be judged by the same rule as respects their treatment. So, also, putrid diarrhoea, communicable by water, and bilious diarrhoea (or English cholera), widely as their sources, etiology, and *ratio medendi* differ, are riddled through the same sieve of interrogation with unmixed algide cholera; and from the general success or non-success attending the medication of the heap, composed of all three without distinction, it is attempted to deduce a rule of treatment applicable to all. To say more on this subject might be deemed presumptuous; but we believe that this attempt of the "Treatment Committee" of the medical council, to deal statistically with the materials submitted to them, will only be remembered as a failure.

There is yet one point more we would shortly advert to in connection with these reports, viz., the desire expressed for more information respecting the treatment of the consecutive fever of cholera. We would suggest that it must needs be extremely difficult to collect statistics of any real value on such a subject, unless, along with the enquiries issued, certain principles be stated, which it is desirable to prove or disprove, by the investigation. If it be true as we have indicated, that cholera is in its essence a congestive disease of quotidian type, then is it only one degree removed from remittent fever; and of this type consequently will be its consecutive fever. Such has always been the case within our own observation; and from long and varied experience we may venture to assert, that wherever cholera is present, or in the neighbourhood, every kind of fever partakes more or less of the quotidian congestive type of remittent fever: witness especially the congestive scarlatina of late years, and the black-tongued typhoid. We would add that we have invariably found mercurials aggravate the congestive character of all these fevers; while salines, combined with minute doses of quinine and sulphate of iron, in general fulfil satisfactorily every indication.

Should the attempt to investigate statistically the best plan of treatment ever be renewed, we shall hope to see a decided line drawn between diarrhoea and cholera, and between those cases in which the algide symptoms are primary, and those in which they are consecutive or superimposed.

Lectures

ON CERTAIN VIEWS ON THE NATURE AND TREATMENT OF PHTHISIS PULMONALIS.

DELIVERED AT THE

BROMPTON HOSPITAL FOR CONSUMPTION AND DISEASES OF THE CHEST, IN THE SUMMER OF 1856.

By EDWARD SMITH, M.D., LL.B., B.A., Licentiate of the Royal College of Physicians; Assistant-Physician to the Hospital, etc.

LECTURE III.

TREATMENT FOR THE ARREST OF THE DISEASE; DIRECTLY, BY VOLUNTARY DISTENSION OF THE AIR-VESELICLES; AND INDIRECTLY THROUGH THE GENERAL SYSTEM. EFFECT OF SLEEP, FOOD, TEMPERATURE, TONICS, COLD BATHING, AND ATHLETIC EXERCISES.

In the last, and also in a part of the first lecture, we were occupied with the consideration of the two great processes whereby life and health are maintained, and with which both health and disease must be connected, viz., alimentation and aeration, as causative of phthisis. We arrived at the conclusion that alimentation is not at fault, since the quantity of food taken is equal to that in health; the digestion is good, and the waste of material is not greater than in health: and hence we believed that that subject might in future be altogether excluded from the inquiry. In reference to aeration, we found a marked diminution in that process as a condition both anterior and subsequent to the deposition of tubercle, and ascribed it essentially to a lessened action of the air-vesicles of the lung, originating in themselves, or conjoined with an atonic condition of the general nervous system; and we proved that all the circumstances considered as causative of phthisis are of a depressing nature. The influence of age and sex was shown to be rather that of the circumstances attending the patient, than any peculiarity of organisation dependent upon them. This condition, it was believed, was always anterior to the deposition of tubercle, and ought to be considered as the first stage of phthisis. The deposition of tubercle was accounted for by regarding the inactive cell as a proper nidus for diseased material, and the tubercle as

a degeneration of a natural occupant of the cell, or the epithelial scales. The extension of it was believed to be due both to the severity and extent of the first causes, and also to the pressure mechanically produced on the adjoining cells by the deposited tubercle. We now proceed to make some deductions from the preceding inquiry, in reference to the treatment of the disease.

The plan of treatment which I venture to recommend is based upon the view which I take of the essential nature of the disease; the latter being a diminution in the action of the air-vesicles of the lung, and the treatment consisting in the increase of that action. Whatever may be the various modes by which the disease is to be combated, it appears to me that this leading idea must be kept in view, and that all other objects are to be considered as second and subservient to it. It is not my plan to enter upon the whole subject of the treatment of phthisis, but to select such parts as have reference to this theory and to my own investigations. The treatment will naturally divide itself into two parts: 1. The arrest of the disease; and 2. The removal of the effects of the disease.

