

CONCLUSIONS. Diaphragmatic hernia is much commoner than generally supposed, and may exist indefinitely without giving rise to symptoms. Dextrocardia and physical signs suggesting those of pneumothorax should always cause the possibility of this condition to be thought of and lead to careful radiographic study of the thorax.

**SOME CONSIDERATIONS REGARDING THE FACTOR OF
FATIGUE, WITH REFERENCE TO INDUSTRIAL
CONDITIONS.¹**

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WITHIN the past few years considerable literature has grown up with reference to the general subject of fatigue, and more recently it has been sought to utilize the results from the investigation of fatigue in helping to solve some of the industrial and occupational problems. While the mass of literature about these subjects is so large as to make any effort toward reviewing it at this time a practical impossibility, yet from it all there stand out certain general principles to which it is well worth while to call attention. There are certain other fairly well-accepted facts which belong within this same sphere, but which as yet have not received the recognition which is their due, to which attention also may be invited.

In the first place the problem of fatigue has been most thoroughly worked over with reference to the subject of muscular fatigue and with the use of the nerve-muscle apparatus. Here the problem has been a relatively simple one, and yet even with all its simplicity there remains a variety of questions to be answered, and the undisputed conclusions are few. It is generally admitted, as a result of this kind of work, that fatigue has two factors, a negative and a positive: the former the result of an actual wearing out of the used substance, in this case muscle, and the second the result of the formation of certain poisonous substances such as carbon dioxide, paralactic acid, and monopotassium phosphate. The term exhaustion has been proposed for this negative side of fatigue, while the word fatigue itself is used solely by some authors to indicate the positive side.

These results are results of the fatigue of muscle. The question of the fatigue of the nervous system is an entirely different one. As soon as an effort was made to define the actual place where

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fatigue occurred in the living animal or the nerve muscle apparatus it was found to be no easy matter. In general, however, it may be said that the nerves themselves do not fatigue, and that the conception of the central nervous organs themselves showing fatigue, as is more particularly set forth by the experiments of Hodge, has changed materially of late years until it is now believed that the central nervous organs, like the nerves themselves, are little susceptible to fatigue. In the case of the nerve-muscle apparatus it would seem as if the nervous tissues were saved from fatigue by the fatigue of the muscle, or that both muscle and central nerve organs are saved from fatigue by fatigue of the motor end plates in the same way that a dynamo may be saved from damage by the melting of a fuse. The classical experiment of Sherrington, which was to the effect that a muscle which is connected with a spinal cord centre, which centre may be reached by several afferent tracts, responds as quickly to stimulation through one of these afferent tracts after one of the others has been so exhausted that no results follow its stimulation, indicates that the locale of the exhaustion is at the synapse between the afferent and efferent neuron, and that, again, the central nervous organs are protected from exhaustion by this synaptic exhaustion just as in the former case they were protected by motor end-plate exhaustion. If these are the mechanisms, then it is readily seen that neither the nerves nor the central nervous system itself can ever be seriously exhausted, that the synaptic junctions and the motor end plates interpose obstacles to the occurrence of any serious disintegrating processes, and therefore are protective devices for conserving the organism.

At this point one cannot resist drawing certain analogies. We have seen that it is fairly well conceded that the motor end plates and the synaptic junctions, by becoming impervious, prevent exhaustion of the central nervous system; so it is commonly accepted that in the mental sphere disorder of attention has the same conservative function. The person who can no longer pay attention is saved from the possibilities of serious exhaustion, and this conclusion is in harmony with the more recent belief, *contra* the original opinions following the experiments of Hodge, that the central nervous organs really do not suffer from exhaustion.

Again, mental fatigue is still more complex. We have no knowledge at all for correlating the symptoms of mental fatigue with any definite changes in the nervous system, and our means of measuring mental fatigue are most of them so complex when one comes to investigate them, and so contaminated by other than mental factors, that it is extremely difficult, in fact almost impossible, to evaluate the results. It is obvious, for example, that such instruments as the ergograph or the dynamometer, the esthesiometer, and the algesiometer all involve both mental and

physical factors, while when we come to take up the question of the symptoms of mental fatigue, we realize that defect of attention is one of them. Then when we realize that the whole attitude of psychology toward attention has been materially changing of recent years, and that attention is being more and more considered as an affective state, as an affective orientation of the individual as a whole toward a stimulus in the environment, the muscels naturally playing a large part, it becomes evident that the difficulties are certainly very great. In the face of these difficulties it would seem that we had better stick to fundamental principles and not mix our categories and so not endeavor to speak of fatigue in one category in terms of fatigue in another. Let the mental and the physical, or as symptoms, called the psychic and the physiologic, remain apart, and endeavor to determine questions of fatigue in the mental sphere purely in mental terms. For this purpose probably the best method is Kraepelin's method of continuous additions.

