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On "Disinfection by Heat, with Description of a New Disinfecting Chamber," by JAMES ADAMS, M.D., L.R.C.S.E., F.F.P.S.G. *Paper read September 28th,* 1883, *at the Congress of the Institute, held at* GLASGOW.

TO Dr. W. Henry of Manchester is due the merit of conceiving and demonstrating by actual trials the fact that heat destroys or neutralises the morbific matter of certain communicable diseases. In 1832 (Phil. Mag., vols. x-xi) he published the results of experiments, showing that cow-pock matter becomes totally inert after an exposure *of some hours* to a temperature of 140° F., and that clothing designedly infected with scarlatina and typhus *contagium* was afterwards worn with no bad results by individuals likely to be susceptible, such clothing having previously been confined *for some hours* in a temperature of 200° to 206° F.

Since the time of Dr. Henry, various methods have been employed for practically utilising his richly suggestive conception, but in some instances without observance of his precautions, of which more hereafter. Meanwhile, a short reference to the nature and properties of the matter that transmits contagious disease will aid my description of a new Disinfecting Chamber professing to have some important distinctive features.

Of the constitution of morbid poisons we are as ignorant as of that of prussic acid or of strychnine, nor is it probable that the knowledge, if we possessed it, would throw any light on their mode of action, so far as relates to the essential morbific principle. We are well acquainted with their *chemical composition* without in any way understanding how they act so powerfully on the animal system; but we know they are capable of being decomposed by weak chemical agents, and rendered inert by temperatures of about 200° F., and therefore that their constitution is not stable, and that they are held together by very feeble affinities.

That contagion is connected with the diffusion of oganisms possessing vitality may now be assumed as an established axiom. This doctrine, known as the "Germ Theory," is usually ascribed to Pasteur, and it is undeniable that he is entitled to pre-eminence in enunciating the truths on which the doctrine is now so soundly based. But the notion was held as far back as 200 years ago, and originated with Kircher, who advanced the view that *animalculæ* or *acari* diffused through the atmosphere was the true originating cause of epidemic and contagious diseases. His opinion was endorsed by Linnæus, and advocated by other eminent men, but did not meet acceptance among physicians. On the contrary, it was by them, until a recent date, held that there existed a deleterious principle or medium, vaguely expressed by such terms as "fomites," "virus," "effluvium," &c., which became contagious or epidemic by some occult and mysterious influence, some "corruption of air," or some spontaneous change in inanimate matter. And it is illustrative of the general notions that long prevailed, and at same time interesting in the present connection to note, that Dr. Henry, who devoted much consideration to the problem, and whose name is so honourably associated with the subject of disinfection, was a strong disbeliever of Kircher's hypothesis, and characterised it as singularly unsound—as having not a single valid analogy to confirm it—and as being at variance with all that is known of the diffusion of volatile contagion.

So late as 1860 the mysterious *something* that originated epidemic and contagious disease, was a matter of keen controversy, and was generally believed to have its most probable solution in establishing or disproving the doctrine of Spontaneous Generation, regarding which the views of the illustrious Liebig were held open to refutation by his no less illustrious contemporary, Pasteur.* The continuous and untiring researches of Pasteur, aided by others, and at a later date very materially by those of his able expositor, Tyndall, have clearly demonstrated not only the fact of the generation of certain communicable diseases by living organisms or "germs," but have gone far to make it very probable that it is through living organisms or germs that all zymotic diseases are propagated. According to this doctrine putrefaction and epidemic disease alike, arise, not from the air but from something contained in the air. This something is not a vapour, nor a gas, nor a molecule of any kind, but a *particle* or bit of liquid or solid matter formed by the aggregation of atoms or molecules. Tyndall has shown that the air is at all times, even when free from microscopically visible particles, beset with much smaller ultra-microscopical particles, and he alleges that air from which the particles have subsided—air which is "optically pure" as determined by the electric beam—

* See Addenda.

is no longer capable of contaminating liquids, or of inducing fermentation or putrefaction. From such facts and from a review of the whole field traversed by other observers, he concludes that these particles are organisms, the germs of septic Bacteria. "The thing" he says, "which we vaguely call a 'virus' is to all intents and purposes a seed * * * as surely as a thistle rises from a thistle seed, as surely as the fig comes from the fig, the grape from the grape, the thorn from the thorn, so surely does the typhoid virus increase and multiply into typhoid fever, the scarlatina virus into scarlatina, the small-pox virus into small-pox."

This "virus," these particles or germs, are *volatile*—are shed freely from infected individuals and from their clothing, and are wafted to and fro by ærial currents. Hence all appliances which have for their object to limit the spread and ensure the destruction of these germs should be planned with special provision for this dangerous property of volatility in the germs of disease.

While the hypothesis of spontaneous generation was in dependence and under anxious discussion throughout the scientific world, heat was the chief agent employed to determine the actual vitality of microscopic forms that are developed so abundantly in infusions of organic matter, and even in saline solutions. And while there was a general *consensus* to the effect that heat destroyed the evidence of vitality drawn from active movements, there was a great conflict as to the *degree* and *duration* of heat required. Moreover, there was a renewed generation of the same forms frequently observed in the liquids supposed to have been sterilised, which naturally gave rise to the belief in spontaneous generation. The first light thrown on this perplexing riddle was by Professor Cohn of Breslau, who showed that confusion and contradiction had arisen from having failed to distinguish *the growing germ* from its seed or spores. And it was reserved for Professor Tyndall to pulverise and destroy for ever the doctrine of spontaneous generation by an exhaustive series of brilliantly conceived and thoroughly conclusive experiments. He showed that the vitality of a germ or embryo organism and its disposition to development was a latent property dependent on different conditions of spores, age, dessication, exposure to air and light, &c., and that subject to these varying conditions there were required varying periods for germination: that in the operation of boiling that was usually adopted for their destruction, the broods or crops may overlap each other, the new brood making its appearance before the old brood died away, and that by repeated boilings at short intervals—and not by prolonged boilings—dealing with each successive crop as it springs into active life, there was effected ultimately a complete destruction. Finally he showed how all preceding observers had failed in their best intended efforts to procure a condition of the air absolutely or—as he phrases it—"optically pure"; and with this necessary condition of purity secured, he predicted with an accuracy that has not since been gainsaid, that there never would again occur an example of so-called spontaneous generation.