1. *Arrest of the Disease.* This treatment is applicable to every stage, but manifestly much more to the early than to the later stages. It is not too early to apply it before there is any evidence of tubercular deposition; for, as there is a condition anterior to the deposition, upon which the subsequent deposition depended, it is surely of the highest moment to direct our attention to that early period. I think it a serious mistake to so far limit our view of phthisis as to consider it to begin when tubercle is first deposited; for by so doing we have permitted a period to pass over when the difficulty of arresting the disease is comparatively little, and have allowed our patient to pass into a state which is so often irremediable. Whenever, then, there is lessened respiratory murmur at the apex of the lung, with shorter and more feeble inspiration, and, as a consequence, some degree of flattening above or directly below the clavicles, it is high time to apply our remedial measures. It is probable that even then, and before any tubercle is deposited, there will be found diminished resonance on percussion; but this will be due to the absence of the air which should cause the resonance, and not to the deposition of any substance foreign to, or of a denser nature than, the lung-tissue. This slightly lessened resonance may readily escape the notice of those whose ears have been practised only in the flat sounds which indicate the total absence of air and the presence of a dense material; and, moreover, as the term dulness is a relative one, it is necessary that the examiner should be able readily to appreciate the sound in health. This I conceive to be less frequently attained than we are led to imagine; for the pupil is taught auscultation almost exclusively on the diseased, and not on the healthy chest; and, further, of those patients who are not suffering from apparent chest-disease, a large proportion have the lessened vesicular murmur to which attention is now directed. Hence there are two sources of fallacy, viz., the frequency of lessened resonance in persons apparently healthy, and the inability to detect slight variations in the degree of resonance. To this may probably be added a physical incompetency in some persons; for nothing is more certain than that the keenness of all the senses, and consequently of hearing, varies much in different individuals. I have ventured to make these remarks, both because I have seen errors made, and because errors may easily be made; and it is of importance to give close and careful attention to the character of the resonance. Whenever, then, there is the slightest dulness on percussion, with lessened vesicular murmur, less forcible and deep inspiration, and flattening of the apex of the lung, there is probably the first condition of phthisis, and the stage anterior to the deposit of tubercle: and, since the only change existing is atrophy with lessened use of the air-vesicles, it is manifest that the arrest of the disease may be more readily effected, than when it is complicated by the addition of further adverse conditions.

It is, however, probable that the tendency to the filling up of the effete cells exists coincidently with the diminution of vital action in those cells; but a period must elapse before the vesicles can be filled with any substance, and much more with so resisting a material as the tubercle which is evident to the naked eye; and still more so, as it is probable that the tubercle is a result of the degeneration of the accumulated natural product—the epithelial scales. How long this period may be, we cannot tell; but, from the length of time during which persons appear to be prone to, without actually falling into, phthisis, we may infer that it is commonly long. That must, however, depend much upon the intensity of the causes which

induced the atrophy of the cells, the power of resistance existing in the constitution, and the circumstances in which the individual is placed; but it is fair to infer that the duration of the disease before tubercle is deposited so largely as to be unmistakably present, is long enough to permit of the use of remedies.

Now, are there grounds for believing that the deposit of tubercle interferes materially with the rapidity of the progress of the case; and hence that the deposition adds seriously to the gravity of the case? I think that there are grounds for this belief; for it would accord with all experience that the duration of the case after tubercle is cognisable is far less than that of the evident proneness to phthisis which is truly the condition prior to tubercle. Tubercle may undoubtedly remain in the lungs for many years, and not induce any evident disposition to its increase; but, notwithstanding the almost universality of this deposit, such is not its ordinary state. I have no doubt but that, as a rule, when tubercle is really deposited, it promotes in an increasing ratio the liability to further deposits; and not merely that it is coincident with a state of things which induced this increasing tendency to further deposition. It is in itself a cause of further deposit. The proof of this, I think, is found in the greater rapidity of the progress after tubercle is manifested: and the explanation is in the pressure which the distended cells and lobules exercise upon the adjoining lobules, and consequently the interruption to the action of the air-vesicles which is thus induced. Thus, to the general cause inducing the atrophy of the cells, must now be added the ever present and acting one of a local kind. As the tubercle is deposited in various parts at the same period, so will this local action, *ceteris paribus*, be more or less effective for the rapidity of the progress of the phthisis.

