The Pulse Rate

A Contribution to Clinical Medicine

by

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I hereby declare that this paper—
at ovs usque ad malum— in conception
and execution— is entirely my own.

George Heron Ritchie

[Signature]
The object aimed at in the following paper is to explain some of the laws which modify the pulse rate. All consideration of its other characteristics has been purposely excluded. In thus selecting for pathological study the rate of the pulse, I have found myself in a field of inquiry which has long been deserted. According to some authors there is no harvest to be gathered in such a field; while according to others what little may be gleaned is of no value for diagnostic purposes. Nor are these sentiments expressed by men whose opinions we can afford to treat lightly. Thus we find Dr. Graves declaring that the causes which modify the pulse rate in a particular case are not only unknown but that they will ever remain so. The causes which produce quickness of the pulse in one case and its slowness in another in every respect apparently similar will for such remain undiscovered; and the same observation probably applies to the causes upon which depend irregularity of the heart's action.
Medicine page 9141. Here this really is we might perhaps find some consolation for having thus to relegate to the region of the Unanswerable, the causes of the modifications of the pulse rate, in the assumption that after all, these causes are not worth knowing. Such at least is the opinion which meets with most favour at the present time. Increased frequency of the beat "says Dr. da Costa (Medical Diagnosis, Third edition page 36) "denotes increased frequency of the heart action and arises from any cause which excites the heart. Hence exercise, rapid breathing, mental emotion or restlessness will occasion the number of beats to exceed the average of health as readily as fever or acute inflammatory disease. In great debility the pulse rises and the more depressed the vital condition the higher the pulse becomes. The heart may thus quicken from so many and such varied causes acting temporarily or permanently that increased frequency of pulse taken by itself has no significant diagnostic meaning."
Such being the state of opinion it is evident that no historical introduction detailing previous discoveries on the subject is necessary. Any disgrace moreover which might attach to failure will be removed by the reflection that failure has hitherto been, and if Dr. Graves is right, ever will be the fate of all who attempt to grapple with this knotty problem. Although however no historical résumé be necessary we must try to form a clear conception of the theories which prevail on the subject. For this purpose a consideration of the extract from Dr. Da Costa's work will serve as well as any other, as in it there is expressed, with greater frankness perhaps than elsewhere, the current opinions on the matter. Any cause" then says Dr. Da Costa "which excites the heart increases the frequency of the beat." This looks a very simple proposition and one to which most people would give a ready assent. It is however very far from being so simple as it looks. It implies a theory as to the action of the heart which
which may or may not be true, but which is certainly as yet very far from having been proved to be so. This which might be called the theory of Excitability assumes that rapid action is the result of stimulation. The heart is supposed to quicken its rate in a manner precisely the same as does a horse when its rider applies the whip or spur. Exercise, emotion, restlessness are all as many goads, so many spurs. It is very easy to see how such a theory has arisen and has received ready acceptance. We are all conscious that under the influence of excitement of any kind we feel our hearts acting rapidly. We feel too that rapidity of motion of the voluntary muscles is one of the constant results of stimulation. An angry man gesticulates; a joyous man feels inclined to skip about. The analogy supposed to exist between rapid movement of the voluntary muscles and rapid movement of the heart, taken along with the fact that rapidity of the heart's action is always found among the results of excitation form
the grounds on which the theory of Excitability
seems to be founded. Were such results the
only ones with which this modification of
the heart's action is found conjoined, the
presumption would doubtless be strongly in
favour of the correctness of such a theory.
Unfortunately however we come upon this
modification very frequently amidst circum-
stances of an exactly opposite kind. As fact
says Dr. Arter (Stimulants and Viscocities their
mutual relations, page 180) is now more
familiar to practitioners than that the
most purely debilitating circumstances are
precisely those which most greatly increase
the rapidity of the pulse; and again "to
great an increase of frequency always
implies debility." Rapidity of pulse then
being found alike amid the results of
depression and the results of excitation we
find ourselves on the horns of a dilemma.
If the theory of Excitability is to be held
as explanatory of all instances of rapid
heart action, then we must conclude that
there may co-exist a violent state of excita-
ment.
ment in the heart with extreme depression of all other parts of the organism. This curious conjunction of apparently contradictory states would appear to have been considered quite possible by Dr. Alison. The loss of blood has... the effect of increasing the suitability of the vascular system and so leading to a state of the system described as Reaction after loss of blood or as Prostration with excitement. (Alison's Pathology and Practice of Medicine page 27). Should we have some difficulty in accepting this as the explanation of the matter, there is another alternative open to us. We may consider that while stimulation will explain some cases of rapidity of the heart's action, depression will explain others. The same result can be produced by two contrary, may, by two contradictory causes. If this alternative be the correct one, then Dr. Da Costa has certainly better grounds than those he states for denying any significant diagnostic meaning to the pulse rate. It is difficult enough to attach much meaning to a phenomenon which
which has "many and varied causes" but the difficulty is increased tenfold when among the variety are included causes like depression and excitement which at first sight would appear to be mutually exclusive of each other. Without attempting to enter further into these theories, one inference from what has been said appears very evident. If we are to make any advance beyond the present unsatisfactory state of opinion on the pulse rate, all words like excitement, depression, reaction, and protrusion, as applied to the heart's action must be put aside. They all imply theories, some of which are erroneous, others improved. The theory of stimulus for example, is still as mysterious as it was in the days of Hippocrates. The use of such words, not only conceals our ignorance, but forms a barrier to any real progress. In this paper an attempt will be made to define the meaning of all words employed and as far as possible to use words which shall describe the facts and no more.