The distinction to be observed between the seed itself, and the developing organism, cannot therefore be too strongly impressed on the mind when dealing with methods of germicide. Notoriously the growing or adult organism can be easily destroyed. Not so the seed. The contrasted difficulty has been well expressed by our senior city member, Dr. Cameron, in his very instructive and in every respect excellent monograph on "Microbes." Dr. Cameron says "as to the spores which they [*i.e.* the developing or adult organisms] produce, and from which succeeding generations spring, there is almost no killing them. The more you dry them the better they resist destruction. Time is no object with them and they maintain their dormant vitality for an indefinite number of years. Absolute alcohol has no effect on them. As to oxygen they can stand that concentrated by the pressure of twenty atmospheres, and be none the worse. Two or three hours' boiling if they have been well dried beforehand, seems not to hurt them, and they have even been known to resist eight hours' of the process. *The only effective means for their immediate destruction that I am aware of is the flame of a spirit lamp.* To that their extreme minuteness renders them an easy prey."

But Pasteur never saw germs resist 230° F., or the adult organism from 122° to 140° F. *when in the moist state.* Chaveau, Calvert, Roberts, Tyndall, and many others, have shown that from 140° to 212° F. is a degree of heat that few developed germs can sustain. According to the very recent experiments of Koch, Wolffhügel, Gaffky, and Lœfleur, exposure to temperatures of 212° to 221° F. in dry heated air effects easily the destruction of Baccili and "adult" Bacteria, while *spores* of mould were not killed after being subjected for one and a half hours to air heated from 230° to 240° F., and *spores* of Baccili were destroyed only after being confined for three hours in an atmosphere of 284° F. On the other hand, these last-named observers have given very important evidence as to the difference in effect of heat according as it is dry or humid, for they found that spores of garden earth and of carbuncular disease lost all vitality by an exposure of only ten minutes in hot vapour registering 230° F., and they assume that this is the temperature to which ought to be heated any morbific principles of an unknown nature which can transmit disease. Their experiments show that in a practical point of view one cannot have absolute confidence in dry heat for the disinfection of all suspected objects. On the whole it may be affirmed that duration of heat and its degree are mutually complimentary both with dry and with moist heat, a long exposure to a low degree being equivalent to a shorter exposure to a high degree.

The natural inference from such experiences is that in seeking to disinfect by heat, that heat should be carried as high, and contined as long as is possible, or necessary, to ensure the destruction of infective matter; subject only to the limit that infected clothing or other articles operated upon, may not be injured or destroyed. And in determining the practical limit that is expedient, I have again to make honourable mention of Dr. W. Henry, who made practical trials 50 years ago on fabrics of clothing and other articles, which have left little save gleanings for subsequent observers. For the experiments of Ransom, Chaumont, Vallin, Koch, Wolffhügel, and others have added little beyond a confirmation of Dr. Henry's conclusions, and to these later observations I need not therefore refer in detail. They show in summary, that exposure in dry heated air to a temperature of 220° to 230° for one or two hours is sufficient for disinfecting purposes, does not injure the integrity nor the appearance of ordinary clothing and bedding, but that a temperature of 250° is risky, may cause injury, and is therefore unnecessary.

In considering the practical application of heat to infective matter for disinfecting purposes, we need not take into account the exceptional laboratory experiences of various observers who have recorded extraordinary degrees of heat withstood by some germs. These are little likely to be noticed in future now that Tyndall has specified so clearly the conditions under which they are likely to have occurred. These rare instances are matters of curious interest more for the naturalist than for the sanitary physician. The general evidence now accumulated, is abundantly sufficient to warrant the conclusions recently laid down by Pasteur and Leon Colin in their Report on Disinfecting Stoves, made in 1880 to the Council of Health of Paris and the Seine. These are to the effect that while humid heat of 212° F. will certainly destroy all life in morbid germs and all dangerous condition in virulent matter, a temperature between 212° and 230° F., whether dry or humid, may be fixed as sufficient for all practical purposes. A higher temperature is unnecessary.*

* See Addenda anent high temperature

When selecting an apparatus for disinfecting by heat, there is a somewhat embarrassing choice between those which are more or less portable, and sold in the ordinary course of commercial manufacture, and those which are erected on an extensive scale to special designs and for the needs of a large population —between those which are employed for occasional use, and those where the process of disinfection may be a matter of daily or hourly requirement. In a description of my own apparatus I will, therefore, best indicate the points wherein others in frequent use seem defective, and wherein my own contrivance seems to supply a desideratum. I say so much because I was led to realise my conception on the request of Major-General Collinson, Architect to the Prisons' Commission for Scotland, who did not find in existing apparatus the economy and convenience or efficiency that were desirable, and in his view attainable. He sought a portable appliance that could be easily fitted up in an ordinary appartment of a gaol, and be quickly brought into use for the few hours during which disinfection of clothing, &c., was occasionlly required. Dr. Littlejohn, with whom I also freely discussed my plan, and to whom, as well as to General Collinson, I exhibited a small model, was clearly of opinion that the apparatus was peculiarly suitable for the service of gaols, workhouses, small hospitals, small country towns and various localities where the use of disinfecting chambers were practically excluded, because of first cost usually was then doubled by the conjoined expense of a special building, and subsequent considerable cost of working, irrespective of their shortcomings in real sanitary efficiency. Several medical officers of health with whom I have since exchanged views, have added their cordial concurrence in this opinion.

