DOES BAKING STERILISE BREAD?

BEING AN INQUIRY, ON BACTERIOLOGICAL AND OTHER GROUNDS, AS TO HOW FAR BAKING AFFECTS THE VITALITY OF ORGANISMS IN DOUGH.¹

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THE following paper is founded on a large series of experiments. Its main conclusion is that baking does not necessarily destroy the vitality of micro-organisms (or their spores) that may be contained in dough. Briefly stated, we have succeeded in obtaining cultures of thirteen different kinds of microbes from bread taken out of the centre of recently baked loaves. Our contention, then, is that baking does not necessarily sterilise a loaf. In order to grasp the full significance of that proposition it will be well to review the circumstances that have led us to undertake the present inquiry.

One of the joint authors of this paper, Dr. Waldo, medical officer of health to St. George's, Southwark, has for some time past been endeavouring to reform the insanitary conditions of the cellar bakeries in his district. There has been little serious attempt at denial of the charges of unwholesomeness brought against these underground workplaces; indeed, the case against the London bakers could hardly be better summed up than has been done by one of their most ardent champions in the following passage:² "The indictment is that half the bakeries in the metropolis are underground; that there is little or no ventilation.....; the air is vitiated from many sources, sewer gas may enter through the drain openings in the floor;..... and that sewage may, through a defective drain-pipe, force its way into the bakehouse. Add to these a closet opening into the room; the men smoking or chewing tobacco; rats, cockroaches, spiders, and other insects in incredible numbers, and general filthiness—and we have something like a fair picture of Dr. Waldo's typical underground bakehouse. This is revolting enough, but add a few realistic touches, showing the operatives kneading the dough while suffering from a form of eczema known as 'baker's itch,' the sweat from their arms mixing with the dough, and we have enough of the nauseous to satisfy a glutton." The question of the insanitary bakehouse, it should be clearly borne in mind, is raised on two distinct and separate grounds—namely (1) the health of the bread producer, and (2) the health of the bread consumer.

1. *The health of the bread producer.*—Into this part of the question we do not now propose to enter; the point has been fully dealt with elsewhere. Suffice it to say that journeymen bakers, as a class, are unhealthy and short-lived.

2. *The health of the bread consumer.*—Hitherto it has been assumed that, however filthy the surroundings of the bakery, however contaminated the flour or other ingredients of the dough, yet all was set right by the mere process of baking in the oven, which destroyed those living organisms now shown to lie at the root of most diseased conditions. In short, it has been a generally accepted belief that the loaf left the oven in a sterilised condition, incapable of conveying disease to the consumer. This view has been forcibly expressed by a correspondent in one of the bakers' journals. "An unhealthy bakehouse," he writes, "is bad because it is bad for the baker, and not because of any injury likely to result to the consumer of his produce. Dr. Waldo has painted a picture of a wretched cellar bakehouse, but he does nothing to show that bread, produced even under these extreme conditions, is capable of causing zymotic disease. The dough in an underground cellar may be as unhygienic as possible; it may be a very hotbed of bacilli; it may swarm with all the germs known (more or less imperfectly) to medical science, and yet the bread made therefrom would be no whit the less wholesome. The loaf has been purified by fire. Germs cannot resist the temperature of the baker's oven, and it is not too much to say that there is more danger of infection in

one single quart of diseased milk, or in one glass of unfiltered water, than there is in all the bread made by all the bakers of the metropolis from one year's end to another." The foregoing contention that baking sterilises the loaf is one of the chief arguments advanced by those who defend the present system of cellar bakeries. It is founded entirely on assumption. Our present investigation was started primarily to test the truth, or otherwise, of that assumption.

There is little need to point out that specific pathogenic organisms may gain an entrance into insanitary bakeries. Sewage contamination has been repeatedly shown to exist in those places. Several gross cases of the kind, for instance, have been proved before the London magistrates during the present year. Then, again, a certain number of bakers are phthysical. These men are closely confined to the bakehouses for ten, twelve, or even twenty hours at a stretch. It has been again and again abundantly proved that the dust of rooms occupied by phthysical patients contains tubercle bacilli. Lastly, it is obvious that germs in a bakehouse could easily be conveyed to the dough either by the air, by utensils, by the hands of workmen, or in other ways. From these data we think it may be fairly argued that some bakeries from which bread is issued to the public are likely to be infected with the specific pathogenic organisms, at any rate, of typhoid fever and of phthisis.

There are two ways in which it is conceivable that a loaf might convey specific organisms capable of causing injury to consumers—viz. (1) organisms that have lodged on the crust of the loaf from surface contamination in an infected bakery; and (2) organisms that have possibly escaped destruction in the interior of the loaf during the process of baking.

