say that, unlike the majority of brewers, they have been by no means slow in realising the aid which science can bring to them.

Pure Yeast Culture and Yeast Purification.

It was in or about the year 1879 that Emil Chr. Hansen commenced at the Carlsberg Laboratory in Copenhagen the work which was to render him famous.

Instead of concentrating his attention on the bacterial flora of worts and beers, as Pasteur had done, he elected to investigate the life-history of the alcoholic yeasts and the yeast-like organisms which occur in Nature, in the cultivated levures of the brewer, and in the air surrounding breweries. A model piece of work of this kind is Hansen's critical study of *Saccharomyces apiculatus* and its hibernation and annual cycle of development. As he continued his work he found that the yeasts and yeast-like cells possess properties which are fixed in character and have specific values, and that although often resembling each other in appearance they can be readily distinguished by such characters as their selective power in fermenting sugars, and their mode of producing endospores. Moreover he never succeeded in transforming one form into the other by any variation in the conditions of cultivation provided he started in each case with an absolutely pure race derived from a single cell.

The technique for cultivation from a single cell was devised by Hansen, the original mother-cell being previously selected and ear-marked under the microscope.

It appears to have been about the year 1883 when Hansen first began to suspect, what had already been proved to our satisfaction at Burton-on-Trent, that the maladies of beer are by no means confined to organisms of the bacterial class, but that many irregularities of flavour, odour, and clarification must be put down to the influence not of bacteria, but of *yeasts* of adventitious origin. He describes how his attention was drawn to this by the occurrence in Denmark and in Germany of a considerable loss of beer through "yeast turbidity."

I quote his own words describing this particular malady as it occurs in lager beers, since they exactly apply also to the early stages of development of wild-yeast turbidity in our high-fermentation beers. He says: "After the bottom-fermentation beer has been stored sufficiently long in the cellars and is racked off it is perfectly clear, and it is impossible with the unaided eye to discover any yeast in it. As soon, however, as the casks or bottles into which it has been drawn are exposed for a few days to a temperature higher than that of the lager cellar, for instance the temperature of an ordinary room, a more or less abundant deposit of yeast is formed, and it only requires a small amount of agitation to distribute this in the liquid and to render it turbid."

Hansen then goes on to describe how he isolated from such a faulty beer three species of alcoholic ferments: ordinary *Saccharomyces cerevisiæ* of the bottom-fermentation type, representing the main portion of the brewery yeast; *Saccharomyces Pastorianus III*, a form of top-fermentation yeast; and *Saccharomyces ellipsoideus II*, a form of bottom yeast.

These three forms of yeast, isolated in pure culture, were then seeded into sterile wort separately, and mixed in various proportions. When the primary fermentation had ceased the beer was poured off and maintained in sterile flasks for a considerable time at ordinary temperature.

Only one of these cultures gave a product which satisfactorily retained its brilliancy, the one seeded with the pure *Saccharomyces cerevisiæ*. In the product from the mixed cultures the yeast-turbidity always asserted itself.

This was the first suggestion to Hansen of the possible value of cultures of pure yeast on a large scale, but there is another series of experiments to which I must refer, since to my mind it explains his comparative neglect of an aspect of the subject which, at any rate to us English brewers, is of capital importance, and can readily stultify any good effects which may be reasonably expected from the introduction of pure cultures. In the experiments just described it will be noted that when Hansen introduced into the pitching or seed yeast a little of either of the adventitious yeasts *Saccharomyces Pastorianus III* or *Saccharomyces ellipsoideus II*, the malady of yeast-turbidity always asserted itself. Carrying the matter further, he tried the effect of adding traces of the same wild yeasts at the conclusion of the primary fermentation instead of at the commencement. In such cases he obtained negative results as regards subsequent yeast-turbidity, from which he concluded that the two ferments which had been demonstrated to be the cause of the malady cannot occasion it when they gain access to the beer only at the end of the primary fermentation. This asserted non-susceptibility of the finished beer to these particular wild yeasts is very surprising and certainly cannot be taken to prove a general proposition. It certainly does not apply to our English beers and the particular forms of wild yeast with which they are liable to be infected.

On the contrary, I have always found that a finished beer is very sensitive to any small infection of this kind, especially under ordinary cask conditions when slight aëration is rendered possible, and more particularly, as we shall have occasion to see later, when it is a more or less bright beer free from primary yeast, a factor which is of immense importance in the tendency to wild yeast frets.

There are some indications that Hansen himself was surprised at his own results, and he is careful to say that they concern only the two varieties of wild yeasts with which he experimented. He also utters a word of caution against drawing the conclusion that it is unnecessary to clean and renew the pitch of the store casks in the lager cellar.

Notwithstanding this I am led to conclude that this apparent non-susceptibility of the finished beer to outside infection had an unfortunate influence over Hansen, causing him to centre his ideas too much on the question of pure yeast and to underestimate the necessity for the rigid cleanliness and sterilisation of surfaces, which is difficult enough to carry out in a high-fermentation brewery and is still more difficult in a lager brewery. He never seemed to me to regard these two questions in their proper relative perspective.

In 1885 at my request, Dr. G. H. Morris, who was then working with me, went over to Copenhagen, where he spent some time with Hansen, studying all the technique of pure yeast culture. On his return we installed a Hansen apparatus at Burton and commenced operations in the brewery for testing the practicability of the process on a manufacturing scale. Each week a portion of a brewing amounting to about 50 barrels was pitched with a "pure yeast" for comparison with other portions of the same brewing which had been pitched with the ordinary brewery yeast. Sample casks of each were placed under similar conditions and frequent comparisons were made and noted. Several different races of yeast were used, all being single-cell cultures.*

After due consideration of the results, obtained during eight or nine years of such work, there is no doubt left in my mind that the pure-yeast beers, on an average, did not show any marked superiority over those brewed with the ordinary brewery yeast as regards flavour, brilliancy, and general qualities. In fact, in one respect, the advantage was with the last mentioned, since the beers from the pure yeast, unless stimulated by the addition of a little diastatic malt-extract at the time of racking, were slower in conditioning than the corresponding samples from the ordinary yeast.

The whole tendency of the evidence accumulated through these years was to show that, in this particular brewery at any rate, the occasional and sporadic appearance of wild-yeast turbidity was not traceable to initial contamination of the pitching yeast but had its origin in infection from other sources.

Since that time I have made many studies elsewhere by approaching the problem from another direction, and these later observations go far to reconcile many of the conflicting statements which have been made from time to time on the application of pure levures to English brewing.

I have already referred to the positive indications of any abnormal tendency to wild-yeast troubles which can be obtained by a process of forcing young beers in *cask* under fixed conditions of temperature.

When this very useful process is carried out systematically, and as a part of the daily routine, it is found that the incidence of wild-yeast development is very sporadic, and cuts across all the lines of cleavage produced by variations in the pitching yeast or even of the materials and mashing conditions. We may, for instance, have two successive brewings whose worts, prepared from identical materials, have the same composition and have been seeded with exactly the same yeast, yet one of these may give a beer which on forcing in cask for 10 days or so will be teeming with actively growing wild secondary yeasts,

* See G. H. Morris, "On Some Experiences in the Use of Pure Cultivated Yeast," *Journ. Inst. Brewing*, 1900, 333.

whereas the other may be quite free from them, the fermentation being carried on solely by the original primary yeast. Such phenomena occurring over and over again suggest that the true source of the wild-yeast infection is to be sought not in the pitching yeast but in small accidental contaminations derived from air or the plant itself. How small an amount of such an infection may make itself felt in a beer forced as I have described may be demonstrated by the addition to a cask of freshly racked beer of a few drops of a cultivation of one of the commoner kinds of "wild" or secondary yeasts to which English beers are subject, using as control a sample to which no such addition has been made. A few experiments made on these lines, varying the amounts of the added culture, and watching for the first signs of yeast turbidity and visible increase of the wild yeasts under the microscope, will bring home to any intelligent brewer, far better than mere words can, how dependent he is for success in the elimination of all traces of these secondary yeasts from his *plant*. Further than this they will prove to him that, *pace* Hansen and his statements, English beers, even in their finished state, are very susceptible to attacks by traces of certain secondary yeasts.

Before proceeding further with the consideration of pure yeast and how far its use is likely to be of benefit it is desirable to refer to certain conditions of common occurrence which can intensify the effects of even small infections with wild yeast, be they derived, in the first instance, either from the plant, pitching yeast, or any other source.

It is a matter of common observation that a beer which has been fined or filtered, and racked off perfectly bright, will retain its brilliancy for a much shorter time than the original beer which has merely been fined in the ordinary way and stillioned with the full amount of sedimentation left in the cask. If these operations are followed with the aid of the microscope it will be found that in the former case the treatment has had a very great effect in encouraging the growth and development of any secondary yeasts which the beer may have originally contained, and if the observations are carried further it can be shown that the mere presence of a certain small amount of healthy primary yeast-cells can exert a marked inhibitory effect on the development of the secondary forms. One way of demonstrating this is to take successive cask samples from a racking or cleansing vessel at different periods of subsidence, all within the ordinary practical limits, and then to place them in the forcing room and compare the results from time to time. If there has been any initial infection from any source, assuming that proper care has been taken in obtaining representative samples, this must be the same throughout, but the times required to produce evidence of the infection will vary considerably, the earliest indications of wild-yeast development occurring in the later drawn samples, that is to say, those containing the smallest amount of primary yeast in suspension, and the order in which these growths will appear is approximately the inverse order in which the samples have been taken. Another and perhaps more convincing method is to draw off two similar samples after the beer in the vessel has been allowed a sufficient period of rest to be racked off with but little primary yeast in suspension. If to one of these a very small amount of *fresh yeast* is added, whilst no such addition is made to the other, these two samples will be found to behave very differently in the forcing room as regards the relative times of development of secondary yeasts, and will bring out in a striking manner the inhibitory action of the primary yeast.