The "principles" on which my chamber is planned have reference to the following points. 1. Portability, cheapness of construction, and economy in use.* 2. Improved method of causing heat and of preventing subsequent waste of fuel. 3. Equality of heat throughout the chamber. 4. A maximum or any desired temperature maintained by a simple contrivance acting automatically. 5. Germs or other infective matter disengaged from infected objects, not permitted to escape into general atmosphere. 6. Infected objects not exposed to products of gas combustion, but only to pure air, conjoined with moisture when desired. 7. Gas consumption regulated by automatic governor to an efficient average quantity.

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* The company that manufactured the workshop model shewn at the Sanitary Exhibition at Glasgow has, with my permission, taken out a Patent, and I am informed that the selling price of a chamber of 130 cubic feet inside capacity will be about £40.

These points I will now comment upon.

The sides, top, and bottom of the chamber form so many complete pieces, prepared, adapted, and numbered in the workshop so that they can be easily put together by any ordinary workman. The interior is furnished with the usual rods and hooks for supporting articles that are being disinfected. A powerful gas stove of peculiar construction, exposing a large heating surface in its interior traversed by pure air, is connected inside or outside of the chamber, preferably on the outer side. It is then only necessary to attach the gas supply and lead the stove flue into the chimney of the apartment in which the chamber is placed. The walls of the chamber consist of double casings made of thin plates of sheet iron, one-and-a-half inches apart, air tight, and containing only confined air. Each casing is farther subdivided by partitions to prevent heated air accumulating in the higher compartments. Confined air is the best non-conducting medium and renders unnecessary the packing material that is usually employed. Such packing in the degree in which the material is solid, necessarily absorbs heat at the beginning of an operation and prolongs the time requisite for getting up the desired temperature. Such packing—the most useful of which is boiler felt—may, however be applied to my chamber with good effect when it is intended to be kept in use for many hours or days at a stretch; otherwise it is unnecessary. A case of wood lining outside, with an inch space between it, and the sheet iron forms a second air jacket, and the wood, by its slow conducting and radiating properties, conserves the heat still farther.

The bottom of the chamber is double, having about six inches deep of an under space communicating by openings at one end with the general interior. The heating stove, whether placed within or connected outside, draws air from the under floor space through two separate pipes, one leading to the furnace, the second admitting air to the pure air caliducts of the stove. The under floor space, therefore, communicates at one end with the general interior of the chamber and at the other end with the stove, and the air drawn from the under floor space divides into two currents, one of which passes through the furnace and ultimately escapes into the chimney of the apartment. The other and much larger current follows a separate course through the stove where it becomes highly heated, and is discharged in a pure state into the chamber. It is not then permitted to escape outside, but is drawn down through openings into the under floor space, and again led into the stove highly heated, and again discharged into the chamber, with each circulation gaining increased heat until the maximum temperature desirable is attained. And so the circulating current goes on traversing and retraversing the general chamber from end to end and from top to bottom in brisk movement as long as the stove continues lighted. The "principle" of action is analogous to that of heating by hot water pipes. This automatic movement of hot air in a circle ensures a nearly equal temperature throughout the chamber. The briskly moving current passes through the suspended clothing more effectively than a stagnant atmosphere can penetrate. It plays in the manner of a light breeze, rustling the garments, disengaging and floating off infective matter, while the rapidly successive impact of heated air molecules must oxidise more effectively than a still atmosphere.

The volume of air drawn through the stove furnace would suffice to fill the chamber more than twice every hour. That quantity is replaced by fresh air continuously admitted from the exterior through a simple valve that prevents reflux. The much greater body of air that moves through the chamber in an automatic circuit would suffice to fill and empty it about fifteen times within an hour, giving a complete revolution and replacement about once every four minutes. All infective matter volatilised and floating in the larger current is therefore being continuously heated and re-heated within the pure air caliducts of the stove, while any matter floating in the smaller current that is being continuously drawn through the stove furnace and burned gases caliducts, is thoroughty carbonised and conclusively dealt with. At the expiry of the time given to the operation, an aperture in the upper part of the chamber is opened to permit the escape of the confined air, which passes through a pipe that has its point of discharge underneath a solid gas flame, so that any infective matter still suspended in the air must pass through this flame. *Thus, from beginning till the end of the operation, all volatilised matter liberated in the chamber must pass through flame.*