Now, so far we may assume that a large portion of the lung is competent for its duty, and that the general system has not greatly suffered from the reaction of the disease, however much it may be injured by or with the condition which first gave rise to the earliest conditions of phthisis. The attempts at arrest may hence have a more single aim than could be the case at a later period, and the hope of success be so much the greater.

What, then, are the plans of treatment which seem the most fitted to bring about this end? The theory propounded is, that the disease essentially consists in the lessened action of the air-vesicles, and that it is commonly due to anterior conditions of the general system of a depressing nature. These general conditions are in part, probably, certain atonic states of the nerves of organic life, and more particularly of the sets of those nerves and of the communicating branches of the cerebro-spinal system which preside over the involuntary and also the voluntary action of the lungs. Hence the remedy would be some direct mode of preventing the further decrease of action and the closure of the cells, as well also as the indirect one of action upon the general system. Hitherto the latter has been the one more commonly practised (although not on the theory referred to), and it has the merit of being based upon good physiological reasoning; but, as we too constantly prove, its action is too slow to be usually efficacious, and consequently it is uncertain and often ineffective.

a. The *direct mode* to which I refer is that of mechanical distension of the air-cells to a degree beyond that which takes place perhaps in health, but certainly in the state of enfeebled respiration in which we find the patient. This may be effected by bodily exertion, which tends directly to increase the frequency and the depth of inspiration; and, as this mode is so consonant with our knowledge of the laws whereby health is maintained, no objection will be urged to it. But to my mind there is the objection that, in phthisis, whether before the manifest deposition of tubercle, or afterwards in the early stages of the disease, the pulse is frequent proportionately to the respiration; so that the respiration is to the pulse, not as 1 to 4, but as 1 to 5, 6, or 8. I have paid much attention to this matter in a long inquiry which I have prosecuted at this hospital, and am assured that, in the early stage of phthisis, the proportion of the two functions is commonly reduced. Now, the pulsation is at least frequent enough, and it is not uncommonly too frequent; and hence we do not need to apply any remedies which may increase the rapidity of the blood-current. But exercise of body, and even the sitting and standing postures, do increase the blood-motion; and, although they at the same time increase the rapidity of breathing, they do it in a less ratio than the former. Whilst, therefore, bodily exertion may be useful, and is indeed necessary in giving more rapidity and depth to the inspiratory effort, it is not an unmixed good. But we must not forget that the quiet motion of the body,

which is now said to be bodily exercise, does not excite the depth of inspiration sufficiently for our purpose; and it is only when it becomes so great as is needful in athletic exercises that the desired result is attained. Hence the directions which we commonly give are of little avail, although the tendency of them is right. Yet, with the violent bodily exertion referred to, the rapidity of the blood-current is greatly increased, and at the same time there is a proportionate diminution in the deposition of material in the tissues, and in the due action of the air upon the blood in the lungs. Thus lessened growth of body occurs, with, at the same time, less vigour of vital processes, and a waste of material through the eliminating organs. This must result when the body is in health; but then the temporary evil is either easily borne, or is compensated by good; but when, in phthisis, at least in the tubercular stages, we find a tendency to a constant rapidity of current, and consequently to lessened growth of tissues, we must attach a greater degree of importance to it. The effect of much exercise in phthisis is, therefore, evil certainly, although, at the same time, it may be, but less certainly, good.

Now, is it possible to meet this difficulty, and to find a mode whereby the depth of the inspiratory act shall be increased, and yet the rapidity of the blood-current not sensibly promoted? Perhaps not, in the fullest sense of the inquiry; but I think it may in a limited yet important sense. I refer to voluntary attempts at deep inspiration. This cannot be continually effected, since volition cannot be at all times directed to that end; and if it could, the very act would fatigue the system; but it may be for a limited period at a time: and the very instruction thus given, if properly explained, will induce the patient to guard against that shallow respiration which is so constant a feature of the complaint. Thus the mind would be directed to an object of value; the spirits would be excited by hope; and the evils attending a listless and enfeebled habit of respiration would be in some degree guarded against. This object is doubtless attempted when the patient is directed to use calisthenic exercises, as the use of the dumb-bells; and there cannot be a doubt that the vigorous employment of such means may excite inspiration. But it is one thing to throw the arms about, and another to make that conducive to the deep inspiration. We must admit that, whilst the object is good, the practice has commonly defeated the object, and that perhaps in a great degree from the want of knowledge on the part of the patient to enable him to make his efforts efficient. Moreover, I am not clear but that sometimes, and perhaps frequently, the effort now referred to lessens the frequency, and without increasing the depth, of inspiration; for nothing is more common than for us to hold our breath when making any unusual voluntary exertion.