There can be no doubt that any wild-yeast infection to which a beer has been subjected is intensified in its effects by racking too "clean," and that any treatment which ensures a sufficiency of primary yeast and the conditions for maintaining these cells in an active healthy state during the after-conditioning has a great influence on the future history of the beer. It is for this reason that the custom of partial fining in the racking square is so often attended with disastrous results. A brewer is generally led to this truly pernicious custom through inability to see that in nine cases out of ten any excessive turbidity of a beer at the time of racking is not due to yeast in suspension but to the want of a proper "break" or flocculation of the finely divided amorphous matter which comes out of solution in the worts during the cooling process, and is amenable to treatment in other ways.

This subject has been dealt with in Part III of my paper on the "Nitrogen Question in Brewing" (this Journal, 1913, **19**, 84). Indirectly it has a bearing on the question of wild-yeast growths, since it is there shown that when this amorphous matter remains in the beer in a very finely divided and unflocculated condition it exerts a very distinct poisoning effect on the primary yeast, which is thus rendered incapable of exerting its full power of inhibition.

The reason why the reproduction of the secondary yeast cells is so much retarded by the presence of the primary cells is to be found in the first place in the great numerical preponderance of the latter, which gives them a better chance in the struggle for existence and competition for some element or elements essential for the maintenance of their vital activities and reproductive power. What is that essential element? We turn in the first instance to the nature of the carbohydrate left in a beer when the principal fermentation is completed, guided thereto by the knowledge that, whilst the primary yeast demands a crystallisable sugar like maltose or dextrose to keep it going, the secondary yeasts are capable of hydrolysing and utilising malto-dextrins and dextrins which the primary yeast cannot touch unaided by some extraneous diastatic ferment. Observations and experiment, on the other hand, go to prove that the true explanation is not to be found here, but resides in the competition for *oxygen*, which is essential to the reproduction of all yeasts. I have already referred to this in describing the difference one observes in forcing a beer in flask and in cask respectively, but the argument will bear repeating in view of its application to this new case.

Yeast cells, and especially those of the primary *Saccharomyces cerevisiæ*, possess an extraordinary affinity for oxygen, so much so, in fact, that quite a small amount of yeast suspended in a liquid will in a very short time completely absorb the whole of the oxygen which is dissolved in it. If the liquid is again saturated with oxygen at its ordinary partial pressure in the atmosphere, the same yeast will continue this process of absorption, and such an experiment may be repeated many times in succession with the same result.

I have given the experimental proof of this in a previous paper (this Journal, 1909, **15**, 264). Some of this absorbed oxygen appears to be used up in the respiratory processes of the cell, but a certain portion is in some way or other stored up in the cell, so as to impress upon it a power of reproduction by budding even after the whole of the free oxygen has disappeared from the surrounding medium.

It is the competition for this dissolved oxygen, so necessary for cell reproduction, to which we must ascribe the influence of the primary yeast in inhibiting wholly or in part the growth and increase of the secondary yeast.

Under the ordinary conditions of practice, unless the infection with secondary yeasts is altogether abnormal, a newly racked beer fails to show under the microscope anything but primary yeast, which, if present in the amount it should be, rapidly fixes the free oxygen which has been introduced during the racking, etc. The traces of secondary yeasts due to the infection are thus placed under anaërobic conditions, and are consequently unable to reproduce themselves. This relative state of affairs will continue to exist even if there is further access of oxygen from without, provided a sufficient number of the primary cells remain in full activity. Should these, however, from any cause lose their vitality, they can no longer act protectively when further access of oxygen occurs, either through slow permeation of air through the wood of the cask, or by rolling the cask over, or from any other cause which directly or indirectly brings traces of air in contact with the beer.

If these foregoing remarks will only induce the scientific brewer to regard the problem of wild-yeast "frets" from this new point of view, he will be in a much better position to understand how it is that very small sources of infection with secondary yeasts may, under certain conditions, give rise to an altogether disproportionate amount of difficulty, and that his efforts to get too bright a beer at racking, especially if he is using an agglutinative* or "clumping" strain of yeast, may be increasing the very risks which he is attempting to avoid.

With this added knowledge we may now again turn to the subject of pure yeast cultures in Hansen's sense, and continue our enquiries as to how far its introduction into an English brewery is likely to be efficient in diminishing wild-yeast troubles. That it can have but little effect in cases where extreme precautions are not taken to eliminate all possible infection from the plant is self-evident, but in what follows we will assume that these conditions have been fulfilled.

The advocates of pure levures have always assumed that an ordinary English brewery yeast must necessarily consist of a mixture of races, but, even granting this assumption, it by no means follows

* These agglutinative strains of yeast generally give very bright racking samples if the amorphous matter has been well flocculated.

that these races must include any appreciable amount of the particular secondary forms which produce the after-effects we have been considering. These secondary forms differ so much in general appearance from the true primary *Saccharomyces cerevisiæ* that if they are present even in very small relative amount they can be detected microscopically, a fact which anyone may verify for himself by mixing a pure race of primary yeast with definite and varying amounts of a culture of the secondary forms. Moreover, the rate of subsidence of the secondary forms, partly owing to less density and partly to the smaller surface they expose, is so much slower in the case of the secondary yeasts, that a process of fractional centrifuging can cause a partial separation which renders their detection much more easy. Now, according to my experience, one may search an ordinary sample of brewery yeast for a very long time without finding any evidence of the presence of secondary yeasts, even when the aid of the centrifuge is called in. But this is no certain proof of their entire absence, and such methods could not detect their spores if present.

In order to differentiate the wild-yeast cells from those of the beneficent *Saccharomyces cerevisiæ* in a lager yeast, Hansen proposed a method which is based on the fact that each species of yeast requires a different time and optimal temperature to produce its ascospores when grown on a solid medium.

Chr. Holm and S. V. Poulsen, working in the Carlsberg Laboratory, tested the limits of Hansen's method, and came to the conclusion it could detect an infection of from 0·5 to 1·0 per cent. of wild yeast in a brewery yeast. This in itself is certainly not a promising result, for such a proportion of wild yeasts is excessive, and any method must be susceptible of much greater refinement than this. Admitting, however, everything which has been claimed for this analytical method as applied to bottom yeasts, no success has, as far as I know, attended its application to English top-fermentation yeasts, nor, according to my own investigations, does it give any promise of practical application. On the other hand, I have found it quite possible to devise a simple laboratory method for testing the presence or absence of wild yeasts in an ordinary brewery yeast, and even of forming some idea of the relative amount of infection when it does occur. It consists in fermenting a sterile wort with a portion of the given yeast under conditions which exclude all possible infection of the wort from any other source than that afforded by the seed yeast, and in treating the resulting beer in such a manner as to ensure the subsequent development of any latent wild-yeast cells derived from the original seeding.

In fixing the best conditions for the final treatment of the beer, and for ascertaining the limits of wild-yeast infection which are thus detectable, I have made many series of experiments, using pure cultures of *Saccharomyces cerevisiæ* (top-fermentation) alone, or mixed with definite amounts of a pure culture of one of the commoner wild-yeast species which occurs in English "fretty" beers, and is morphologically very distinct from the ordinary primary yeast. The resulting beers from the laboratory fermentations were incubated at a temperature of from 80° F., some with and some without aëration. In this way a technique was soon developed which allowed the detection with ease of an infection of the original yeast of 1 in 5,000.

Having once established the practicability of this method, I proceeded to examine the ordinary yeasts from a large number of breweries in different parts of the country, and run on different systems, and found that *only in a very few instances could any distinct traces of infection with wild yeasts be discovered.* It is quite possible, nay almost certain, that some of these yeasts were mixed races, but it is equally certain that they were primary yeasts, unmixed with the troublesome forms which characterise secondary "frets" and yeast-turbidity. Now it is perfectly evident that the incidence of any troubles of this kind occurring in the breweries in question would not have been in the least alleviated by the introduction of a pure yeast culture; the infection where it did occur must have been derived from sources independent of the pitching yeast. It is to unsterilised plant and air-borne organisms that one must look, in the vast majority of such cases, for the initial cause of defect.

When we consider the extraordinary freedom of brewery yeasts, as a rule, from the secondary forms, even where we know the breweries are by no means beyond suspicion as regards extraneous infection, we are led to think that in the primary fermentation, up to the period of yeast separation, some process of elimination is going on which results in the complete crowding-out of the deleterious forms. In order to throw some light on this point I have taken definite mixtures in known proportions of pure cultures of primary yeast and of a common secondary form, and have passed these through a successive series of fermentations in the laboratory, determining at each stage the relative proportions of the two kinds of yeast by the usual method of cell-counting in the hæmocytometer.