I place much stress, and not unduly, on the paramount importance of imprisoning volatile infective matter throughout the entire period given to a disinfecting operation. There is otherwise a very fallacious security. The quantity of epidermic scales shed from the skin of patients affected with typhus, scarlatina, smallpox, and other eruptive fevers throughout the desquamative or convalescent period is very great, and these together with the more subtle, or less visible emanations exhaled by the skin or breath, or the grosser discharges from typhoid or diptheritic patients, contain the matter that makes those diseases commuicable. I have seen the shirts and other clothing of such patients when held up and briskly shaken, discharge clouds of contagious dust, similar to what takes place in shaking a flour bag. Now this infective matter may be dried and exposed to the air for weeks together, and yet lose little or nothing of its virulance, and this dust constitutes the infective particles or germs that we dread should float from a sick room into the adjoining apartment of a dairy or farm-house where it may settle in the milk vessels—that we apprehend from the association of our children with school mates recently convalescent, or coming from an infected house—that we fear may be suspended in the atmosphere of a cab that has conveyed an infected patient. How evidently necessary it, therefore, is that in a disinfecting operation there should be provision made for following up to utter extinction such contagious matter. The facts that have within so recent a period been adduced, and that since are daily illustrating the vital importance of the "Germ Theory" were not in the prevision of those who designed the gas disinfecting chambers at present in frequent use throughout the country. There is not in many of them any provision for such heroic treatment as I am advocating. In some the infected clothing is merely hung up in a chamber travesed by a current of hot products of gas combustion that enters at the bottom, and passes instantly out at the top. The shirt or blanket may be detained for a conventional period of two hours or thereby, but any volatile matter that is disengaged, is instantly swept off into the general atmosphere. The shirt or blanket is itself disinfected and may afterwards be worn with impunity, but where do the infective "particles" alight? They have not been detained for two hours nor for as many minutes. They have not been subjected to the necessary temperature during the necessary period of time, nor for any notable period of time. They cannot under a momentary exposure to heat of 230° or 250° have been rendered inert. They have simply been blown away to join the idle wind that wanders where it listeth; and we never can know what becomes of them during the weeks, or it may be years, that they retain their deadly properties, although we may conjecture when we meet epidemic and contagious disease springing up in some locality under inexplicable conditions. Dr Henry detained his cow-pock, scarlatina, and typhus matter within his heated laboratory oven for *hours*, proving as he cautiously proceeded that time was an all important condition in disinfecting by heat—that sometimes a duration of exposure for one hour, sometimes for two hours was insufficient, and that where he stopped short the contagious matter retained its potency. All subsequent observers have followed in the same lines, and confirmed substantially his conclusions.* When Pasteur, Tyndall, and others, boiled the germs on which they operated, they found at one time one hour, at another time three hours too little. But we now know through the teachings of Tyndall that at the beginning of a heating operation there may be germs in such an embryo state that heat of a certain duration only stimulates their growth a stage, and if at this stage the exposure to heat be discontinued, the germs retain their vitality, although—as shown by Pasteur—in a frequently attenuated condition that impairs their subsequent activity or virulence. Heat must therefore be long continued, or it must be renewed more than once, or it must be applied in the form of actual flame, a verification of the old adage "fire purifies all." Less capable observers than Pasteur and Tyndall stopped short in their process of boiling the liquids they assumed to have been sterilised by one boiling operation, and when afterwards vital organisms appeared in the liquids, these were assumed to be examples of "spontaneous generation." Let it be assumed that in the experiments of Pasteur and Tyndall, there had been permitted to boil over from their flasks a number of germs not yet sterilised, these would have dried up, and afterwards have become capable of being developed through ever active agencies. What did occur in the fallacious observations referred to, and what might occur, are conditions analogous with those which obtain in a chamber, where volatilised *contagia* are floated from off the surfaces of infected clothing and wafted into the general atmosphere on a strong up-rushing current of burned gases. The best known of these chambers is in its mechanism fashioned with an ingenuity that reflects credit on its inventor, and from its commercial success has been honoured with imitations, but that chamber was in its "principles of action" designed eighteen years ago, and consequently before the "germ theory" had become an accepted fact. In the light of our present knowledge that chamber and all others of like "principle" are behind the time.

It is not that products of gas combustion have, or are alleged to have, any destructive effect on germs, or any effect whatever apart from their heated condition that differs from pure air, or a mixture of air and watery vapour. It is, on the contrary, open to surmise that certain products of gas combustion—certain tar products—may actually have a temporary conservative effect of the kind that Dr. Dougall, of this city, showed to be produced by the vapour of carbolic acid on vaccine lymph. The variations in the products of coal gas combustion,

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* See Addenda.

according as that is more or less perfect, and the variability in the composition of coal gas, its impurities and the like, make this surmise a point worth consideration. Dr. Dougall, whose researches on chemical disinfectants are, in extent and original conception, second to none, is of opinion with me that the influence of burned gases is not only probably, but very likely, of the nature I have suggested. But assuming that they have no influence either for good or for evil, so far as destructive action on germs is concerned, it still seems to me that to avoid pure air and to make choice of an atmosphere contaminated with the products of gas combustion, is something equivalent to a preference for dirty water before clean water for washing purposes. It might be tolerated if no other method of applying heat was available, and it is an extenuation if used in the belief that exposure to the burned gases of gas coal will do no harm. But on this latter point I have grave doubts, and if compelled to give over my property for disinfection, I would be very fearful of the results of exposing a piece of fine lace, a delicate coloured silk fabric, the valued photograph of a deceased friend, a precious letter, or an important law document to be played upon for a couple of hours by a strong current and heavy atmosphere of burned coal gas.

Setting this point aside, it is deserving of note—although the least of probable evils belong to the use of such chambers—that the waste of heat, and consequently of fuel is so great, that the hot products of combustion escape in enormous quantities, carrying off heat that has been only partly utilised, and this waste of heat is aggravated by the method of burning, in which the rush of inflamed gas is accompanied by a practically unlimited quantity of unnecessary cold air that mixes with the hot gases, dilutes and cools down their temperature, and renders one-half or two-thirds of the heat ineffective. Instead of the gas being burned with about ten volumes of air to one of gas, the theoretic proportion, or even twenty volumes, which is the practical limit aimed at by all who have studied the laws of gas and coal combustion, I have estimated that from 150 to 300 volumes of air are admitted into such chambers for each volume of gas. But so little consciousness does there seem to be of extravagant waste on the part of the makers, that I have seen a letter written from one who refers to 50 or 100 cubic feet of gas per hour as the required quantity for his apparatus, and as a quantity "not worth consideration." And yet a chamber of equal capacity can be equally heated with pure air by the use of 20 cubic feet of gas properly burned and properly applied. Before leaving this question of *pure air* versus air contaminated with burned gases, I may plead that while pure air can have no hurtful action on clothing or other infected objects, it has on the other hand assuredly an oxidising and distinctive action on infective matter. Still farther let me urge that there can exist no valid reason why the same chamberful of air when once heated, should not economically be made to do duty again and again in the manner of hot water warming apparatus, more especially when the heat can be kept up to give all the effect that heat can give by the use of one-third or one-fourth of the gas fuel that in contrasted arrangements is *avoidably* wasted.