This much I have said preliminarily because questions of fatigue which arise in the consideration of industrial and occupational problems almost invariably imply fatigue both of mind and of body. It will be seen, thus, that in speaking of fatigue we are using a term of vague connotations, and dealing with a condition that admits of measurement only with the greatest of difficulty. Not only this, but in the present state of our knowledge it is practically impossible to state wherein the fatigue is resident, what part of the individual really is fatigued, and what are the mechanisms both chemical and physical of that fatigue. The general gross fact which seems to issue from this complex situation is that human beings, worked under given conditions, tend to show a gradual falling off in the efficiency of their work, and that this falling off in efficiency can be prevented by changing the conditions, more particularly by increasing the opportunities for rest; and that, further, when human beings continue to work under conditions which show a gradual falling off in efficiency, other manifestations tend to come into evidence—namely, various kinds and descriptions of disturbances of health. So that with our present knowledge it would seem more accurate, and perhaps wiser, to deal with the human being as if he were a machine and with his efficiency as measured by his output and endeavor to find what the conditions are which lower his efficiency, either impairing it temporarily or tending to impair it permanently, and then endeavoring to discover what the conditions are which will prevent this temporary or permanent impairment, and so increase the efficiency.

I have been tempted to say what I have said, which is more especially a plea for greater definiteness in the use of terms, by looking through the literature of fatigue, particularly in connection with various occupations, and noticing with what little regard

for accuracy the term fatigue and certain other terms are used. I refer more particularly to the conditions which are presumed to be the results of mental fatigue. I find numerous papers, some of them by talented writers, showing the prevalence and the increase of neurasthenia and hysteria in certain occupations; and I note the statistics, especially those of foreign sanatoria for the working classes, showing the immense increase in cases of nervous diseases that were admitted to these sanatoria during recent years. There appears to be little in any of this literature that at all adequately accounts for these conditions.

In the realm of the neuroses and the psychoneuroses, such particularly as neurasthenia and hysteria, to which I have called attention, the particular character of the work or its severity could by no possibility operate as adequate causes if our present ideas of these conditions are correct. Hysteria, for example, is a mental disease, dependent upon purely mental causes, in other words, psychogenic in origin. Work of any character, description, or degree of severity could not be conceived to be a cause in any true sense. It is well known that if we have some weak point in our bodies it hothers us more when we are not in good condition, and we are able to adjust ourselves to it better when we are in good health. The muscles of accommodation, for example, partake of the tone of the general musculature, and when health is good they may give little trouble, but when health is poor, they may cause difficulty. In the same way, and only in that way, can occupation or fatigue of any kind be said to be a causative factor of hysteria. It can only be an adjuvant cause at best, and at that, you will see, an unimportant one. With neurasthenia we are dealing with a condition which is not so prominently mental. In fact, neurasthenia, as we understand it today, is not a mental disorder at all, but a physical disorder. However, the term neurasthenia is perhaps one of the most loosely used terms in medicine, and as I see it through the literature that I have spoken of before, there appears to be no definiteness about its use. It is applied to all sorts and conditions of things, including the whole realm of the neuroses and the psychoneuroses, and probably some of the actual insanities. We have a fairly well-defined syndrome to which the term neurasthenia is applicable, the symptoms of which are, in the main, a feeling of pressure on the top of the head, more or less insomnia, spinal irritation with perhaps pain in the back, certain paresthesias, easy fatigability, and emotional irritability. This may be a primary neurosis or it may be a secondary one following upon other illnesses, such as prolonged sickness of some kind, or succeeding an acute illness, such as typhoid fever. As a primary neurosis it may be described as a primary fatigue neurosis, although it must be realized that the assumption that it is due to fatigue and that the symptoms

are dependent upon the elaboration of toxic fatigue substances is purely hypothetical. Even admitting the truth of all these things, however, there is absolutely no warrant, if our present concepts of this condition are correct, and they are being verified every day, for believing that occupation of any kind or of any degree of severity can be other than a purely adjuvant and unimportant cause of this condition, as is the case in hysteria.

The limits of this paper prevent any extensive illustration of the opinions that I have just brought out with reference to hysteria and neurasthenia. It will be recognized that I have in mind the more recent hypotheses of Prof. Freud, who would place both of these diseases, and a number of others, in the same category—namely, he would ascribe as the necessary etiological factor in both cases some sort of disturbance in the sex life of the individual, so that these diseases become of great importance, not only because they are, as I would call them, social diseases, but because their problems are essentially broader than the individual—they are biological problems. A single illustration will give an idea of how I regard the situation.