Commencing with a mixture of cells in the proportion of 1 of secondary yeast to 10 of primary, this proportion at the end of the fourth fermentation became 1 of secondary to 40 of primary. It was not found possible in the laboratory to eliminate completely the secondary yeast in this way, but since the processes of yeast separation on the laboratory scale take place by deposition, and therefore differ so much from the processes of separation in practice, it might be expected that on the manufacturing scale a much more perfect elimination would take place, as in fact we know it does.

An intelligent brewer who closely studies the behaviour and idiosyncracies of his yeast in the brewery learns to recognise small differences in its way of working which are quite inappreciable in the laboratory; such for instance as those connected with the time taken to show the first signs of fermentation, its progressive rate of producing the attenuation, the need or otherwise of rousing in the tun, the particular stage of the fermentation at which it commences to separate, the mode of attemperation which it demands, and many other small differences. Now it cannot be too frequently borne in mind that such properties which characterise a brewer's yeast are not those of an individual, but represent the statistical average properties of an enormous aggregate of individual cells all slightly differing from one another as do the leaves in a forest, and each one capable of giving rise, if separately cultivated, to another aggregate of which the average properties will certainly differ somewhat from those of the previous one.

If, therefore, a brewer, having found a strain of yeast which suits his requirements, and to whose peculiarities of treatment he has become accustomed, should for any reason require a change, say through accidental contamination of the yeast with bacterial organisms, it is pretty certain he will not be able to reproduce all the combination of qualities of the original stock, by a *renewed single-cell culture*, even if that stock is a pure race. He will be sure to find some differences of habit which will require renewed study and to some extent break the continuity of his operations until they are understood and allowed for. Even if he has kept a pure stock going in a Hansen cultivator for any length of time he cannot be sure of absolute uniformity, since in this case the conditions of cultivation through successive generations are so different from what they are in the brewery. These, I may say, are by no means mere academic objections but are founded on personal experience.

One of the most frequent reasons for a change of yeast is to be found in its accidental contamination with a very common bacillary organism, the *Saccharobacillus Pastorianus*, which was well known to Pasteur, and was described by him as the ferment of *bière tournée*. Derived originally from the plant, or in some cases from the surrounding air, it tends to accumulate in the yeast and is passed on from brewing to brewing by this means. In its early stages of growth in the beer it produces a "silky" turbidity, and at a later stage lactic acid. If the infection of the yeast is not very great this organism may escape notice when the yeast is examined in the ordinary way under the microscope, but I find that extremely minute infection may be readily detected by mixing a sample with water and submitting it to four or five fractional or partial separations in the centrifuge, and spinning the last and only slightly turbid fraction for some time. Such a final fraction will contain the *Saccharobacillus* mixed with any harmless short bacteria which may be present, and from which it can be readily differentiated by its appearance.* If this fractionation is carried on under standard conditions a very good idea can be obtained of the relative degree of infection of any two samples of yeast. A good pitching yeast ought not to show the least trace of *Saccharobacillus* when submitted to the process. Assuming, however, that distinct signs of infection are present, have we any means of purifying such a yeast short of single-cell culture, using some process which will not sensibly interfere with the general average qualities of the yeast in other respects?

Of late years I have given a considerable amount of attention to this question, which is one of great practical importance, since of all the bacterial diseases to which beer is liable there is not one which is more responsible for irregularities than that produced by the *Saccharobacillus Pastorianus*, and species allied to it.

* The process of fractional separation by the centrifuge can be applied in many useful ways in the examination of beers.

The practical solution at which I arrived was based in the first instance on some observations of Pasteur, made as far back as 1876, showing that a commercial yeast may be purified from certain contained bacterial organisms by cultivating it in a solution of cane-sugar rendered slightly acid, and on some later experiments of Van Laer indicating that an acid medium is particularly inimical to the growth and reproduction of the *Saccharobacillus Pastorianus.*

It is somewhat remarkable that these well established facts should not have found earlier an application to brewing practice, but the explanation is to be found in the strong opposition and commanding authority of Hansen, who was so engrossed with the idea of single-cell culture that he could never contemplate with equanimity the possibility of anything short of his method having any practical value in yeast purification. Hansen's principal objection was based on an assertion that an acid medium actually favours the growth of wild-yeast forms, and that although some bacterial organisms may be thus wholly or partially eliminated, great risks are run of the secondary yeast forms being actually increased by cultivation in the presence of free acid.

This was naturally a very alarming statement, and coming with the authority of Hansen it appears to have been very generally accepted, and no one, as far as I know, was led to enquire further into the question.

I must own that I am no respecter of authority myself, and never care to build on foundations constructed by others without due proof that they are well and truly laid.

In the first place I satisfied myself that it is extremely easy, by one or two cultivations of a yeast in a malt-wort acidified with 0·1 per cent. of tartaric acid, to eliminate all *visible* signs of any *Saccharobacillus* with which it may be infected. Moreover such a yeast after two or three successive acid cultivations can then be grown in an ordinary non-acidified malt-wort without any subsequent appearance of the *Saccharobacillus* in the resulting beer placed under forcing conditions.

The next step was to put to the test Hansen's statement with regard to an acid medium encouraging wild-yeast growths.

Varying and known mixtures of primary yeast and wild yeasts, such as occur in English beers, were cultivated in parallel series, one set being in ordinary wort and the other in wort acidified with 0·1 per cent. of tartaric acid. When cell-counts of the two kinds of yeast were made on the completion of the fermentations, no evidence could be obtained in any case that our commoner species of wild yeasts are differentially favoured by such additions of acid. These results, and the fact that it had been previously shown that English brewery yeasts seldom contain any appreciable amount of wild yeasts, gave me every confidence in applying this system of yeast purification in practice.

The only additional plant required consists of three small fermenting vessels, A, B, and C, of a capacity say, 2, 6, and 15 barrels respectively. The actual sizes will necessarily vary with the special requirements of the brewery, but in any case C should be large enough to give an outcrop of yeast sufficient for the smallest unit amongst the ordinary fermenting vessels of the brewery. A and B at any rate should be made of aluminium, and all three vessels should be made and arranged with a view to excluding the possibility of infection of any kind.

Sterile hopped wort in A, to which has been added 0·1 per cent. of tartaric acid (*i.e.*, 5¾ oz. per barrel) is fermented with the yeast to be purified, and from the resulting outcrop in due time similarly acidified wort in B is pitched; and from this again a further cultivation is made in C in ordinary wort without the addition of acid.

By these simple means the brewery yeast can be entirely freed from the *Saccharobacillus*,* leaving the yeast with all its initial qualities and peculiarities intact.†

During the summer months it is desirable to carry out the process frequently, and at any time when the fractional centrifuging shows any trace of the *Saccharobacillus*.

It is now about eight years since I first introduced this process into various breweries, and wherever it has been applied it has had a very marked effect in reducing the difficulties of summer brewing, in increasing the general stability of the beers, and in rendering changes of yeast unnecessary. Its use is extending rapidly, and I believe many brewers are availing themselves of it who have no knowledge of its origin.

* The acid treatment does not eliminate certain short, thick bacteria which are sometimes present in the yeast. As far as my experience goes these are comparatively harmless.

† It is generally found that the fermentative activity of a yeast treated by this acid process is somewhat increased the first time it is carried over into the brewery, but this very soon rights itself.

On the Use of Pure Air in Breweries.

It is well known that a sterilised beer-wort exposed for a short time to ordinary air, and then incubated, will develop organisms which cause fermentation or decomposition of the liquid, whereas if the admitted air has been filtered through cotton-wool, or some such filtering agent, or has been previously heated by passing through a red-hot metal tube, no such organisms will appear.

Some rough idea of the relative amount of the adult forms or spores of yeasts, moulds, and bacterial organisms present in the air of different localities, and capable of thriving in a hopped malt-wort, may be obtained by exposing, under similar conditions and for equal times, Petri dishes charged with wort-gelatin, and then counting and microscopically examining the various separate colonies which appear on incubation.

If such a survey of a brewery is made it will be found that deleterious organisms are always more or less present in the air, but are particularly abundant in those parts which are accessible to the extremely fine particles of dust proceeding from the grinding or screening of malt or barley, or from chaff-cutting and such like processes. These particles may be disseminated by feeble currents of air for long distances in a perfectly invisible form, and constitute a real danger in a brewery, so much so, in fact, that the most rigid precautions ought to be taken to isolate completely the milling and screening rooms.* These dangers are of course intensified where the old plan of exposing the worts on the coolers is still adhered to.

Provided the grosser sources of infection, such as I have just referred to, are avoided, it is quite possible to get too exaggerated an idea of the dangers of air infection, just as Lister did in the early days of antiseptic surgery. It by no means follows that the meteoric showers of germs which fall on a malt-wort in the last stages of cooling on the coolers or refrigerators will produce an effect on the resulting beer in any way proportional to their intensity; in fact, were this the case, brewing practice could not have developed along the lines it has done.

* One often finds malt-houses in close and dangerous proximity to a brewery, distributing highly infectious dust from the kilns and screening rooms. No worse locality for a malt-house can be selected than this.