Returning
Returning to the extract from Dr. Costa's work let us direct our attention for a little to the reasons which he adduces for refusing any diagnostic value to the pulse rate. These appear to be the multiplicity and variety of the causes which affect it. To diagnose is to infer from the effects which we observe the cause or causes which have produced these effects. If an effect has many causes, it is obvious that we cannot from a consideration of the effect alone determine in a particular case to which we are to attribute it. The argument appears perfect in its simplicity and conclusiveness and yet just as we found that the stimulation theory of the heart's action landed us on the horns of an awkward dilemma so we shall find the acceptance of this apparently conclusive argument will bring us face to face with difficulties of no mean magnitude. It will throw doubt on the diagnostic signification of a symptom which unlike the pulse rate is generally regarded as of great value. So the variation in
in the temperature of the body observed in disease has been assigned by universal consent great importance. And yet the temperature would appear to be influenced by well nigh as 'many and as varied causes' as the pulse rate. The heat of the body is elevated by exercise as well as 'by fever, or an acute inflammatory attack.' A high temperature may be found conjoined with Relapsing fever or Rheumatism, with Pleurisy or Peritonitis. Wherein then lies the difference between the pulse rate and the temperature? Why refuse all dignity meaning to the former and assure us (as does M. Da Costa Medical Diagnosis page 9) that 'the accurate study of the latter may be of much service in the recognition of a malady and in foretelling its issue'? There is evidently one law for the pulse and another for the temperature. Out of this difficulty there is only one way. We must deny that what are called causes of increased temperature are in reality such, however they may appear to be so. In a
a loose and popular sense fever is no doubt the cause of increased temperature, just as dry weather and an east wind are said to be the causes of the rise of the mercury in a barometer. We know, however, that in a scientific sense neither the east wind nor any other modification of the weather is the true cause of the variations in the height of the column of mercury. The different kinds of weather exist on the barometer only in so far as they possess the common property of modifying the weight of the atmosphere. So is it doubtless with the variations in the temperature of the body. It is in the highest degree improbable that Rheumatism, Syphilitic Fever, and all the morbid conditions which we find associated with a rise in temperature, are such and all separate causes any more than that the various kinds of weather are separate causes of the modifications in the barometer. The temperature is most probably elevated through the agency of some condition which like the modifications of atmospheric pressure
pressure is possessed in common by all the apparent causes. What that common condition is we do not as yet know. At first sight this would appear to be but a sorry way of accounting for the favour with which thermometrical observations are justly regarded. To deny that a phenomenon has many causes and to assert that the one or more which it probably has, is altogether unknown, would seem to be a very thorough method of denying to it any value whatever. And yet notwithstanding this, in the thermometer, medicine has an instrument the potency of which has not as yet been fully appreciated. As this has an important bearing on the laws which regulate the pulse rate, I shall, at the risk of appearing to digest attempt to show in what direction the thermometer has been of most essential service in the study of disease.