1. *Organisms that have lodged on the surface of the loaf after baking.*—When taken from the oven a batch of loaves is often allowed to lie about in the bakehouse for a considerable time. It is obvious that there is every chance of contamination of the crust by the lodgment of bacteria either from the air, from the trays or shelves on which the bread is laid, or from the hands of the workmen. The fact that the baked loaf is open to infection in a dirty bakehouse seems to be overlooked by those who insist so loudly that baking sterilises the loaf. Their assumption appears to be that the loaf, once sterilised, will remain so for an indefinite time. The fallacy of this assumption, on the other hand, is recognised by some few people, who resterilise the crust by putting their loaves in a hot oven for a short while before use. We propose to deal with the whole question of the surface contamination of the baked loaf in another paper.

2. *Organisms that have possibly escaped destruction in the interior of the loaf.*—This consideration brings us to the starting-point of our investigation. It will be impossible to do more in this place than give a short summary of the numerous experiments carried out under our directions by Mr. Walter Severn at his laboratory in Bentinck-street, London.

TEMPERATURE.

In considering the likelihood of the survival of organisms in the baked loaf the question of temperature is of first importance. From a number of experiments made on loaves baked in a small laboratory oven it appears that: 1. The average maximum temperature in the middle of an ordinary quartern loaf during baking varies from 73° to 86° C. (163·4° to 186·8° F.), and in small loaves from 86° to 95° C. (186·8° to 203° F.). 2. There is a steady increase of temperature in the centre of any loaf during baking; thus, in a quartern loaf during one hour it rises from 25° to 75° C., and in a half-quartern loaf from 25° to 88° C. during the same time. The increase of fifty odd degrees (Centigrade) in the centre of an ordinary loaf is attained somewhat after the following scale:—

In the first 8 or 9 minutes about 6 degrees are gained.									
"	"	"	next	10	"	"	6	"	"
"	"	"	"	10	"	"	7	"	"
"	"	"	"	10	"	"	9	"	"
"	"	"	"	10	"	"	12	"	"
"	"	"	"	10	"	"	9	"	"
"	"	"	"	10	"	"	from 5 to 7	"	"

So that for the first forty minutes the maximum temperature is probably not more than 48° or 50° C. The practical inference is that any organisms that might be present in the centre of a loaf would be exposed for a short time only during baking to a maximum temperature of 73° to 86° C. (163·4° to 186·8° F.) in a quartern, and of 86° to 95° C. (186·8° to 203° F.) in a half-quartern loaf.

It is known that most bacteria are not destroyed by an exposure to the temperatures above mentioned as the average for

¹ A paper read in the Public Health Section at the meeting of the British Medical Association at Bristol on Aug. 1st, 1894.
² The Truth about the London Bakeries, New Review, May, 1894, p. 607.

the centre of a loaf during baking. The death point of bacteria has been generally expressed by Koch and Wölfhügel in the following passage: "1. Sporeless bacteria are destroyed in one and a half hours by hot air at a temperature slightly exceeding 100° C. (212° F.). 2. Spores of fungi require one and a half hours at 110° to 115° C. (230° to 239° F.). Spores of bacilli require three hours at 140° C. (285° F.)." It should be noted that these statements apply to dry heat only. In the middle of the loaf there is presumably moist heat, which is, of course, more destructive to organisms, and on that account we must make a considerable reduction in the figures of Koch when we apply them to fungi and their spores inside a loaf. After making ample allowance it is obvious that if the temperatures found by us in the centre of loaves be correct it will be safe to predict, on theoretical grounds alone, that bacteria and their spores need not necessarily be destroyed by baking inside a loaf.

CULTIVATIONS.

The next step was to ascertain whether ordinary bread contained organisms or their spores in a state of vitality. It is clear that even if the actual organisms were destroyed by baking, while their spores survived that process, there is nothing to prevent the subsequent development of the spores. The loaves used in the following experiments were obtained from various parts of London—such as Limehouse, Belgravia, South Kensington, Minories, and Barbican. In all cases the examinations were made within a few hours of the bread leaving the oven. 1. Direct microscopic examination yielded unsatisfactory results, and was soon abandoned in favour of cultivation methods. 2. A series of twenty loaves was examined by the method of plate cultivation in nutrient gelatine.

The plate cultures were made in two different ways. 1. By the trituration method. A loaf is cut in half with a large sterile knife, and a cut made at right angles into one of the halves by a second sterile knife. A small portion of bread taken from the deeper part of the second incision is rapidly triturated in a stoppered weighing bottle with sterile water, and from the resulting cloudy liquid plate cultures are poured. 2. By the "direct" method. A morsel of bread removed in a similar way is rapidly crumbled up, and a few crumbs are dropped into a tube of the liquefied gelatine, which is well mixed and poured out on a plate. The loaves used were as follows:—

LIST I.