Vast numbers of these air-borne germs never have a chance of developing in the finished beer, although their presence in the original worts can be so easily demonstrated. This is due to their suppression and crowding out by the predominant pitching yeast before they have had time to establish themselves, and also to the changing nature of the medium during fermentation, which renders it less suitable for the growth of some of these extraneous organisms. In this respect the increasing proportion of alcohol is an important factor.

An excellent illustration of this is afforded by the effects produced by a very minute bacterium which is found pretty generally in sterilised worts which have been exposed to ordinary air for a short time. If after such a brief exposure the wort is incubated and allowed to ferment "spontaneously" the liquid acquires a very peculiar and characteristic "honey-like" odour which is extremely unpleasant, and is associated with the minute bacterium referred to. If, however, such a wort is fermented in the brewery at a fair speed, with the addition of a normal amount of pitching yeast, it may develop no trace of this peculiar odour, the opportunities for the reproduction of the organism being absent. But if, on the other hand, the fermentation in the early stages is not sufficiently vigorous, either through an undue reduction in the amount of added yeast, or through checking the fermentation too early by attemperation, the characteristic odour to which I have referred can be detected in the gas evolved from the fermenting vessel and in the finished beer. I remember very well first coming across this odour and flavour many years ago in the "spontaneously" fermented high-fermentation beers of a brewery in Copenhagen. It is by no means uncommon in this country, and when it occurs can always be traced to air infection intensified by slow fermentations.

I recorded another well-established instance of trouble produced by an air-borne organism, in a paper read before this Institute in 1895 (see *Journ. Inst. Brew.*, 1895, **1**, 14). In this case it was produced by a *coccus* inducing "ropiness" in the beer, and derived from breeding places adjacent to the fermenting room.

Examples of aërial infection of worts with wild yeasts and bacteria are by no means rare, but there is sometimes difficulty in differentiating them from the much more common cases of infection from the plant, and a brewer is very apt to attribute difficulties to insidious air infection which have a totally different origin.

There is no doubt in my mind that the modern applications of pure filtered air in a brewery, especially in the refrigerating room, are a step in the right direction and are to be generally commended, but there is a constant danger of expecting too much of them, of regarding them as a panacea, and of applying them before all other needful precautions have been taken to close the door against the numerous chances of infection afforded by imperfectly sterilised surfaces of vessels, pipes, taps, etc.

I have on several occasions disappointed brewers who were desirous of installing a pure air plant by assuring them that such outlay could not be justified until manifest dangers in other directions were removed.

To sum up my experience in this direction, the introduction into certain parts of a brewery of air which has been previously submitted to effective filtration may be a distinct advantage, but the process must be regarded in its true light as a last finishing touch when all other sources of infection have been eliminated. It is, in fact, a coping stone and crown to the edifice, which should only be considered when the substructure is complete.

The Cold Storage, Filtration, and Carbonation of Ales.

One of the most important developments of brewing practice which has taken place within my recollection is the comparatively recent introduction of a process of chilling, filtering, and carbonating ales. It has already practically revolutionised the bottling trade of this country by supplanting to a large extent the time-honoured methods of maturing the beer by a secondary fermentation in bottle, and it bids fair to have an equally wide influence on the cask trade in the near future.

Those connoisseurs of bottled ale who still retain a preference for beer treated in the old way regard with disfavour these modern innovations, and a few well known firms will have nothing to do with them. It is true that, as regards bottled ale, there is a certain quality of flavour which can only be attained by maturing in bottle, but the perfect brilliancy, absence of sediment, and constant uniformity which are attainable by the new processes outweigh this objection, and, since they are particularly applicable to the lighter beers which are now in demand, the brewer must adapt himself, as he has done in the past, to this change of public taste. Besides this, the new process has by no means reached the finality of which it is capable.

Every brewer is conversant with the fact that when a well-fined top-fermentation beer, produced in the ordinary way, is lowered in temperature to somewhere between 45° and 50° F., it loses its brilliancy, and that in cold weather, and in a cellar where the temperature cannot be maintained at 52° F. or thereabouts, this "chill" may become very troublesome. Some twenty-five years or so ago, when I was studying the nature of the very finely divided matter which thus comes out of solution, and the temperatures at which it makes its first appearance, it occurred to me that, if we could take a matured beer in good gaseous condition, cool it down for a sufficient length of time for the maximal separation of the matter causing the turbidity, and then filter while still cold, we ought to obtain a bright beer, retaining its original gas, but no longer sensitive to further alternations of temperature, provided it was not cooled lower than the original point at which the first chilling had taken place.

Experiments on a small scale proved so promising that I attempted to extend them, but, owing to my inability to obtain suitable filters for working on a large scale, I had to adopt a compromise by fining the beer in a cold state and re-racking. At this time the continuity of the work was broken by my leaving Burton, and I was prevented from following up the idea. On my first visit to America in 1896, I learned with considerable interest that some of the top-fermentation ale brewers of the New England States had recently applied a principle of this kind for both bottled and draught light ales, guided thereto by competition with the lager beer brewers, the process being supplemented in some cases with carbonation of the ales after cooling to about 30° F. before filtration. On my return I gave an account of this process to our Institute (see *Journ. Inst. of Brewing*, 1897, **3**, 467), but it was some little time before the necessary plant was obtainable in this country, and the process had to pass through a lengthy probationary period before its merits were fully appreciated.

It was somewhat unfortunate that, when it was first introduced into this country, this new class of bottled ale should have had the term "non-deposit" attached to it, since it led the public to expect it to remain permanently bright and saleable for any length of time, an ideal condition which can only be fulfilled if the process has been supplemented with pasteurisation. Unless this additional precaution has been taken such a chilled and filtered beer will sooner or later throw a deposit of secondary yeast. The time required to give the first indications of this change may vary from a few days to several weeks, according to the amount of care taken in carrying out the process, and in fulfilling certain conditions which will be specified presently; with proper precautions there is no difficulty in prolonging the period of perfect brilliancy and freedom from deposit sufficiently to meet all practical requirements without pasteurisation.

From what has been said in an earlier part of this paper on the more rapid reproduction of secondary yeasts in a racked-bright beer when they are no longer under the inhibitory influence of primary yeast, it might be expected that a filtered beer of this class would be especially sensitive to any slight infection with secondary yeasts, a fact which is sufficiently patent to anyone who has had any experience of the chilling and filtering process.

Comparative freedom from such infection demands in the first place that the beer submitted to the process should not have developed any appreciable amount of wild-yeast forms, for no matter how perfect the filters may be they are sure to allow traces of these small secondary yeasts to pass through them. But a still greater difficulty is to ensure the absolute sterility of every part of the plant from the filters onwards, and no one who has not submitted the process to a rigorous bacteriological test at various points can appreciate the full significance of this difficulty, especially as the designers and makers of most of the special and complicated bottling plant have not yet been educated up to the point of seeing the importance of constructing it with an eye to ready cleaning and sterilisation.

Assuming that all these difficulties have been surmounted and that infection has been, I will not say entirely abolished, for that is well-nigh impossible, but reduced to a practical minimum, there is still a very important contributory cause to early sedimentation of the bright beer in bottle which has to be considered; that is the access of traces of atmospheric oxygen to the beer especially during the actual filling into the bottles on the bottling machine, for on the presence or absence of these traces of oxygen depends the possibility or otherwise of the subsequent reproduction of the few yeast cells of any kind which are present at this stage.

Some experiments which I have made bearing on this point may be mentioned here. It was found by measuring and analysing the mixture of air and CO_2 occupying the space between the beer and stopper of the bottle that there was a distinct relation between the amount of free oxygen present and the "life" of the beer, using that term to indicate the period of perfect brilliancy and freedom from deposit. Moreover, if the beer was kept under observation until the sedimentary yeast had attained its maximum development that point was found to coincide with the complete disappearance of free oxygen from the gases occupying the aforesaid free space. Further than this it was observed that taking a series of bottles filled in succession to a different extent, the "life" was always shorter the greater the ullage of the individual bottles.

The essentials in this process for deferring the sedimentation in bottle, that is to say for prolonging the "life" of the beer, may be summarised as follows* :—

(1) A beer which has been matured entirely under the influence of primary yeast.

(2) Good filters.

(3) Rigid cleanliness and sterilisation of all plant, connections, taps, etc.

(4) Chilling, filtering, and bottling under conditions which are as near anaërobic as possible.

(5) Perfectly clean bottles and stoppers.

The difficulties as regards fulfilling No. 4 are considerable at the present moment, for there is no bottling machine on the market which enables the beer to be delivered into the bottles under conditions which absolutely exclude air. Attempts are now being made to devise such an ideal machine, which in addition must also have the advantage of being readily cleaned and rendered sterile. Like many other things

* It is assumed that the turbidity or sedimentation is due only to the reproduction of wild yeasts, not to Saccharobacilli, cocci, or any other bacterial organisms which indicate radical unsoundness of the beer.

the practical solution of this problem has been interfered with and delayed by the war.

The future has much in store in the way of improvement in this process of chilling and filtering beers, and these improvements will be the quicker in coming by a careful study of the principles on which it is based. There is no branch of the operations in a brewery which is more promising for scientific investigation, which is more educative for the brewer, or which can give him a better idea of the importance to be attached to very small sources of infection, at first sight apparently negligible.