I think that nothing less than direct voluntary attempts to breathe deeply would effect the object which we have in view; and even this is certain to fail unless it be carefully effected. The seat of mischief is chiefly in the upper lobe and the apex of that lobe. Now, if we take an ordinary inspiration, we find that the expansion of the chest is disproportionately greater in the lower than in the upper half of the chest; and when the respiration is unusually feeble, this disproportion is so much the greater that scarcely any breath-motion may be detected under the clavicles. But, on deep inspiration, the first sensation of fulness is at the base of the lungs; and that sensation gradually rises as the depth of the effort increases, until, at the very end of the deepest inspiration, the sensation is felt at the apex. This may be readily proved by any one who will take the trouble to try it carefully upon himself. Now, in this very fact lies the difficulty of the matter. It is almost impossible to persuade a phthisical patient to take an inspiration of the depth referred to; for his habitual shallowness of effort induces him to consider that a deep inspiration, in which the lung is by no means fully distended. It is my habit to show the mode and the required depth by my own inspiration, and to inform them that it is only the *very end* of the deep inspiration which is of service to them. Our aim should therefore be to have the deepest inspiration performed as often as we think right, with a view of thus preventing the process of closure, which is, in my belief, the mode of action of the disease. If there were not a serious objection to the introduction of any instrument as a part of medical treatment, I should advise the employment of a spirometer, which would measure the amount of air inspired; and this, whilst engaging the patient's attention, would enable him to regulate his voluntary efforts, and to ascertain the result. I have several in use; and, after a repeated employment of them in determining the amount of vital capacity at various periods, the patients have expressed much gratification in the

assurance that they felt much better from this forcible attempt to inspire deeply.

I fear that this may be thought too mechanical a plan of treatment; but I beg to observe, that the very existence of the air-cells themselves is in part due to the mechanical introduction of air within them. There are no developed air-cells in uterine existence; and even during the first early period of extra-uterine life they are so slightly developed as to be said not to exist at all. When the air is first admitted into the bronchi, there are no true cells such as may be found in later life; and the period of their development is that of breathing, and their maturity is due to the continuance of the effort. Thus the development of the air-cells may be said to be due to the mechanical agency of inspiration. Moreover, we know how greatly the depth of inspiration is due to volition, to the thousand necessary occurrences of daily life, and to the effect of other diseases; and we admit at once that the effort of inspiration varies under these several conditions. Hence it is not unphysiological to direct an effort to make the act of inspiration perfect (as we daily do to render the digestion of food perfect), and to keep in a due state of distension, or to increase the existing degree of distension, of the air-cells of the lung.

I do not know if any difficulty would present itself to any mind in reference to the limitation of the lessened action, or of collapse to isolated small portions of the lungs, as is believed to exist on this theory. Perhaps it is more easy to understand how the whole organ may be influenced, rather than a part of it; but, in addition to the special disposition which must exist in the upper lobe, and especially of the apex, from the direction and depth of the air-current in inspiration, I may refer to the fact that the atelectasis of the newly born is always partial, and may be even limited to one or to several isolated and separated lobules. Hence it may be said to act only on individual cells, and is a fair illustration of that which is believed to exist in the earliest stage of phthisis. The one is not more difficult of belief than the other.

To show that voluntary inspiration not only may, but has been defended on physiological grounds, I would refer to a remark made by Lehmann, vol. iii, p. 382. In reference to excretion of carbonic acid largely, he says: "We may perhaps aid a tuberculous patient quite as much by recommending him to respire warm moist air, as if we prescribed lichen or cod-liver oil. Instead of tormenting an emphysematous patient suffering from congestion, and of hemorrhoidal tendencies, with aperients and saline mineral waters, we might relieve him far more effectually by recommending him to practise artificial augmentation or expansion of the chest in respiration (filling the lungs several times in the course of an hour), or to take exercise suited to produce this result; while we should forbid the use of spirituous drinks, and not prescribe tinctures, which might hinder the necessary excretion of carbonic acid."