I turn to a description of the means I employ for maintaining a desired maximum temperature. A thermometer fixed inside the chamber, but visible outside, indicates the heat. A single Arnott chimney valve, specially balanced and regulated, supplies every other necessary requirement. This valve consists of a thin leaf of metal resting with a fine edge upon a delicate balance socket. A rod with a screw turned upon it, and having a weight at its extremity is attached to this balanced leaf, and by rotating the screw the weight is shortened or lengthened from its fulcrum. The principle of the valve is that of a balance having at one end the elastic force and pressure of air confined at 250° F. (or any other temperature that the valve is weighted to withstand the elastic force of) and at the other end the equivalent force or counterpoising weight of metal attached to the valve. When the air becomes heated above 250° F., the elastic force and pressure upon the valve is increased, the valve yields and the heated air escapes until once more the pressure of the air and the weight of the valve become equal. The adjustment of the valve is made when the thermometer reaches 250° F. (the temperature at present assumed for illustration) and it is effected by simply turning the milled head, which, for convenient manipulation, is attached to the screw rod. When the valve is perfectly vertical at 250° F., preventing all escape of heated air, but yielding at 255° F. or thereby, the adjustment is practically perfect, and requires no further attention for that temperature. By this contrivance I dispense with costly arrangements easily deranged, and in practice not admitting of ready adaptation to varied standards of temperature that may.be desired, while other automatic arrangements when out of order can only be repaired by specially skilled workman.

To meet the requirement that has arisen from recent observations, showing that moist heat is more effective than dry heat, I employ a simple provision whereby the chamber is charged with watery vapour, subject to the same control as regards temperature, as in the case of dry air. A shallow vessel containing a quantity of water, about 16 oz., is placed in the line of the entering hot air. The water evaporates, and the moisture condenses on the colder clothing, ultimately becoming steam of the elastic force, proper to a mixture of air and moisture at 250° F. Should the temperature exceed 250° F., the valve moves precisely as in the case of dry air alone; for in all cases the sum of the elastic forces of a mixture of moisture and of air is equal to the weight of the atmospheric pressure, which has its equivalent in the weighted balanced valve, the vapour taking always the force due to its temperature, and the air making up the complement. The arrangement is entirely free from the cost, difficulty of management, and dangers attendant on the great elastic force of steam generated in and delivered from a high pressure boiler.

In the application of moist heat it is desirable that the watery vapour should be generated at the early part of the disinfecting operation, and while the clothing or other infected objects are at a relatively low temperature, so that the moisture may become condensed upon them, and afterwards it is only necessary that the temperature of the vapour does not descend below 212° F.. Instead of a water vessel, the object may be very well effected by suspending a piece of woollen blanket of about half-a-pound weight, wrung out of water, of which it will retain about 1lb. And it may be still better effected by simply sprinkling the infected articles pretty liberally with water, and directing that they are not to be removed from the chamber until quite dry. Under all circumstances, it will detract from the efficiency of the moisture, if that moisture is delivered at first of a high temperature, say 220° and upwards, because at these temperatures it approaches the condition of a perfect gas and will only act as dry heat.

After the strictures I have passed on any mode of disinfection by heat that permits the escape of volatile *contagium* into the atmosphere, it is only due that I should state that disinfecting by heat in a chamber which confines all volatile matter until the close of the operation, is carried out in various apparatus in frequent use. Dr. Esse of Berlin, who has given much attention to the subject, makes this the principle of several chambers constructed under his direction at the Hospital, St. Moabit. In one arrangement the infected articles are placed within a cylinder, which is itself contained within a larger cylinder, the space between being filled with steam, and thus the inner cylinder is heated. There is no communication between the outer and inner cylinder, and the latter contains dry heat only. This is precisely the arrangement on a larger scale devised by Dr. Henry in his small laboratory apparatus 50 years ago. In another form Dr. Esse's chamber is lined or traversed with steam pipes. M. Vallin, in his comprehensive treatise on disinfection, objects to Dr. Esse's chamber that the air is stagnant and cannot have the heat penetrating property of hot air in motion. He suggests—and the suggestion is applicable to other apparatus—that at a stage of the operation a stop cock should be opened, admitting a jet of vapour for 15 to 45 minutes, having a temperature of about 212° F.; this vapour to be afterwards got rid of by ventilating openings, and the operation completed by dry heated air, so as to thoroughly dry the infected articles. He believes such an arrangement to be perfect. I think it will be evident that in my chamber, the process that M. Vallin recommends is practically ensured with this superadded advantage,—that ultimately all air or vapour that has been in contact with the infected matter is made to pass through a furnace.

Mr. W. Lyon, a London manufacturer, supplies a machine that confines all volatile *contagium* till the close of a disinfecting process, and also aims at carrying out the "principle" of moist heat; and as it professes likewise to introduce an entirely novel "principle" of action peculiar to itself, of which moist heat is the characteristic, that claim requires the detailed examination I give in an addendum, because of its novelty and, as I believe, of its fallacy.

In conclusion, and on a careful consideration of the entire subject of disinfection by heat, I feel warranted in laying down the following axioms, of which the first is a truism:—

1. That as contagious matter volatilised and diffused in the atmosphere is the chief condition that propagates contagious disease, it follows that the best preventative consists in excluding the entrance into the atmosphere of such contagious matter.