The whole process lends itself throughout to a perfect system of bacteriological control by the examination of samples taken at different points, either for plate culture, or observation in bottle under the standard conditions of a small forcing room kept summer and winter at a temperature of about 70° F.

The Guinness Research Laboratory.

Towards the close of the last century, owing to the strenuous endeavours of those leaders of thought who were justly dissatisfied with the educational system of our country, and the apathy of our people towards science, public attention was directed to the urgent need of original research in all branches of knowledge. By one of those extraordinary waves of public opinion, so difficult to explain, to which we are occasionally subject, the idea caught on, and it soon became fashionable for scientific research to be discussed in season and out of season, in the daily press, and in social circles by people who in most cases had not the least idea of its true aims, objects, or methods. It was a shibboleth which took the place of the technical education craze of a few years before, and it has been succeeded by babblings about radium, electrons, and a few other things since.

It is always unfortunate when a good idea is thus taken up by the populace in a frenzied manner, for apart from the waste of effort which accompanies it, the difficulties of those who have the requisite knowledge to guide the nation along safe lines only become intensified when the fever has abated and the inevitable reaction sets in.

Nevertheless a certain amount of good came out of this particular movement, and at one time it seemed probable that, amongst other things, the higher branches of brewing research would receive permanent benefit from it.

In the early part of 1901 I was requested by Messrs. Guinness, of Dublin, to draw up a scheme for their guidance in establishing a research department which would have for its object the investigation of outstanding problems in brewing which were waiting solution. In response to this request I prepared, in the first place, a detailed "Scheme of Work," systematically arranged, and embodying the main points which a long study of the question had led me to regard as the most important. Shortly afterwards I had the satisfaction of seeing the scheme put into practical effect, and the research laboratory inaugurated under my direction, with an excellent staff, and with every facility for a successful prosecution of the various investigations.

The nature and scope of the work carried out in this laboratory during the five years of its existence, and the bearing of it on the chemistry and physiology of the brewing and malting processes can be gauged by reference to the 350 pages of *Transactions* which were privately printed by Messrs. Guinness and generously made available to anyone specially interested in such matters. The work covered a pretty large field: of its value I must leave others to judge.

Unfortunately there was a reversal of policy in 1906 when the research laboratory, as such, ceased to exist, and the scheme, which at one time promised so much for the brewing industry, came to an end, partly through a mistaken idea that it had reached finality. Be that as it may, the results of any further work of the kind which may be going on are not shared with the outside world.

The Reaction of Scientific Investigations of the Fermentation Industries on Pure Science.

I have already dwelt somewhat fully on the great influence which Pasteur's early investigations on the manufacture of vinegar, wine, and beer have had on our fundamental conceptions of the proximate causes of fermentations and putrefactions, and how from these investigations has sprung the new science of bacteriology with its ever-increasing applications to medicine, surgery, and the various arts in which living organisms play a part.

Of all the phases of that constant action and reaction between pure science and its applications, this is one fraught with perhaps greater human interest than any other, and we can be justly proud that the industry with which we are connected should have been, as it were, the centre of disturbance from which this ever-widening train of ripples has proceeded.

But apart from the phenomena of fermentation, science has been enriched in many unexpected ways by attempts made in the first instance for the express purpose of obtaining rational explanations of other processes connected with brewing.

I have already alluded to the extension of our knowledge of the mechanism of enzyme action which has been brought about by the studies of starch and its hydrolytic transformations, and also to the manner in which a detailed study of the germination of a grain of barley has led to conclusions with regard to the particular mode by which the reserve materials of the seed are prepared and rendered available for the young plant in its earliest stages of development; conclusions which have considerable importance in the science of vegetable physiology.

Many other instances could be given of this kind of reaction between applied and pure science, but there is one which I desire to dwell upon somewhat in detail, because it serves to illustrate in an interesting manner how very far the explanation of a simple fact in brewing practice may carry anyone who is prepared to follow the clue wheresoever it may lead.

The fact to be explained was the homely one that the addition of a small amount of hops in the dry state to a finished beer has a distinct "freshening" influence; that is to say, that such a treatment tends to excite and continue the after-fermentation in the cask and to maintain the beer fully charged with carbonic acid.

No satisfactory explanation of this simple fact, known, I imagine, to brewers for many generations, had ever been forthcoming.

Ultimately, after a prolonged enquiry, in which every possible explanation was considered in turn, it became evident that the property in question is due to the hop strobiles containing a little *diastase*, which, in co-operation with the yeast remaining in the beer, induces a slow breaking down of the otherwise unfermentable dextrins and malto-dextrins left in the beer after the cessation of the primary fermentation, hydrolyses them, and reduces them to a condition in which they are fermentable.

The bracts which constitute the hop strobiles belong botanically to the foliar system of the plant, and when pondering over these results, I was led to enquire whether this occurrence of diastase is common to *all* leaves of plants, and, if so, whether it has not some physiological relation to the minute starch-granules which occur in the chloroplasts of leaves, and which, as shown by Sachs, are the first visible result of the assimilatory processes of the leaf-chloroplasts under the influence of sunlight.

Following up this idea, I was gradually led into a new path of physiological research which I had certainly not anticipated at the outset, and which took me into regions far remote from brewing.

The first part of it was carried out with the assistance of the late Dr. G. H. Morris, whilst I was still at Burton, and it resulted in tracing the relation of the starch and the sugars existing in the foliage leaves of certain plants from their first inception to their migration into other parts of the plant, and laid the foundation of new methods of research in vegetable physiology, which are at the present time being further elaborated by Messrs. W. A. Davis, A. J. Daish, and G. C. Sawyer, at the Rothamsted Experimental Station.

The first of my papers on the subject is contained in the *Journal of the Chemical Society* for 1893, under the title of "A Contribution to the Chemistry and Physiology of Foliage Leaves."

A few years later, working in the Jodrell Laboratory at Kew, with the assistance of Mr. F. Escombe, the study of the action of a green leaf was started at a point much further back in the train of natural processes which are involved in the assimilation of the carbon dioxide of the air.

Since it had been proved by F. F. Blackman that the only means of ingress into the leaf of the very dilute atmospheric CO_2 can be the minute perforations through the leaf epidermis, the so-called *stomates*, and that in most cases these fine pores, even when fully open, do not exceed 1 or 2 per cent. of the total area of the leaf surface, it became very difficult to understand how the assimilatory centres within the leaf could obtain from the air the requisite supply of CO_2 by processes of gaseous diffusion, especially as its dilution is represented by 3 in 10,000 of air.

The difficulties seemed further enhanced when it was found that in a fully assimilating leaf the atmospheric CO_2 was passing through the stomates at least 50 times as fast as it would have passed into a series of small openings of equal size with the stomates, provided these had been filled level with a strong solution of caustic alkali. It was clear that there was here something wanting in our knowledge of the laws of diffusion of gases under these special conditions, something which must be able to explain on physical grounds the *vis a tergo* which urges on the molecules of atmospheric CO_2 through these small openings. To find the answer to this question, it became necessary to go pretty deeply into the physical processes of diffusion and the mathematical expression of its laws, and to carry out a lengthy series of experiments on the diffusion of gases through small apertures, and more especially on diffusion through multi-perforate diaphragms analogous to foliage leaves with their stomatic openings.

The final result was an experimental proof that when, under certain well defined conditions, diffusion either of a gas or a substance in solution in a liquid is carried on through a circular aperture in a thin diaphragm, the rate of flow does not vary, as might be expected, according to the *area* of the aperture, but, owing to the converging lines of flow, according to its *diameter* or *radius*. Hence, as such an aperture is diminished in size, there is an acceleration of the flow through equal areas.

One curious result arising from this law is that it is quite possible to arrange a series of very small and equidistant apertures in a thin diaphragm of celluloid, or some such substance, in such a manner that, although the sum of the areas of the free apertures may only represent 2 or 3 per cent. of the entire area of the diaphragm, such a diaphragm, when inserted in the line of a diffusive flow of a gas or solute, exercises scarcely any retarding effect on the flow. This is the fundamental principle on which the green leaf is constructed, and by means of which it is able to drink in with sufficient rapidity the atmospheric CO_2. Speaking generally, it is found that the fine stomatal openings of a leaf are arranged at suitable intervals to produce the maximal effect in this direction.*

I have necessarily given here the briefest possible outline of some of these results, and cannot even refer to many other cognate problems connected with the economy of the leaf which presented themselves and

* See *Phil. Trans.*, 1900, B, 193, 223; also *Roy. Soc. Proc.*, 1905, B, 76, 29.

were investigated as the research spread wider and wider; problems, for instance, connected with the absorption and utilisation of solar energy by the leaf, and the proportion of this energy which is used up for the chemical processes of assimilation, or dissipated in the transpiratory process by re-radiation, and by the cooling effect of the surrounding air.

You will note that we have travelled very far from our starting point, which was a modest attempt to find an explanation of the effects of the dry-hopping of beer. This was one end of a chain which, followed up link by link, led without any break in its continuity into a region in which the building up of organic from inorganic material by the living plant is in full activity; the great synthetic process of Nature, in fact, on which the continuance of all life on this globe depends.

Incidentally, also, it led us through a comparatively unexplored province of physics, and to a recognition of the remarkable natural adaptation of the structure of a green leaf to certain physical laws affecting the free diffusion of gases.