In advising this course, I do not for a moment refer to any increased chemical influence which the increased volume of air may or may not have upon the blood; neither do I make use of the theory that, by this means, we effect pressure upon tubercle, and promote its absorption; I only claim for it, that it will tend to prevent the decay and the closure of the cells from inaction, and thus prevent the further deposition of tubercle in cells which are not already rendered useless by or with it. But it is fair to infer that there must be by this means a more complete renewal of the residual air, and thereby a further benefit be obtained. It may, however, be proper for me to refer to the experiments of Vierordt in reference to the influence of voluntary respiration in promoting the evolution of carbonic acid. He ascertained that the more frequent the respiration, the less per centage of carbonic acid was evolved; but, as the total quantity of air taken into the lungs was increased by an increased number of inspirations of an uniform depth, the total quantity of carbonic acid evolved in a given time was greater than with fewer inspirations. Thus—

With 12 inspirations per minute, 13½ cubic ins. were evolved.

" 24 " " " 24.2 " "

" 48 " " " 42.5 " "

And, in reference to variation of depth, the frequency being constant, he proved that, with an inspiration twice as deep, the quantity of gas evolved was the same as when the inspirations were three times as frequent, the depth then being constant. Thus the objection which is so commonly raised to voluntary attempts to respire, viz., that it does not increase the vital force, is incorrect; for, in practice, we are not concerned with the percentage evolution of carbonic acid, but with the total evolution in a given period.

The reason for the large increase in the amount evolved by

an inspiration simply twice as deep as an ordinary one, is, that the air in the air-cells is richer in carbonic acid than that in the minute bronchial tubes, in the proportion of 5 to 3; and hence, as a deeper inspiration causes more movement in and exchange of the residual air, the air-cells must lose a larger quantity of the products of respiration. Hence the remedial influence of deep voluntary inspiration is both mechanical and chemical.

The effort now recommended may weary the patient; and hence I have thought it enough if the patient thus deeply, slowly, and gently respire for five minutes at a time, and on three or four occasions in the day, at the same time explaining the object, and recommending him to avoid shallow breathing in his ordinary respiration. Thus fatigue is avoided, and yet probably the effect is obtained.

It is, however, essential to the success of this plan that it should be fairly carried out: and if, from other causes, no success results, I do not know of any mischief which could possibly arise from this. Success will of course be dependent upon many causes, and hence neither this nor any other single plan of treatment can be exclusively relied upon. It has, however, this merit, that it is of almost universal application, has evidently a tendency to improve the health, and cannot do harm. When there is no tubercle deposited, I am of opinion that the plan, if fairly carried out, can hardly be inefficacious; but, in the last stage of phthisis, the possibility of arresting the disease by any means is very small.

b. Indirect means. In these also the aim should be to increase the respiratory function; and there are several which may be proved to exert this influence. In the investigations at this hospital to which I have already referred, several of these influences were under consideration; but the results of the inquiry were as to the frequency, and not as to the depth of respiration. It is to the latter that I wish to direct attention more particularly; and for that purpose I shall detail the results both of that inquiry and of one on the depth of ordinary inspiration now in progress.

Sleep was the most powerful cause of increased frequency of respiration in this disease, contrary to the occurrence in health. Thus I found that a sudden increase of eight or ten respirations per minute occurred from sleep, with a corresponding decrease on again waking; but the amount of the increase depended apparently on the soundness of the sleep, and consequently it varied with the hour of the night. As a rule, it was the greatest in the first hours of sleep; but, in instances of second sleep, it was as great at 6 or 7 A.M. as at 11 P.M., although in the interval the frequency was greatly diminished. A similar increase occurred in the occasional hours of day sleep; and hence it appears that the cultivation of sleep in many phthisical patients would greatly tend to increase the amount of respiration. This influence has also the great merit of not increasing the pulsation; for the rule was, that sleep lowered the pulse and increased the breathing. Its employment, therefore, is much to be commended.

Fig. 1.—This diagram represents the rate of respiration in each of twenty-eight consecutive hours in two patients, male and female, in the Hospital for Consumption, etc., at Brompton. The hours are placed at the top, and the scale of rate at the side of the diagram. Each column therefore records one hour; and where the letter S. is placed, it signifies that the patient was then asleep, and $\frac{1}{2}$ S. signifies half asleep. It is thus seen that in both cases the rate increased greatly with the first hours of sleep (11 and 12 P.M.), and in both it then fell. But the case represented by the dotted line had a further increase at 5 A.M., shewing the influence of second sleep. In the latter case more particularly there was a great fall in the rate with the first waking hour of the morning (6 A.M.). Throughout the day, the rate was much lower than during the night, and especially before meals. (Extracted from Medico-Chirurgical Transactions, 1856.)