It will be seen in the literature, for example, that many of the telephone girls are becoming neurasthenic, and the cause is attributed to long hours of work, the extreme effort of attention that is necessary because of the character of the work, and its constant annoyance and irritating character. All these things are true, but if the modern hypothesis of neurasthenia, to which I have referred, is correct, they cannot be the fundamental causes. To speak broadly we can only understand the neurasthenia in such cases by thinking of these girls as individuals who have been prepared by nature, up to a certain point, to fulfil a certain function; and then because of the exigencies of life, all the energies which have been developed in that direction are, so to speak, side-tracked, and at about the period of puberty, when nature might expect physiological fulfilment, the individual is called upon to make a complete readjustment, to find entirely new avenues of outlet for nervous energy, to concentrate upon entirely alien interests. Some people are so constituted that they can do this thing; as a result they succeed. Others are so constituted that they cannot. They become neurasthenic or develop other neuroses, while certain others, and they are perhaps the most frequent, occupy a borderline position. These girls under favorable conditions of employment, with plenty of opportunity for rest, good food, good housing, etc., manage to get along. With prolonged hours of work and irritating conditions, perhaps coupled with unhygienic and unsanitary living, they break down. So it will be seen what is meant by fundamental causes, and how we regard the usually attributed causes as only adjuvant. It will be seen also why we believe the problem is deeper than the individual, and strikes at once at the

social conditions brought about by the various industries and occupations.

We might speak of other conditions, but these two are sufficiently illustrative. Hysteria surely and in all probability neurasthenia belong to the diseases which are not dependent upon the introduction into or the action on the body of some specific morbid agent. They are essentially social diseases which depend for their existence upon the maladjustment of individuals to their social surroundings, their inability to meet the demands that are made upon them because of their relations to other people, actually or prospectively, and as such these diseases cannot be dependent for their existence upon long hours of work or upon the character of that work. These considerations, however, do not make it any the less important that these conditions be considered in connection with the various industries, nor do they make it obvious why there has been such an increase in the number of these diseases. A moment's reflection will convince anyone that their importance lies not in the fact of their association with any particular kind or character of work so far as the fatiguing qualities of that work may be concerned, but in the fact that they are expressions of causes that are more widely operative, social causes which have invaded and changed the social conditions under which people live, and evidently changed these conditions disadvantageously, so as to make possible the outcropping of these diseases, and investigation of industrial conditions should realize this factor in the situation as exemplified by the presence of this class of diseases.

My plea, then, in this paper is for the recognition of what we have termed social diseases, for a realization that the problem of the various industries, as that problem deals with the question of the health of the workers, is a broader problem than the problem of ordinary physical disease. It is a problem which touches the whole question of society, and which presents for consideration the neuroses and the psychoneuroses as indications of a disease which is not individual, but social. Here we have in this class of diseases a point of attack upon abnormal social conditions, and by their study some kind of idea may be had as to the best means of approaching the faulty social conditions of which they are the expression, and which may be incident to the industrial conditions under consideration.

The necessary restrictions of such a paper as this make it impossible for me to illustrate what I mean except briefly. It has been shown by careful analysis of certain cases of these psychoneuroses that the disorder was dependent upon experiences in the early years of childhood—types of experiences which, in many instances, were dependent upon the general crowded condition in which the families were forced to live. It will be seen, therefore, in such cases that the disorder from which the patient suffers

leads directly back to social conditions, which social conditions may or may not be dependent upon industrial conditions. The psychoneuroses, therefore, may become a good barometer of certain social states dependent upon industrial conditions, and while it is known that the social state of the worker has been investigated fully, still we do not think that it has been appreciated that many of the specific manifestations of illness are outward indications of certain types of social conditions that are undesirable. It will be seen then that a correlation between the work of the psychiatrist and neurologist and the social worker is possible along these lines. This paper merely bespeaks a recognition of the advantage of this correlation, which if pursued would be seen to offer many possibilities for work.

THE TOPOGRAPHY OF THE CARDIAC VALVES AS REVEALED BY THE X-RAYS.¹

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FOR many years attempts have been made to determine definitely the exact spots on the anterior chest wall under which lie the valves of the heart. The method by which studies in this direction have been made has consisted of the thrusting of long pins into the thorax and then opening the latter and noting the points at which the pins pierced the heart. From data thus obtained diagrams were constructed which were supposed to depict the valves in their relation to the anterior chest wall. That this method is inaccurate is demonstrated rather forcibly by the lack of unanimity in the descriptions given by various authors. In the following excerpts we present the opinions of a number of authorities, and it is interesting to note that the more superficially located is the valve the more nearly do the different authors agree, while the deeper lies the valve the more widely do the writers disagree.

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