Still another instance of how a simple observation connected with our industry may extend scientific knowledge in unexpected directions is afforded by some recent work of Adrian J. Brown on the green colouring matter which exists in the integuments of certain varieties of six-rowed barley, and which is rendered very apparent when these barleys are steeped in water preparatory to the malting process. This investigation led to the discovery that one of the coatings of the seed consists of a "semi-permeable membrane," that is to say a membrane which, whilst allowing water to pass freely in either direction, is impervious to certain substances in solution in the water, and especially such solutes as are produced during the metabolic processes which go on within the grain during germination. Such a differential "sieve" is necessarily of great importance physiologically, since it ensures the complete retention within the grain of the substances which are required for the nutrition of the young plant. But this is by no means the whole story, since a careful study of this semi-permeable membrane and its differential treatment of solutes of various kinds is extending our knowledge of the general phenomena of osmosis, and their connection with the surface tension of liquids. It has, in fact, led its author into one of the most difficult branches of physics, and has suggested new methods of attack, of which he is taking full advantage.

In this connection reference may also be made to the work which is being carried on by E. S. Beaven on the production of new varieties of barley by cross-fertilisation on Mendelian lines, with a view to the combination and fixation of certain qualities which make for the best results from the point of view of both farmer and brewer. Apart altogether from their practical importance, such experiments have a high scientific value in relation to a true comprehension of the laws of heredity.

This list might be extended further, but enough has been said to show that the benefits which brewing has of late years received from science represent only one side of the account; the return which has been made to science for this invested capital has already been very considerable, and it will increase as time goes on provided we have a succession of properly equipped observers ready to seize, appropriate, and develop the suggestions so frequently presented to the brewer by his daily practice.

If I were asked to embody in as few words as possible these two interacting and reciprocal forces, I should take for the first the motto of the Royal Agricultural Society—"Science with Practice"—and for the other "Omnis Scientia ex Cerevisiâ."

The Degree of Protection against Infection Conferred on Worts and Beers in Virtue of Difference in their Initial Chemical Constitution.

In the whole range of brewing practice there is no more difficult problem than that involved in the question of how far comparatively small differences in the chemical composition of a wort can favour or disfavour the attacks of deleterious organisms whilst that composition is maintained within the limits necessary for the growth and reproduction of the beneficial yeasts, and for the appearance of the desired qualities in the final product.

It must be understood that I am not referring to any extensive changes in the composition of the worts which would follow a wide departure from the ordinary traditions of brewing practice. Long experience has mapped out these main thoroughfares along which every brewer must travel under pain of failure. The differences to which I refer are minor ones such as are due to the varieties of barley employed, its "quality" as affected by seasonal and regional variations, the blending of the materials, variations in the malting and mashing processes within permissible limits, and so on.

If the brewer were always working under exactly constant conditions as regards the intensity of infection it would be comparatively easy to obtain a qualified answer to these questions, but this very factor of infection may perhaps be altering from week to week and from month to month, and, as a rule, he has no criterion of the extent of the variation. Hence, when he looks back on a year's average results, he is apt to attribute any shortcomings to some occult and hypothetical seasonal differences in his material, tending towards instability, whilst the real determining cause in all probability is some undiscovered centre of infection in his brewing plant, etc.

I must confess that my own ideas on this question of "predisposition" have up to the last few years somewhat fluctuated, and even now, although my views upon it are much more settled, so difficult are some of the points connected with it, when all the evidence is reviewed, that I am not prepared to assert dogmatically that it is altogether a fallacy, any more than is the cognate view that in a community of individuals, all *apparently* under the same chance of becoming infected with a given communicable disease, there are some who from indefinable "constitutional" reasons are comparatively immune. There is nothing unreasonable in the proposition, but owing to the very nature of the problem, which cannot be satisfactorily attacked in the laboratory, but demands experiential work on the large scale, it is extremely difficult to obtain evidence which excludes every alternative explanation.

It is long ago since I became quite convinced that if this constitutional "predisposition" of beer worts is a fact, its origin is to be sought not in their carbohydrate, but in their nitrogenous constituents, and it was considerations of this kind which induced me when the Guinness Laboratory was founded to make a leading feature of this outstanding problem, and to commence an investigation of the nitrogenous constituents of barley and malt, and more particularly those of a permanently soluble nature after boiling, which can alone have any influence in the direction mentioned.

In the Dublin Laboratory we made a considerable advance in the study of the various classes of these substances, and progress was made in devising means for their approximate differentiation and estimation. This work was afterwards pursued in my own laboratory and the results were laid before this Institute in a series of three papers on the "Nitrogen Question in Brewing," between the years 1907 and 1913, and since that time I have not ceased to give it attention, and to make free use of the system of "forcing" in cask which I have previously described, a system which in one respect reduces successive observations, as it were, to a common denominator, and enables one with the aid of laboratory methods to determine whether there is any suggestion that such small changes of constitution as are likely to occur in practice, are correlated with a greater or less tendency to favour the growth of undesirable organisms.

Absolute precision in individual cases is not attainable, but experiential methods of this kind, with due regard to all ascertainable facts, can reduce the probabilities of error if the observations are extended over a long period. Such observations, coupled with synthetic experiments in the laboratory, do most certainly indicate that, as far as regards maladies of a *bacterial* origin, and especially as regards the growth and development of the *Saccharobacillus*, there is some sort of relation between the total nitrogen-content of a wort and its liability to respond, more or less, to infections of this class, after allowance is made for the well known bactericidal influence of the hops.

Worts whose total solids contain a high proportion of nitrogen appear to be more sensitive to a given amount of infection than those with a relatively low proportion, but the effective resistance is diminished as the intensity of the infection is increased, and at a certain point it may vanish entirely.

Since it has been shown that, other things being equal, the permanently soluble nitrogenous bodies of a wort vary directly with the amount of nitrogen initially contained in the original barley, we are led to the conclusion, one which is now pretty generally admitted on other grounds, that barleys of high nitrogen-content are to be avoided by the brewer. Moreover, since "forcing" conditions during germination on the floors tend to increase the total permanently soluble nitrogenous bodies in the malt by causing undue development of the plumule and abnormal accumulation of these bodies within it, a good reason for the avoidance of these conditions is forthcoming.

But if a case can be made out for the "predisposition" or sensitiveness of a wort to *bacterial* infections, we may ask whether anything similar exists as regards *wild-yeast* growths, which on the whole, under modern conditions of brewing, give rise to far more trouble than the bacterial maladies.

By employing the same methods of observation and supplementing them by direct infection of beers with known amounts of these wild yeasts, I have quite failed to obtain any evidence of constitutional differences predisposing to such growths. The evidence, in fact, is all the other way, pointing to the determining cause being in the first instance the degree of initial infection from the plant or from infective dust. Ordinary variations in the compositions of the medium do not appear to favour or disfavour the growth of these extraneous yeasts when once seeded, but their reproduction depends, as I have shown in a previous section, on the access of traces of free oxygen and the relative competition for this oxygen which goes on between them and the primary yeast.

Fifteen years ago, and even up to the last five or six years, I felt pretty hopeful that a closer study of the assimilable portions of the nitrogenous constituents of a beer wort would be helpful in finding a proximate solution of these bacterial and wild-yeast maladies. If I have been to some extent disappointed in the result I certainly cannot regret the time and trouble bestowed on this elusive nitrogen question. A negative answer, if sufficiently emphatic, may be as valuable as a positive one; it at any rate clears the ground and saves further effort in the wrong direction, both for oneself and others. Moreover, the quest has taught me much, and has led me into roads, by paths, and pleasant lanes in a comparatively unknown country.

The one great lesson it has taught is that it is possible to over-estimate the influences of small differences in the composition of the medium with which the brewer has to work, and that the micro-organisms which he wishes to encourage or avoid are, after all, not so selective in their choice of nutrients, and do not exhibit such distinct preferences, as we have sometimes imagined. The result does not depend so much on small differences in the nature of the "soil" as upon the inherent qualities of the seed sown, and a choice of methods of cultivation which will give the wheat a better chance to develop than the tares.

Thus we are led back to Pasteur and the pre-eminent importance of bacteriology for the brewer, assuming, of course, that he has adhered to the ordinary lines of safety in the selection and manipulation of his materials.

A certain positive result has followed from these particular investigations, which is of considerable importance, and in itself worth all the time devoted to them. They have thrown much light on the nature of the nitrogen-containing substance which is precipitated during the cooling of hopped worts, and the indirect influence which this may have on the subsequent development of the yeast. They have also shown how important it is to select cooling methods which bring about the effective "flocculation" of the worts and the best conditions for producing this.

The Education of the Operative Brewer.

Important and essential as is the cultivation of that instinctive "touch" to which I have already referred, a quality which is fostered and developed only by practical experience, the brewer of to-day is in no better position than his ancestors if he neglects to equip himself with the resources of modern science which bear directly on his work.

When the new ideas were shaping themselves in the minds of brewers some 35 or 40 years ago, if I had been asked to forecast the position in 1916, I should certainly have ventured to predict that by that time every brewer in the United Kingdom would have been educated up to the point of understanding and applying the scientific principles of his business. How comes it therefore that, although development has in some cases taken place along the right lines, the majority of our young operative brewers are still content to go on either in total ignorance of principles, as their fathers and grandfathers did, or but with a mere smattering of so-called scientific knowledge, picked up anyhow, having no real educational value, and often worse than useless.