Early retiring to rest, and consequently *early rising*, are of great moment. As the evening advances, both the functions of respiration and pulsation are greatly reduced; and hence any

bodily or mental exertion, or even mere consciousness, must be attended with unusual wear and tear. In the morning, after a long night's rest, the respiration has greatly fallen, and then attains the minimum of the whole twenty-four hours; and hence the stimulating influences of food, exercise, and excitement are then imperatively called for.

Fig. 2.—This diagram represents the result of 3,000 observations on twelve phthisical patients in the hospital on each day (except Sunday) for one month, in May and June; and shows the relation of the rate of pulsation and respiration to temperature. The lower line (temp. ext.) records the average daily temperature at Greenwich. The second line (temp. int.) records the same fact as observed in the hospital wards. The third line (puls.) gives the average daily return of the rate of pulsation in the three postures combined; and hence to obtain the average, it is needful to divide the recorded quantities by three. The fourth line (resp.) indicates the same facts in relation to respiration; but as the totals of the three postures, and at two periods of the day, are given in order to shew the variations more perfectly, the totals must be divided by six. The column for the Sunday is used to represent the averages of the preceding week as it respects pulsation and internal temperature. On comparing the course of the lines of pulsation and respiration with that of temperature, it will be seen that as the temperature rose the pulsation rose and the respiration fell; and in three of the four weeks the returns of Monday were the reverse of those of the Saturday. (Extracted from Brit. and For. Med. Chirurg. Review, April and July, 1856.)

Food also powerfully excites respiration; and, although it at the same time excites pulsation, it acts upon the former in a greater ratio. The effect is, however, temporary; and hence frequent meals must be indicated. With regard to the kind of food which excites respiration the most, it appears that tea or coffee is the most powerful; but animal food is also extremely efficacious. The administration of food, therefore, in the hours preceding the night sleep, is very valuable, as also in

the early morning; for at the latter, and then at the former periods, the frequency is at or near the maximum.*

The effect of increased temperature of the air is to lessen the frequency of respiration as the heat increases; and the same is also true of the influence of dryness of the air. Vierordt's inquiries are again *à propos* to our subject; for he ascertained that both increased temperature and dryness of the air lessened the amount of carbonic acid evolved; whilst, in a moist atmosphere and a moderate temperature, the evolution was at its maximum. Hence a moderately cool and moist atmosphere is the most beneficial in phthisis, and is a most important desideratum in institutions such as this, in which an attempt is made to obtain uniformity of temperature by artificial means. In the inquiries before referred to, I investigated both these circumstances with care, and found that it was almost impossible to obtain an atmosphere sufficiently moist when it was subject to artificial heat. The difference between the dry and wet bulb thermometers indicated the same degree of moisture as that which was manifested in the external temperature on the average of the whole quarter, but a much less degree than was at that time contained in the external air. This also, both within and without a hospital, must be influenced by the season of the year. Both of the former conditions, viz., high temperature and dryness, also induce more frequent pulsation—a circumstance which adds to their evil influence. (See diagram opposite.)

A fair flow of spirits, and moderate excitement of both mind and body, must doubtless increase respiration; and hence variety of scenery and changes of a pleasant character must be proper.

The influence of posture of body over the respiration may be well seen by the following results of an investigation in which the lying posture succeeded to the standing, and that again was succeeded by the standing. After standing upwards of an hour, the respirations were—

	No.	Cubic in. each.	Cub. in. per min.
Standing . . .	19.4	20.3	395
Lying . . .	16	19.5	312½
Standing . . .	18.8	19.2	361

So that the standing posture increases the frequency of respiration, and the quantity of air admitted into the lungs; the depth remaining much the same. Very gentle exercise, such as could be obtained by walking to and fro for a few feet, had marked influence. Thus, as to the period when all influences are less potent, the respirations were—

	No.	Cubic in. each.	Cub. in. per min.
Sitting . . .	17.2	27.3	470
Standing . . .	16.6	29.1	484
Walking . . .	17.3	36	625

And in another case, the quantity per minute was—

Sitting, 470; standing, 471; and walking, 612 and 640.