- A.—Limehouse.
 - Three ordinary (underbaked).
 - Two ordinary (brown outside, black in places).
 - One brown (fairly baked).
 - One tinned (underbaked).
- B.—Belgravia.
 - Two ordinary (fairly well baked).
 - One tinned (fairly well baked).
 - One small brown tinned (well baked).
 - One long French roll (rather underbaked).
- C.—South Kensington, No. 1.
 - Two ordinary (well baked).
 - One ordinary (rather underbaked).
 - One brown tinned (rather overbaked).
- D.—South Kensington, No. 2.
 - Two ordinary (well baked).
- E.—South Kensington, No. 3.
 - Two tinned (well baked).

In all, thirteen species of bacteria have been found in a total of sixty-two loaves examined. For convenience of reference the following table may be given:—

LIST II.

Bacteria (or their Spores) found in a Living Condition in Freshly Baked Loaves of Bread.

1. *Bacillus subtilis*, Variety 1 (hay bacillus).
2. *Bacillus subtilis*, Variety 2 (hay bacillus).
3. *Bacillus subtilis*, Variety 3 (hay bacillus).
4. *Sarcina* (a), large.
5. *Sarcina* (b), smaller than (a).
6. *Bacillus* A (large, thick, rounded ends).
7. *Bacillus* B (large, thick, smaller than A).
8. *Bacillus* C (small, copious spore formation).
9. *Bacillus* D (smaller still; some spore formation).
10. *Bacillus* E (bacillus figurans).
11. *Micrococcus* A (small white colonies).
12. *Micrococcus* B (rosaceous (?), accidental).
13. *Staphylococcus* (very regular, larger than *staphylococcus aureus*).

LIST III.

*Results of Plate Cultures.**

- A.—1. First loaf, first plate; colonies, 25 (1), 8 (2), 2 (4).
First loaf, second plate; colonies much more numerous.
Second loaf, first plate; results approximately same.
Third loaf; colonies, 1 (1), 1 (10).

- 2. First loaf, first plate; colonies, 52 (1), 8 (2), 1 (3).
First loaf, second plate; colonies (?); liquid.
Second loaf, first plate; colonies, 6 (1).
Second loaf, second plate; colonies, over 100; much liquid.
- 3. Colonies, 3 (1), 2 (10), 3 (8).
- 4. Colonies liquid, number doubtful.
- B.—1. First loaf, first plate; colonies, 8 (1), 2 (13), 3 (2), 1 (3).
First loaf, second plate; colonies, 10 (1), 1 (13), 13 (2), 3 (7).
Second loaf, first plate; colonies, 1 (13), 6 (2), 20 (7), 2 (1).
Second loaf, second plate; 10 (? 1), plate destroyed.
- 2. Practically sterile.
- 3 and 4. Practically sterile.
- C.—1. First loaf, first plate; colonies, 2 (6), 1 (9).
First loaf, second plate; colonies, 15 (6), 1 (9), 3 (1).
Second loaf, first plate; colonies, 2 (6), 2 (5).
Second loaf, second plate; colonies, 5 (6), 3 (9), 10 (5); large liquid area with many colonies.
- 2. Sterile; all but two surface colonies, and one of *saccharomyces rosaceus*.
- 3. Five surface colonies; plate remained long under bell; 1 of *saccharomyces rosaceus*.
- 4. Also practically sterile; four plates.
- D.—1. First loaf, first plate; colonies, 1 (9), 2 (2), much liquid; could not count; one of (9)—fortunate.
First loaf, second plate; colonies, number doubtful; all liquid, (1).
Second loaf, first plate; practically sterile.
Second loaf; colonies, 1; *bacillus fluorescens*.
- E.—All sterile, except moulds and surface accidental colonies; one of *bacillus aeruginosus*.

* The figures in brackets refer to the foregoing list of organisms.

From all the colonies, which we may assume were derived from bacteria in the bread itself, tubes were inoculated and microscopical examinations made. We do not propose to give in detail the results of numerous other plate cultivations; it may be stated, however, that all the bacteria given in the list above have at one time or another been obtained from sixty-two loaves which have been examined by plate cultivations.

Thus far our experiments have shown that thirteen different species of micro-organisms can be cultivated from the centre of loaves within a few hours of their being taken from the baker's oven.

GENERAL REMARKS ON THE PLATE CULTIVATIONS.