2. That any apparatus for disinfecting by heat is insufficient that does not make provision for detaining volatile contagious matter, and subjecting it to the same degree of heat, at least, and the same duration of exposure that is found practically necessary in dealing with infected clothing, bedding, and other objects.

3. That the alternate use of moist and of dry heat, or of both conjoined, is preferable to the use of dry heat exclusively.

4. That in disinfection by heat, the inmmediate and certain destruction of volatile contagious matter can only be ensured by passing such matter through the flames of a furnace.

ADDENDA.

In 1860 it was announced as a discovery by Schröder that "the atmosphere contains an active substance which induces the phenomena of fermentation and putrefaction, and which is decomposed by heat and arrested by filtration," although why muscle, yolk of hen's egg, and milk should occasionally putrefy in spite of filtered air alone being permitted to come in contact with them was a point most difficult to explain, and one which Schröder admitted was not in perfect accord with his theory. Pouchet about the same time cited several experiments which he considered crucial, and which he performed with the special object of settling the question of spontaneous generation, and he affirmed that the results of the experiments were perfectly conclusive of the spontaneous origin of animal organisms. To test this doctrine Pasteur made very extensive investigations (published 1860) which resulted in establishing the fact that the air at all times contains microscopic organised corpuscles, which may be collected by filtering it through gun cotton, dissolving the cotton in ether, and allowing the ether to evaporate spontaneously. Pursuing his investigations into the origin of ferments and putrefaction, he in 1863 announced that his previous experiments had entirely disposed of the hypothesis that fermentation can be effected by the influence of decomposing albuminous substances, and affirmed conclusively that the sole agents in the process are animalcules or their germs, termed by him "vibrios," of which fact he adduced examples. From this date the "germ theory" made rapid progress, although strenuously resisted up to a very recent period by Bennett, Bastian, and other very able observers.

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Up till a comparatively recent date, the highest temperature short of actual injury to infected clothing, &c., have been employed or thought desirable, while by tradesmen who vend disinfecting apparatus, a minimum standard of 230° to 250° F., or even higher has been assumed and taught as necessary, and the power of producing temperatures of even 300° or 450° F. has been vaunted as a claim to recommendation. It should, however, be very obvious that in *all* gas heated chambers the *degree* of heat that can be reached above the highest heat that is necessary, *viz.*, 230° F., is a mere matter of so many extra feet of gas, and therefore implies no superiority in the apparatus, because an equal degree is attainable in *any* gas heated chamber.

But the point that deserves consideration in this relation is the expenditure of gas at which any temperature that is desired can be attained and maintained.

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The important distinction that obtains between the vitality of the spore, or germ, or particles, and the organism which is developed from the spore, is all in all in considering the practical application of heat in the process of disinfection. For the quality, degree, or duration of the heat which destroys the vitality of Bacteria, Torula cells, spiral fibres, and fungi, fails to destroy the spore of the Bacterium. We have quite a cloud of witnesses as to the effects of heat on germs and their offspring, for the long contention on the doctrine of spontaneous generation that has so very recently received its quietus, brought into the field of practical investigation a host of observers. A very favourite method of experimentation consisted in heating or boiling infusions of vegetable or animal material, in which Baccili, Bacteria, and Vibrios are so readily and so abundantly developed. On the point merely of the degree of heat that will destroy Bacteria, there is a pretty general concurrence of experience, and Pasteur, Chaveau, Calvert, Roberts, Tyndall, and many others have shown that from 140° to 212° F. is a degree of temperature that few developed organisms can sustain. Haizings agrees with Cohn that Bacteria are killed by 10 minutes boiling, but against this general assertion Roberts has shown that the length of time varies greatly, according to the nature of the matter that develops the Bacteria, one kind requiring 20 minutes, another 40 minutes boiling. If, however, the infusion is rendered alkaline, Roberts alleges that it is not sterilised until one, or two, or even three hours boiling. Mr. Dallinger and Dr. Drysdale report that while certain living septic monads were killed by a heat of 140° F., there were spores of one variety that germinated after exposure for 10 minutes to 200° F. Instances are recorded of certain spores resisting for hours a temperature of 400° F. The resistance to destructive agents of certain spores is very extraordinary. Koch relates that the spores of splenic fever—a disease of cattle that occasionally affects—and it may be in a fatal form—the human subject, retained their infective vitality for an indefinite period in spite of all kinds of mal-treatment. They could be reduced to dust, wetted and dried repeatedly, kept in putrefying liquid for weeks, and, nevertheless, at the end of 4 years they still displayed an undiminished virulence. Such experiences give point to the principle for which I am contending, *viz.*, that in disinfecting by heat, the only effective application for the immediate destruction of infective matter suspended in the air is to subject it to the action of the flame.

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I am not aware of any experiments exactly similar or so direct as those which Dr. Henry made with infective matter of cow-pock, scarlatina, and typhus. Those with cow-pock matter have been repeated, but the others have not. He dealt with what he recognised only as infective matter in the gross, and without thought or reference to germs. Our conclusions, therefore, regarding disease germs are drawn from a different class of observations, and mainly from the known analogies that link together the entire animal and vegetable kingdom. The number of species and the number of individual varieties of germs are very great, and a distracting multiplicity of names have been invented, and are being continually manufactured and applied by different observers in the field of investigation that is covered by the germ theory. And it is perplexing, even to a well-informed student of modern scientific literature, to track out the intended meaning of some writers who, within half a page, will make reference to half-a-dozen names all with little apparent discrimination, and often with little knowledge that they are dealing occasionally with mere synonyms. I give here a cluster of these ingeniously varied descriptions of minute organisms: cells, monads, germs, seeds, spores, sporules, torula, spirilla, vibrios, bacteria, baccili, zooglea, micrococci, microzymes, microphyptes, microbes, microzoaires, entophytes, saprophytes, infusores, contagium corpuscles, particles, &c., &c.