Thus the effect of this gentle exercise was to increase the quantity of inspired air fully one-third more than the quantity breathed in the standing posture, and also to increase the depth or extent of inspiration. The quantity of air inspired differs however according to the rate of walking; and at the rate of four miles per hour, the quantity is about 2,000 cubic inches per minute.

In reference to the influence of medicinal agents, I think there is reason to believe that tonics of every kind act in the mode under discussion. It is quite certain that their tendency

* On a large average in both sexes in phthisis, I found that the frequency of respiration was increased by breakfast 1½, by dinner 3, and by tea 2½ respirations per minute.

The effect on myself from one cup of tea on an empty stomach was to increase the respiration as follows:—

	No.	Cubic in. each.	Cub. in. per hour.
Before the tea	18.5	21.2	393.5
After (maximum within 55 min.)	20.6	21.9	434
That of a moderate dinner at 6 P.M. was—			
Before dinner	16.8	23	387
After (maximum within 90 min.)	21.6	22.3	456

Thus in health, although the quantity of air introduced with each inspiration is not increased, but rather lessened, by food, the frequency of respiration and the total quantity of air inspired per minute, are greatly increased. Wine has a contrary result, for although it may slightly perhaps increase the frequency of respiration, it lessens the quantity of air inspired; so that in my own case, at 5½ P.M., after a fast of four hours, one glass of pale sherry reduced the quantity within twenty-eight minutes from 23 to 20 cubic inches per inspiration, and from 442 to 402 cubic inches per minute, or a reduction within a few minutes of one-eighth of the total volume. Should these results be confirmed by the further investigations now in progress, it would raise a very serious question as to the propriety of administering such fluids in phthisis: but it has long been known that spirituous liquors decrease the quantity of carbonic acid evolved, a circumstance opposed to their employment in this disease.

is to improve the health by giving more strength to the system; and since, as the health improves, the ratio between the pulse and breathing is increased, without any great change in the rate of pulsation, it is fair to infer that the increased ratio is due to the increase of the rate of respiration.

The employment of *cold baths*, properly administered, is doubtless of use, by causing deeper inspirations; for we all know that this is the ordinary effect of the sudden application of cold. It is, however, essential that the application should not occupy too much time, lest the continued exposure should lower the temperature of the body considerably, and with it the nervous power; and thus induce a state of exhaustion which the application was intended to avert. It is, therefore, needful that the patient should be limited to one or two plunges, or to a rapid sponging of the whole body.*

Such, then, are some of the most powerful positive means within our reach for the arrest of phthisis; and they may be summed up in the following sentence:—Forced inspirations, out of door exercise, good and frequent food, sleep, early rising and retiring to rest, cool moist air, cold washing, moderate excitement of the mind, and medicinal tonics. There is also another, which may rather be considered a prophylactic of phthisis, and which, in my opinion, is of far greater value than the community at the present day admit; viz., athletic exercises, and country sports and games. One of the greatest evils attending upon the state of society at the present day is the entire absence of this healthful, yet more or less severe bodily and mental recreation. The attempts to lessen this want by encouraging country rambles is valuable, by enticing the individual from impure into pure air, by lightening his mind of pressing cares, and inspiring him by the sight of new scenes and objects; but it fails in the want of the rough athletic sports of former days, which directly increased the depth of respiration, and also in the very rarity of its occurrence. It appears to me that we cannot duly appreciate the prophylactic power of such amusements and exercises as were practised constantly by our forefathers, and that in the present day we can offer no efficient substitute for them. Now, without entering more fully into this question, if they may fairly be regarded as conducive to health, they would be preventive of phthisis; and if preventive, then they would be of service in the early stage of the disease. The encouragement of such exercises as cricket, ball-playing, and rackets, for males, and the skipping-rope and hoop for females, cannot fail to be a step, however small, in the right direction.