Conditions of experiments.—The plates used were ordinary 5 in. by 4 in. photographic plates, carefully cleaned and sterilised at 160° to 170° C. in a box made for the purpose. The damp chambers were double dishes from Muencke of Berlin, 22 centimetres in diameter. The shelves were made in one piece of glass and were also sterilised by heat. The chambers were sterilised, not with chloride of mercury, but with carefully boiled water, as recommended by Dr. Percy Frankland. The incubation temperature varied from 18° to 25° C. (64° to 77° F.). The gelatine was made from fresh meat and peptonum siccum (Savory and Moore). It contained 10 per cent. of Coignet's gold label gelatine and 3 per cent. of pure glucose, and was faintly alkaline. The methods of culture have been described.

General results.—The following points may be noticed. 1. The comparative scarcity of colonies on the plates, which may be partly accounted for by the facts that (1) the number of bacteria capable of growing in nutrient gelatine is evidently variable even in closely adjacent parts of the same loaf; (2) in the case of cultures made by the trituration method only a few of the germs present on the morsels of bread triturated are removed by the sterile water, and only a very little of this water is used for the plate culture (in every case the quantity used has been as much as could be removed in the loop of a platinum needle 3 mm. external diameter, made from wire 0.85 c.m. diameter); (3) in the case of cultures made by the direct method probably almost the only germs that grow are those on the outside of the crumbs which come into actual contact with the gelatine; and (4) it is, of course, possible that many of the bacteria present will not develop colonies in nutrient gelatine. 2. While in the series of twelve plates from Bakery A there is a large preponderance of the *bacillus subtilis*, there are fewer other organisms than in the South Kensington and other West-end bakeries. We may, however, state that this is by no means always the case, as there is an evident relation between dirty dough and the number of bacteria found living in the baked loaf. This point we ascertained in the course of further experiments with artificially infected dough. These and other results must be reserved for future papers. A loaf from a low-class, dirty bakery will almost invariably contain more living bacteria (or their spores) than one from a good, clean bakery. 3. More bacteria seem to grow by the method of direct culture than by the trituration method. 4. The extremely variable results obtained, not only from bakehouse to bakehouse, but also from loaf to loaf, in the same batch. This points to the necessity of further experiments. 5. The

unexpectedly sterile condition of plates from some loaves—notably from tinned and small loaves (half-quarters). This does not necessarily prove that a particular loaf is sterile, but only that in the parts examined no bacteria capable of growing were in contact with the gelatine. In the case of a tinned loaf the heat appears to penetrate more effectually, so far as the destruction of organisms is concerned, than in an ordinary loaf. In small loaves the temperature is higher on an average in the centre than in large loaves. 6. The organisms 8 and 9 (see List II.), or their spores, are killed with comparative ease—that is, their death point in dry heat is about the same as that of *bacillus tuberculosis* under certain conditions. We may therefore assume that if these organisms survive the baking the tubercle bacillus might also survive. We have other reasons for believing that pathogenic organisms often do survive the baking. 7. A further proof of the ubiquity of *bacillus subtilis* and of the bacilli resembling it is afforded by the frequent preponderance of those organisms in our cultures. On the whole, we are inclined to think that, notwithstanding the comparatively low temperatures reached in the centres of loaves, the conditions are such that the greater number of the bacteria present in the unbaked dough are destroyed.

Accepting the fact that certain non-pathogenic micro-organisms (or their spores) can survive the process of baking in the centre of an ordinary loaf, we argue by analogy that pathogenic forms of bacteria (and their spores) may also pass safely through a similar ordeal. Our experiments, however, have not been confined to mere cultures from bread. We have made many experiments with dough infected with pathogenic organisms, but, as the indispensable evidence, obtainable only by inoculation of living animals, is wanting, we are unable at this stage of the proceedings to publish our researches on this important point.

Enough has been said, we trust, to encourage other bacteriologists to repeat and extend our experiments. If it be proved, as we venture to think has been done, that certain non-pathogenic organisms are able to retain their vitality in the loaf as it leaves the baker's oven, the fact thus established will overthrow the last defence of insanitary bakehouses—namely, that bread is absolutely sterilised by baking, and is therefore harmless to consumers. Another important result may be deduced from the following data: (1) unwholesome bakeries are exposed to infection by pathogenic bacteria—e.g., typhoid fever in sewage; and (2) such bacteria may gain entrance to the dough and maintain their vitality after baking—by analogy with what has been shown to occur in the case of non-pathogenic bacteria. If these premisses be true, it follows that pathogenic organisms may under certain circumstances be distributed in loaves and so produce specific disease among consumers.