Lack of opportunity can no longer be pleaded, as it might have been in my young days, for we have everywhere excellent institutions which are giving systematic instruction in all those branches of science which are the most needful for the brewer, and where he can obtain a thorough training in first principles, to be followed later by special courses of study such as are given in the Brewing School attached to the University of Birmingham, and elsewhere.

When we look into this question more fully, we recognise that the real root of the trouble after all does not lie with the operative brewer, but with those who have the responsibility of appointing him. It is they who really fix the standard of his mental equipment and training, and from them little or no encouragement is forthcoming to second any efforts to raise that standard. We have here, in fact, a special case of a general and widespread evil which affects all our manufacturing industries in this country, one to which attention has been drawn for years past by arguments which have hitherto fallen on deaf ears, but which in the present national crisis have some better promise of listeners.

No one is more alive than myself to the advantages conferred on the individual and the community by a thorough classical, literary, and mathematical education, and I have no sympathy with those extremists of the present day who, with a zeal more fervid than rational and discreet, are agitating for a root and branch policy for the extirpation of the classics from our schools, a policy which, if successful, would produce as much lopsidedness of mind and narrowness of outlook as the older system, perhaps even more. Nay, I would go even further than this, by saying that, if the exclusive choice lay between the *literæ humaniores* plus mathematics on the one side, and natural science on the other, I would unhesitatingly vote for the retention of the former, a choice which is based on a life-long observation of the human product of the two systems.

But, happily, there is no truth in the existence of such an antagonism. The popular belief to the contrary had its origin in the first place in the mistaken conservatism of the votaries of the old system, who, believing that they had reached finality in education, refused to admit the need of any adaptation to modern requirements and changing ideals; secondly, to the frequent over-statement of their case by those who, rightly advocating the claims of science, would remit the humanities to a very subordinate position. Owing to this clash of arms, and hand-to-hand fighting, which appears to be never-ending amongst educationalists, and owing also in great measure to the attitude of our older Universities, we have adopted in many of our schools a compromise, which I think has had very unfortunate results, since it has not only favoured the idea that the humanities and natural knowledge cannot be cultivated together, but, owing to the prestige still attached to the older form of learning, it has helped to keep alive the notion that the pursuit of science is an inferior branch of study, suitable only for youths of less intellectual capacity. I refer to dividing a school into the sheep and the goats, represented respectively by a classical and a modern side, separated by a well-marked division, permeable only to mathematics, the common property of both, but effectually preventing any further mingling of the older and newer cultures.

These are some of the reasons why such a large majority of the men turned out by our schools and Universities, men who may be destined to become our rulers, administrators, and leaders of industry, have so little knowledge of or sympathy with science and scientific method. It is only with the last-mentioned class that I am dealing at present. Wherever one looks and enquires the same story is told, that those who are really the responsible heads of manufacturing businesses seldom have any desire to know anything of the inner meaning of their processes or of the scientific principles which underlie them, and in many cases they give no encouragement to their immediate subordinates to prosecute any original enquiries, or even to apply well-known principles to their daily work. In the brewing industry the conversion of proprietary businesses into Limited Companies has, within my own knowledge, had an influence the reverse of beneficial in this respect, and I believe the rule applies to many other manufacturing concerns.

Boards of directors seldom include any member or members sufficiently versed in the higher technical requirements of the business, and with a sufficiently broad outlook to regard anything as being useful which does not make for *immediate* dividend-earning capacity. The shadow of the ordinary shareholder is always upon them. Improvements in processes, and especially in methods of scientific control requiring well-trained and well-paid men to carry them out systematically are neglected, and in the selection of the operative staff sufficient weight is not attached to efficient training nor, as a rule, are the inducements offered in the way of emoluments sufficient to encourage men of the right stamp to prepare themselves for such a career by a lengthy and expensive course of training.

The scheme which this Institute has now under consideration will, it is hoped, tend to raise both the standard of education and the status of the operative brewer, and have some effect ultimately in remedying this unfortunate state of affairs. But if the effort is to be really effective, we must find at the same time some means of approaching a much more difficult task, the enlightenment of the average brewery director, who cannot, like his brewer, be captured young.

If boards of directors cannot somehow be made to realise the importance of selecting the younger members of their staff from amongst applicants who have been properly equipped, I fear that neither this nor any other scheme can prove successful. In such matters we have much to learn from our enemies.

Conclusion.

I suppose that to any man who has been engaged for more than half a century in some study or investigation which has demanded the best of which he is capable, there must be moments of regret that just at the time when his experience has matured and he is getting a clear mental insight into things which hitherto have been seen darkly, the Fates with their "abhorréd shears" are at hand to cut the thread of life. Regrets such as these are tempered by the knowledge that there is always a younger generation to whom the lamp of knowledge can be handed with the certainty that it will keep the sacred flame alight and hand it on in turn; but as years pass the full sense of the proverb, "Si jeunesse savait, si vieillesse pouvait," is brought more and more home to one.

No one has given better expression to this feeling than Ruskin. When I first read the following passage* I was a younger man and I did not feel the truth of it; a few months ago I came across it again and appreciated its forcible wisdom. He says:—

"Among the many discomforts of advancing age, which no one understands till he feels them, there is one which I have seldom heard complained of, and which therefore I find unexpectedly disagreeable. I knew by report that when I grew old I should most probably wish to be young again, and very certainly be ashamed of much that I had done, or omitted, in the active years of life. I was prepared for sorrow in the loss of friends by death; and for pain in the loss of myself, by weakness or sickness. These and many other minor calamities I have been long accustomed to anticipate; and therefore to read in preparation for them the confessions of the weak and the consolations of the

* See Ruskin's *Complete Works*, 24, Chap. XI, 371.

wise. But as the time of rest or of departure approaches me, not only do the many evils I had heard of, and prepared for, present themselves in more grievous shapes than I had expected; but one which I had scarcely ever heard of, torments me increasingly every hour. I had understood it to be in the order of things that the aged should lament their vanishing life as an instrument they had never used, now to be taken away from them: but not as an instrument, only then perfectly tempered and sharpened, and snatched out of their hands at the instant they could have done some real service with it. Whereas my own feeling now is that everything which has hitherto happened to me, or been done by me, whether well or ill, has been fitting me to take greater fortune more prudently, and do better work more thoroughly. And just when I seem to be coming out of school—very sorry to have been such a foolish boy, yet having taken a prize or two, and expecting to enter now upon some more serious business than cricket—I am dismissed by the Master I hoped to serve, with a 'That's all I want you for, Sir.'"

The CHAIRMAN: Before calling on one of our honorary members to propose a vote of thanks, I should like to say that I have received a telegram from the President of the Society of Chemical Industry, Dr. Carpenter, regretting his inability to be present to-night. I will now ask Professor H. E. Armstrong to propose a vote of thanks to Dr. Brown.

Professor ARMSTRONG said:—Of all forms of literature, autobiography is perhaps the most engaging and valuable, if the writer have a story to unfold of real work accomplished and the power of telling it in apt and lucid terms. Dr. Horace Brown's reminiscences belong to this category. The Institute of Brewing has reason to be proud that it has such a man active within its ranks whom it can honour, and is greatly to be congratulated also upon being the recipient of confidences so illuminating as are his—confidences which are the outcome of an ever busy life spent in the practice of an industry which sentimentalists are apt to condemn after their manner without considering the ways of mankind and how little the wants of a virile majority are compatible with their own.

Naturalist by inheritance and also a man of artistic gifts, born and reared in the reek of beer, early brought into touch with science by association with the Dr. H. Böttinger of whom he speaks in such appreciative terms and with the incomparable chemist, Peter Griess, who, like poor Yorick, was also a man of infinite jest, Horace Brown has been in every way fitted to undertake the scientific study of the complex problems which the brewing industry offers. An original inquirer to the manner born, the methods he has used are the reflection of his own personality, of exceptional finish, penetrant and always philosophical. He is my oldest scientific friend and we have always had many interests in common. We met first in 1865, in Hofmann's laboratory, at the College of Chemistry in Oxford Street. In the early years of which he speaks, I frequently had the privilege of visiting him in his home, thereby laying the foundation of my own interest in the fermentation industry. The society which he pictures was a remarkable one: Burton-on-Trent, in the seventies, probably was the most active and stimulating scientific centre in the country, the home of a real biochemistry; vital problems were always under discussion. It is sad that the scientific life is gone out of the town if not of the industry and that a group of men so remarkable as Peter Griess, Horace Brown, Cornelius O'Sullivan and Adrian Brown have no lineal descendants; the lapse is one that is not to the credit of the industry and should be inquired into so that it may be repaired.

Not the least interesting section of Dr. Brown's address is that in which he contrasts the labours of Lister with those of Pasteur and draws attention to the influence of Pasteur's studies of wine and beer on the work of the surgeon and on the development of bacteriology and preventive medicine. No claim can be justified more completely than the one he makes, that men such as these, in solving problems connected with brewing, have extended the boundaries of natural knowledge beyond all expectation.