The negative means to be employed for the end proposed are doubtless as important as the positive ones just described; for we thus strive for the absence of those very influences which, when present, gave rise to the disease. I need not enter into this part of the subject with any minuteness, and shall therefore simply enumerate them. Such are the prevention of abuse of the body, as by excessive repletion, whether by solids or intoxicating fluids; by sexual excesses and masturbation; by too long continued labour; by the absence of impure, hot, and dry air; great anxiety; undue exposure to cold, and any other depressing cause; also by the correction of indolence of body and

mind, and the due supply of nutriment in quality, quantity, and frequency; with the correction of functional derangement of the alimentary and excretory organs. The removal of these and similar conditions must be at least concomitant with the employment of the more positive agencies before described; but, as so many of them are inextricably mingled with the necessities or the vices of mankind, it must be that their influence upon society in the production of phthisis can never be eradicated. To these must also be added another, and that perhaps the most widely spread and the least remediable of all these agencies; viz., the defective system handed down from father and mother to child—the result of accumulating vices of life, and of injudicious marriages. With all these circumstances opposed to the physician, one cannot wonder at both the frequency and the pertinacity of the disease; and so long as mankind are exposed, or do expose themselves, to the action of so many depressing agencies, so long must the vital energies be reduced in vigour, and the lungs, from their very structure, be the most constantly subjected to the results of deficient respiration.

Reviews and Notices.

ON POISONING BY STRYCHNIA; with Comments on the Medical Evidence given at the Trial of William Palmer for the Murder of John Parsons Cook. By ALFRED S. TAYLOR, M.D., F.R.S., Lecturer on Medical Jurisprudence and Chemistry in Guy's Hospital. pp. 152. London: Longmans. 1856.

The trial of William Palmer must ever hold a prominent position among the *causes célèbres* of medical jurisprudence; not as a triumph of chemical skill, for chemistry, whatever may be said, proved nothing on either side; but as opening the question whether pathological symptoms can be justly taken as evidence of poisoning, in the absence of the positive proofs which an examination in the laboratory might be expected to afford. The excitement attendant on the progress of the case having in a great degree passed away, scientific men are now in a position to examine the question carefully, and to determine more accurately how far the modes of chemical analysis with which we are acquainted, are sufficient for the detection of poisons, and how far we must be content, in some cases, to recognise as our only guide the evidence presented by symptoms. To declare his opinion on these points, with special regard to strychnia, is the object which Dr. TAYLOR has in view.

After giving an outline of the circumstances connected with the death of Cook, Dr. Taylor proceeds to examine in order certain prominent questions contested at the trial. In support of his opinions on each question, he brings forward a large amount of authority, bearing both on the general subject and on the particular instance. This is indeed required in writing on a subject so full of doubts; and especially it is incumbent on Dr. Taylor, for the sake of science and justice, as well as for his own justification, to prove the correctness of his views by reference to the evidence afforded by other cases than the immediate subject of his remarks.

The first question which Dr. Taylor takes for consideration is:—

Were the symptoms and appearances in the case of Cook caused by strychnia, or might they have arisen from some natural disease?

This inquiry, it will be seen, is capable of being resolved into two parts; first, whether the symptoms and *post mortem* appearances were such as are commonly known to attend strychnia poisoning; second, whether, supposing this to be the case, they might not so closely resemble those of natural disease as to be undistinguishable?

With regard to the first part of the inquiry, Dr. Taylor adduces the evidence afforded by three undoubted cases of poisoning by strychnine; and, comparing them with the case of Cook, he infers that the symptoms were sufficiently alike to justify the opinion that they arose from a similar cause. In

* As evidence of the precise effect on the inspiration, etc., in health due to cold bathing, I offer the following results, from an experiment made upon myself:—

	No. of resp. in a minute.	Cub. inches evolved in each resp.	Cub. inches per minute.	Pulse.	Temperature of Breath.	Temperature of Air.
At 10.18 P.M., 4½ hours after dinner, 2 hours after coffee, while sitting by the bath.....	14.6	31.4	459	77	—	61.5
During first five minutes after complete and quiet immersion in water at 62°.....	16.2	33.8	542	80	5.5	61.5
Twenty-six minutes after immersion, with sensation of great cold and shivering.....	20.2	37.8	764	76	6	61.5
After lying in bed 40 minutes, and still feeling very cold.....	15	27.6	415	69	5	64
I then took a glass of moderately warm weak brandy and water; and in 50 minutes after leaving the bath.....	14.4	27.8	401	76	4.5	64
In ten minutes later, still feeling very cold, although buried in blankets.....	14.8	27.5	407	74	3.75	64

For some hours afterwards, I was unable to feel warm. Thus the effects were most remarkable in increasing the frequency of respiration and the quantity of air inspired per minute, and in lowering the temperature of the breath and that of the body to an extent probably very injurious to health.