We see no particular reason why the origin of many mysterious septic invasions of the human body may not eventually be traced to the agency of bread. A generation ago milk was not suspected of being the means of spreading disease, and a similar observation applies to water. At any rate, the subject dealt with in this paper seems to us to be well worthy the attention of all who are interested in the scientific developments of preventive medicine, no less than in the protection of the public that consumes the bread.

THE PATHOLOGY OF ENLARGEMENT OF THE PROSTATE.

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THERE are three very different views current at the present day with regard to the pathology of enlargement of the prostate. One is based upon a supposed analogy between it and fibroid disease of the uterus. This was first suggested by Velpeau, and is supported by Sir H. Thompson and Professor White of Philadelphia. According to another, enlargement of the prostate is merely one of the occurrences in a constitutional disorder that begins as arterial sclerosis and ends in fibroid degeneration, affecting the genito-urinary organs in an especial manner. This has the support of Guyon, and, following him, of the whole French school. The third has been suggested by Mr. Reginald Harrison, who holds that the

primary change is in the bladder, and that the enlargement is secondary to it and developed as a compensatory measure. None of these views can be considered satisfactory. The first is negatived entirely by the facts of development. The homologue of the uterus is not the prostate, but the prostatic utricle, an entirely independent structure, which is included in the prostate gland merely by an accident of growth, and which has never been shown to take the least active share either in tumour formation or in general enlargement of the prostate gland. The homologue of the prostate, if it exists at all in the opposite sex, is to be found in the tissues of the perineum or near the lower ends of the ducts of Gärtner—nowhere near the uterus. Nor is it supported by histology. Uterine growths originate as fibro-myomata. They have little or nothing to do with the mucous membrane or the glands that it contains. Enlargement of the prostate, on the other hand, is glandular from the first; it begins and spreads as an adenomatous growth, and even the rounded masses that it contains originate, with few exceptions, in the glandular tissue. Both organs, it is true, contain unstriped muscular fibre (although the proportion in which this is present in each and the arrangements and uses are entirely different), and the growths that originate in each of them resemble one another to a certain extent in shape (they are usually rounded) and in their anatomical relations (sometimes they are encapsuled, sometimes they assume a polypoid form); but these peculiarities are by no means confined to these organs or their growths. Unstriped muscular fibre occurs in abundance elsewhere. Rounded encapsuled masses that shell out when they are exposed by section are by no means rare in fibrosarcomata; and tumours, when they project into the cavities of other viscera, not infrequently become polypoid in shape. That these two organs, uterus and prostate, should both be liable to tumour formation and enlargement during the latter half of the reproductive period, when the first flush of full sexual activity is over, is a fact of great significance in many ways; but it is scarcely sufficient in itself to recommend the adoption of this theory even provisionally.

Guyon's hypothesis is still less tenable. It is true, as Sir Benjamin Brodie has pointed out, that enlargement of the prostate and atheroma frequently occur together; the time of life at which they are both most common is the same; but this does not show in the least degree that the one is the cause of the other. Enlargement of the prostate does not begin as fibroid degeneration but as glandular overgrowth. Arterial sclerosis and passive congestion may do a great deal, but they cannot cause this, and still less can they cause the growth of masses that sometimes almost fill the lower part of the bladder. Moreover, enlargement of the prostate is compatible with perfect health, continuing sometimes even to extreme old age without its existence being so much as suspected. Those who suffer from urinary troubles form but a small proportion of those in whom enlargement occurs. The vast majority are free, and, although some may be atheromatous, many live on with no evidence of vascular degeneration other than that which is the normal companion of healthy old age.

There is even less to be said in favour of the view that the primary change is in the bladder and that the enlargement is a compensatory contrivance. The prostate, both as regards its glandular substance and its stroma, is of sexual origin, and has to deal only with sexual functions. It develops with the testes and atrophies if they are removed. It has nothing to do with micturition, which, if the prostate remains undeveloped or undergoes atrophy, takes place just as well as it does if the prostate is normal. Nor is there any evidence that the sinking of the posterior part of the bladder is a primary change, or that when it does sink the prostate enlarges in compensation. This can only be imagined so long as the erect position is maintained and the prostate bears the same relation to the bladder that it does in man; whereas enlargement takes place in dogs and other quadrupeds, in which the anatomical relations are entirely different. Nor is it easy to understand why compensation for sinking of the posterior wall of the bladder is confined to one sex.

None of these theories, therefore, can be considered as offering a satisfactory explanation. There are, however, certain facts, some long since known and almost forgotten, and others of recent discovery, that suggest that the real cause is to be found in another direction. The enlargement is in some way dependent upon the testes. The evidence in favour of this is briefly as follows. The normal development