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Washington Lyon's disinfecting apparatus is substantially the same as Dr. Esse's, but the cylinders communicate through a stop-cock. The outer cylinder is first heated by the admission of *saturated* steam with the effect of heating at the same time the inner chamber. The objects to be disinfected are then placed within the chamber and the cover tightly closed by a number of screws, an operation requiring about ten minutes, during which the imprisoned air and the infected objects are getting heated. The stop-cock of communication is now opened and steam of about 260° F. is admitted. If any condensation occurs, provision is made for that being immediately carried off by drainage pipes. But it is stated that "*the great object of the casing* [*i.e.*, the heated outer cylinder] *is to prevent condensation* of the steam within the chamber, for if such a condition were to arise the object would become wetted." Nevertheless the theory of the maker is that "the steam on coming into contact with the colder infected objects *instantly condenses* upon them until *all are damped and moist*, and when the pressure is removed the *water*, no longer restrained, evaporates, and the objects remain *in an almost dry condition*." M. Vallin, when commenting on this phenomena, says "this is what we do not very well understand," and his difficulty claims sympathy. An eye-witness to the process whose evidence is adduced by the maker says "on opening the door the several articles above named [*i.e.*, various fabrics and a letter in coloured ink] were *to our surprise and satisfaction perfectly dry!*" Now there is here a blowing of hot and of cold, a condensing, a wetting, and a damping conjoined with a provision against condensing, or wetting that needs to be reconciled. My explanation is that the maker's theory—a very pretty one by the way—is altogether based on a fallacy. I feel assured that no condensation of steam can take place unless slight and momentarily at the close of the process when the articles are being removed from the chamber. At the instant when the cover is swung aside there will take place a rush of cold air, a portion of steam will be condensed, and the remainder dissipated. For steam so far from being a moist fluid is *perfectly dry* as long as it retains the elastic force due to its temperature—that is, so long as the temperature does not fall; and in this case the temperature is kept up by the hot outer casing and the continuous supply of hot steam, from beginning till the end of the process. It has been pointed out by a great authority that "steam is of so drying a nature that it cannot be contained in wooden vessels (however well seasoned they may be) without drying them and making them shrink till they crack and fall to pieces. Steam is never moist." When steam is *condensed* it becomes water itself, and the mere removal of pressure will not evaporate water, although it will permit the dispersion of steam wnen liberated from pressure. If the articles placed in the disinfector under consideration are ever penetrated with steam that has *condensed*, then assuredly they are wetted with water as effectively as if cold water had been poured upon them, and that water will only evaporate under a separate drying operation. If the chamber had been filled only with saturated steam, and if that steam had been isolated from water in a space of fixed dimension, and if the temperature had been permitted to fall, instead of being steadily maintained, there would then have been a certain amount of condensation, although very trivial because of the little margin of fall that could be possible under the conditions. But the chamber is not filled with saturated steam, but with a mixture of hot air that previously filled the chamber, conjoined with as much moisture from the steam as the air can absorb, viz., about five-eighths of the gross weight of the mixture. The weak points in the theoretical "principle" of this apparatus consists in overlooking the *perfectly dry gaseous nature of steam* while retaining the elastic force due to its temperature, and in applying to a mixture of air and moisture the physical laws applicable to saturated steam.

It must not be supposed that I am disparaging the real efficiency of the apparatus *quantum valeat*—that is as a *dry heat* apparatus. I think it an improvement on those of Dr. Esse's and Dr. Henry, but the quality and action of the heat is precisely the same as in these or any other dry heat appliance where dry heat alone is employed. The penetration of heat will be quicker, because of the pressure, and that is all. But it will lead to a fallacious security if the alleged property of *moist* heat is made the ground for materially shortening the process. I say so much because I am told by an official having charge of the erection and experience of the working of one of these machines, that half-an-hour was a customary period to give to one operation, but that ten minutes would really suffice. Against that belief, and against operations conducted under that belief, I enter a protest and give warning. The duration of exposure should be nearly the same as in any dry air apparatus of the same temperature. Dr. P. Bate, of London, thinks well of this machine after having seen it in action, admitting that "it has the drawback of being more costly than others, together with the necessity of requiring an able and experienced attendant," but he adds, "the rapidity and certainty of a sufficient heat are a sufficient compensation." With regard to the "rapidity," I must lodge this caveat that a furnace must be got into action and a steam boiler be in full delivery before the disinfecting process can be commenced. I had it in view to make an *experimentum crucis* to settle beyond any cavil all question as to the alleged moisture of the heat under which it operates, and with that object I have on two occasions made visits to Belvidere Hospital, in the suburbs, where one of these machines is in course of erection, but it is not yet in readiness, and my intended experiment is delayed.