He refers to Pasteur's methods as in sharp contradiction with those of Lister, but perhaps does not make sufficiently clear that these two men represent two entirely different and complementary habits of mind—the physical and naturalistic. Pasteur was trained in the methods of an exact science and was eminently logical; a golden thread of logical continuity runs through the whole of his work; he also possessed astounding intuitive powers; it is wonderful how everything he attempted was achieved. Later workers in the same field have often been foiled; none has shown his practical power of at once divining the nature of the problem that was before him and forthwith advancing to its solution.

Lister, on the other hand, had not merely passed through a less rigid and extended course of scientific training but naturally viewed matters far more from the broader and laxer point of view of the biologist; by his study of the processes of inflammation, his mind had been prepared for the reception of ideas such as Pasteur put forward—he was already far better prepared than anyone else to appreciate their practical value—a flash of intuitive genius was all that was necessary to effect the transfer that was required from the processes of fermentation and to establish the microbic nature of inflammatory processes. Dr. Brown himself gives an illustration of a like influence upon his own work produced on reading Pasteur's studies on wine. My object, however, in referring thus at length to these two men and the contrast they afford, is rather to call attention to the remarkable conjunction in Dr. Brown of the attributes of the exact worker in physical science with those of the biologist. He has always shown an instinctive sympathy with vital phenomena, a penetrative appreciation of the physiological significance of chemical and physical phenomena such as is very rarely met with in the chemist, hence the sterility of so much of the so-called biochemical work of the day. The biochemist is too often a simulated hybrid, in no way the natural blend that alone can be effective. The real investigator, like the poet, like all true artists, is born, not made—it is worth noting that Horace Brown had only a year's systematic scientific training. In these days, when there is so much idle talk of research, we may well bear this fact in mind; if we do not, the efforts that are being made to promote such work will be wasted and end in discouragement.

The address contains references to so many different lines of inquiry that it is impossible to discuss it in detail, especially as so much is to be read between the lines. It teems with suggestions and many of the paragraphs need much extension to make their full meaning apparent. Far from the account being too long, it is to be regretted that, having entered upon such a task, Dr. Brown did not avail himself more fully of the opportunity. I should like to see the address referred back to him for expansion, so that the precious experience he has gained, the views he has formed, may not be lost to us; at least he must be made to promise that Part II shall be forthcoming at an early date. In a pregnant passage in an early paragraph he tells you that he hopes to give his matured opinion on some practical questions which are more or less *sub judice* because they depend for their solution on experience and extended observation rather than on laboratory experiments. It is just these results of experience and observation—of large scale experiment—which are of infinite value, and we need to have them put before us by men who can not only observe but also are able to reason and put their conclusions into clear language: the ordinary practical man rarely has this ability. I shall not be satisfied until we have from Dr. Brown "The thoughts of a yeast cell"—all his thoughts on the subject. I venture to think it is his duty to give us these in full—no one else has the same ability or so large an experience from which to draw; by his own confession, he is alive to the value of observations such as he has been able to make and to appreciate their bearing on vital problems generally.

Finally, let me point out, that in recognising and honouring Dr. Brown, this Institute has necessarily recognised that it is under an obligation to the public: if brewing be, as he says, an industry from which a very high rate of interest on the scientific knowledge sunk in it can be obtained, surely it is to your advantage that you should continue to sink money in it—the more if it can be shown as it undoubtedly can that no other industry can claim to have contributed so much to the public weal. I am one of those who, with Dr. Brown, believe that invaluable experience has been gained directly and indirectly through the scientific study of wine and beer; moreover, I regard both as indispensable articles of food—*pace* all sentimentalists—on account of their special dietetic value, though the use of spirits may be abandoned so far as I am concerned, as they have a very slight dietetic value in comparison with beer and wine. In holding this celebration to-day, the industry is in fact recognising the need of securing a succession of men like Dr. Brown and is tacitly admitting also that it has no right to maintain secrecy, such as, between the lines, he complains of in the case of Messrs. Guinness and Co. Do not forget that the body scientific will not be in a position to insist on the public importance of your labours, if such a policy be continued much longer.

Dr. Brown very opportunely expresses his regret that the Lister

Institute has no official connection with your industry. It is much to be desired that this omission will be remedied in the not distant future and that action will be taken to improve the status of that institution, which has failed hitherto to justify expectations, perhaps because it has not been sufficiently in touch with practice. But we have to remember that all such institutions are dependent for success upon the inspiration of genius and imagination and that these qualities are manifest but rarely—we are apt constantly to forget that the pursuit of knowledge succeeds only in competent hands, so few know how knowledge is won. You are recognising to-day the services of a man of real genius: the most effective proof you can give that you are in earnest will be to provide every opportunity that is possible for men of like ability to arise.

I beg to move that this meeting tender its warmest thanks to Dr. Horace Brown for his address, and venture to express the hope that the continuation I have asked for may be forthcoming at no distant date.

The CHAIRMAN: I will ask Sir James Dobbie, who is one of our latest honorary members, to second the vote of thanks.

Sir JAMES DOBBIE: Mr. Chairman and Gentlemen,—I am afraid that the interests which I represent here are not always considered as being coincident with your interests. But at least we have this in common, that we all recognise the debt which we owe to those men of science who have made brewing their study. Both from your point of view and from the fiscal point of view, they have conferred an immense benefit upon all who are concerned with the industry.

It has been a great pleasure to me to be here to-night, and to listen to the most interesting account which Dr. Brown has given us of the progress of scientific brewing during the last half century. The story of Pasteur's work in relation to brewing is always a fascinating one; but it acquires a new fascination when unfolded by a Master such as Dr. Brown, and when its significance is explained, as it has been to-night, by one who has himself watched all its developments in their application to the art of brewing.

I cordially join with Professor Armstrong in the expression of the hope that Dr. Brown will not only let us have in full the manuscript from which he has read his extracts to-night, but will also let us have the additional matter which Professor Armstrong, with the great advantage of having read the manuscript, tells us would so usefully supplement the paper in certain directions.

My own connection with the subject of brewing is of recent date. When I came to London, Dr. Brown was collaborating in an inquiry of great practical importance to us, and I think of considerable practical importance to you—I refer to the new gravity tables—and I think I may truly say that the smoothness with which the new tables were brought into operation, and the comparative ease with which the difficulties connected with them were got over, was largely due not only to the skill which Dr. Brown brought to bear upon the work, but to the eminently reasonable spirit with which he entered into the discussion of the problems involved with the fiscal authorities. I can assure him that the chiefs of the Department of Customs and Excise would join most cordially in the honour which you have done him to-night. When I say that, I am not expressing merely my own personal opinion, but I am giving the opinion which I have heard expressed by the high officials of the Department.

I beg to second the resolution which has been moved by Professor Armstrong.

The CHAIRMAN: Gentlemen,—Both the proposer and the seconder of this motion have coupled with it a request to Dr. Brown. Professor Armstrong, in fact, said that the paper should be referred back to him. That I am not going to do. But I do agree that Dr. Brown should supplement it at some future date, possibly embodying some further contributions to our knowledge which we all hope he may make. I think I must couple that with the motion.

I have very much pleasure in putting the motion that a hearty vote of thanks be accorded to Dr. Brown.

(The vote of thanks was carried with acclamation.)

Dr. HORACE T. BROWN, in reply, said:—Mr. Chairman and Gentlemen,—I am exceedingly grateful to you for the way in which this vote of thanks has been received; and it gives me especial pleasure to hear my old and valued friend, Professor Armstrong, refer to our long association, first as students, and afterwards in various capacities.

Dr. Armstrong referred to the possibility of my amplifying this paper at some future time. I am not quite sure that the members of the Institute would think it requires much extending when they have the whole of the text before them. In fact, I am afraid that some of them will think it is rather long already. The only way of extending it would be to put it into something like book form; which I do not think I am quite prepared to do at present.

I thank you, gentlemen, most sincerely.

MEETING OF THE LONDON SECTION HELD AT THE IMPERIAL HOTEL, RUSSELL SQUARE, W.C., ON MONDAY, APRIL 10TH, 1916.

Mr. ARTHUR R. LING in the Chair.

The CHAIRMAN said that they would all welcome their friend, Dr. Fernbach, who was about to read two papers.

The following papers were then read and discussed:—

On the Mechanism of Alcoholic Fermentation.

By A. FERNBACH, D. ès Sc., Professor at the Pasteur Institute, Paris.

SINCE we have learned that the transformation of sugar by fermentation into alcohol and carbon dioxide is due to the action of enzymes, the problem of the mechanism of alcoholic fermentation has been the subject of numerous investigations. Whilst some workers, among whom Professor A. Harden deserves to be mentioned in the first place, have endeavoured mainly to elucidate the conditions under which the enzymes act in splitting up a fermentable sugar, others, taking up from a somewhat different standpoint, have investigated the successive chemical compounds which are formed during this transformation, attempting to draw a continuous chain between the original sugar and the final products. Notwithstanding the immense amount of work that has been done, we have to admit that this complicated problem is far from being solved. The present paper only claims to add a slight contribution to the solution of this fascinating question.*

* I have been greatly helped in my experimental work by my assistant, Mr. M. Schoen, and it is only fair to say that the merits, if any, of these researches, are, to a great extent, due to his skilful activity.