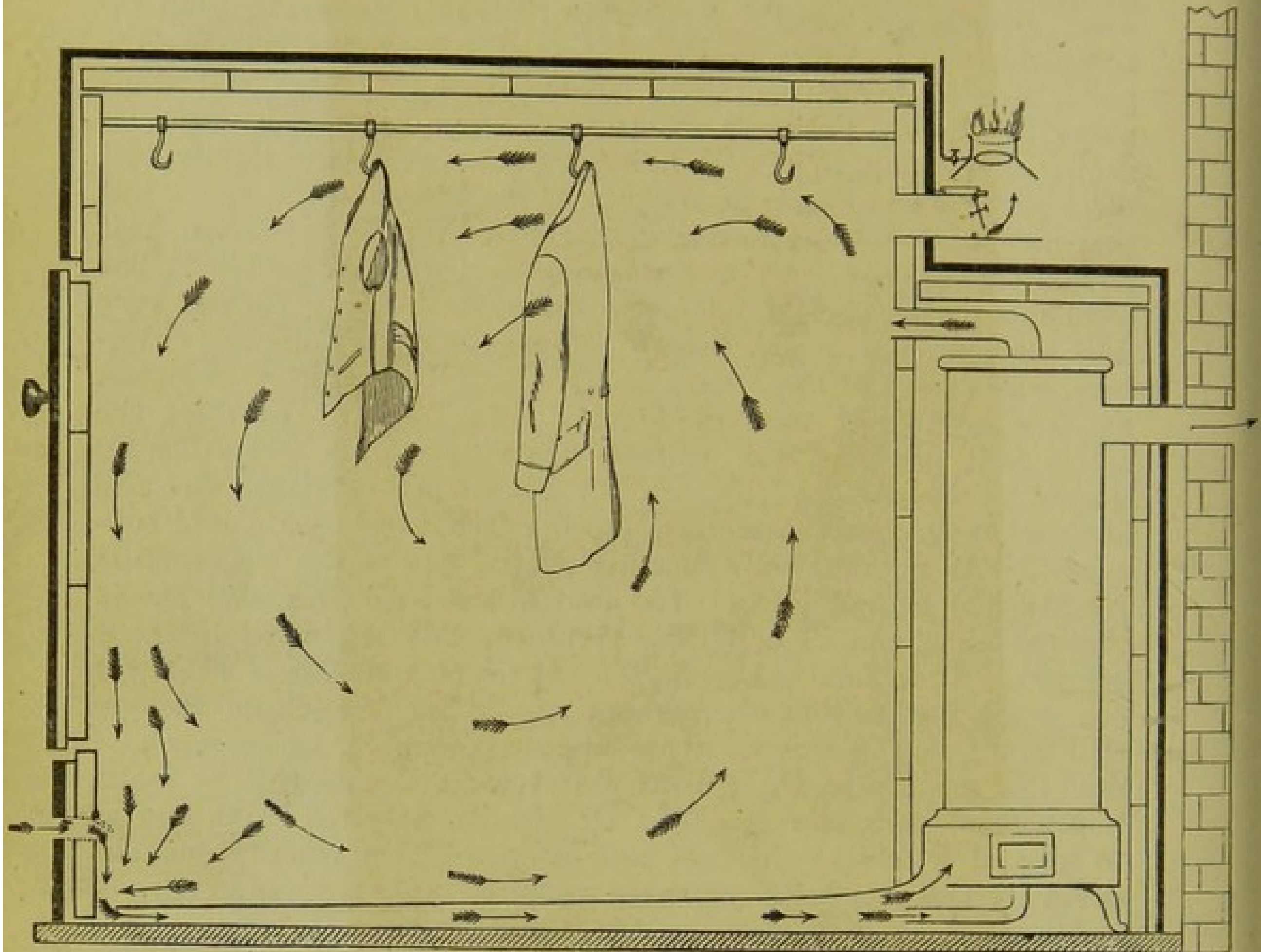

DIAGRAM SECTION OF DR. ADAMS' DISINFECTING CHAMBER.

Deaths. 20·XII·99

ADAMS.—At 10 Queen's Crescent, W. (the residence of his son, Dr Fred. V. Adams), James Adams, sen., M.D., F.F.P.S.G., aged 82 years.

Glasgow Herald

20·XII·99

DEATH OF DR JAMES ADAMS.—We greatly regret to have to intimate the death of Dr James Adams. The sad event took place last night at the residence of his son, Dr Frederick Adams, of 10 Queen's Crescent. Dr Adams had reached the advanced age of 82 years, and was the oldest member of the Faculty in Glasgow. His last illness began ten days since. James Adams was a native of Edinburgh. His father was Dr Alex. Adams, a well-known practitioner in his day, and his two brothers also belonged to the medical profession. He was educated at the University of Edinburgh, where he graduated in 1841. He did not practise in the city of learning, but was for a short time assistant to Sir William Ferguson. Removing to Glasgow in 1842, he began and continued his medical career until ten years ago, when he retired owing to deafness, although otherwise he was abundantly able to continue his professional pursuits. He had a large practice as a consulting physician. For fifty years or more he resided in Cambridge Street in a self-contained house, the only one of its kind in what is now an extremely busy thoroughfare. He was examiner to the Faculty of Physicians and Surgeons, president of the Medico-Chirurgical Society, and president of the Glasgow Medical Mission Society. He was also the author of a number of treatises on medical subjects. In conjunction with the late Professor Penny, he was mainly instrumental in detecting the presence of aconite in the victims of the notorious Dr Pritchard. Going back to the cholera epidemic in Glasgow half a century ago, he was entrusted with the supervision of one of the large tents for patients erected on the Green. He enjoyed throughout life the respect and confidence of the profession. But Dr Adams interested himself in subjects not included in the medical horizon. He studied and wrote a good deal on the heating power of coal gas, and his experiments on the subject led to a practical issue. After his retirement from active life, he was a frequent contributor to the "Herald," several of his communications on Burns being afterwards reprinted in collected form. He was a man of great personal charm —sincere, kindly, and always ready to assist others. All the same, he was a man of strong character, and on subjects of importance he held clear, decided views, and was not slow to uphold them. He was indeed a fighting man, yet always a gentleman. In politics he was a pronounced Gladstonian until the introduction of the Home Rule Bill. Even after this time he did not expressly renounce his faith in the author of that unfortunate measure. The passing away of this grand old man of the medical profession is mourned by three daughters and two sons, Dr James Adams and Dr Frederick Adams, who follow in their father's footsteps.