It cannot be denied that so long as we confine our attention to the variations in temperature which take place in the different forms of disease, and try to give these
these variations diagnostic meaning the result is somewhat disappointing. The fact for example that the average temperature of Relapsing fever is higher than that of Typhus is no doubt of importance. But further than the facts we cannot go; they tell us nothing beyond themselves. In some far distant future when the laws which govern the manifestations of disease have been unravelled, the reason will perhaps be discovered why these variations must be, and what they mean. In the meantime however they are mere isolated fragments of knowledge waiting for some master mind who shall fit them into a harmonious whole. It is when we turn our attention away from the different kinds of disease, and look at it as a whole that we see what the thermometer has done for us. Though it there has emerged almost unnoticed a Law of disease. The fact that Fever and Inflammation are accompanied by a rise in the temperature of the body is so invariable, that it takes its place among the
the Laws of Nature. As familiar are we with this correlation that we are apt to forget how recent and important is this addition to our knowledge. That the thermometer could be employed as an absolutely certain guide, enabling us to say in a given case, that Fever or Inflammation, is not present, was unknown forty years ago. Thus we find Dr. H. Holland [Medical Essays and Reflections page 513] doubtless whether there are any distinctive characters of Inflammation certain enough to warrant their being classed apart. And more than once we find Marshall Hall lamenting that sometimes it is quite impossible to tell whether we have to do with a case of inflammation or mere irritation. The only instrument with which he was acquainted potent enough to make a differential diagnosis was the lancet. In doubtful cases this [the repetition of blood letting] furnishes us with a fresh means of diagnosis. If much blood has flowed before syncope occurred we must suspect inflammation, if little, we must
must suspect; that however similar the
symptoms, the case is in fact one of a
different nature, perhaps irritation, perhaps
exhaustion" (Review on the Morbid and Lacerous
Effects of Loss of Blood, page 181). We may
well rejoice in the interests of humanity no
less than in those of Medicine, that the
exigencies of diagnosis no longer require us
still further to exhaust an exhausted patient
merely as a means of discovering the condition
with which we have to deal.

In face then of the fact of the in-
variance of the connexion between Fever
and Inflammation on the one hand, and
raised temperature on the other, all other
facts in thermometry sink into comparative
insignificance. It is not an absolute law,
for we have as yet been unable to fix upon
the common element in these forms of
disease in which it occurs. When this common
condition shall have been discovered it will
bear the same relation to the law connecting
febrile conditions with increased temperature
that Kepler's laws of Planetary Motion bear
to
to the Law of Gravitation.

With the pulse rate all, this may seem to have little connection. I have however dwelt a little on the nature of the law of increased temperature for several reasons. Firstly, because a consideration of this law shows us that we are by no means justified in putting aside a phenomenon as destitute of "significant diagnostic meaning" merely because it has or appears to have "many and varied causes". Secondly, because in this paper an attempt will be made to prove that laws similar in character subsist between the condition of the body and the heart on the one hand, and the rate of the pulse on the other. Its ultimate character will be deemed for these laws. Probably if true, they are only derivative; but this hardly lessens their practical value. Lastly, I have dwelt on the temperature, as between its modifications and the modifications of the pulse rate, there is universally acknowledged to be a most intimate relation. One of the laws will affirm the invariable nature of this connection.
connection and an attempt will be made to explain the apparent exceptions we meet with in various forms of Fever. A consideration of the value of the variations in temperature showed us that this consists not so much in enabling us to diagnose particular kinds of disease, as in giving us a law which regulates a very large number of diseases. The same will be found to hold good with regard to the variations of the pulse rate. So long as we try to identify particular kinds of pulses with particular diseases, so long will our industry go unrewarded. Then however we approach the subject expecting less, we shall probably get more. The pulse rate, as a rule, does not indicate the disease; it indicates the condition of the patient modified in certain cases by the condition of the heart's structure.

In the light of these principles let us approach the facts of the case. When we take a comprehensive survey of all the circumstances in which we find modifications of the pulse rate, we discover that these include...
include Physiological as well as Pathological conditions. This fact could have no significance so long as disease was considered to be an entity which, occupying the body, much as a bad tenant does a house, was to be expelled at all costs. When however we regard Pathological conditions as being mere modifications of Physiological ones, the fact of any phenomenon being found in both comes to be of primary importance. This manner of viewing the subject says Dr. Henry Holland (Medical Notes and Reflections page 513) through the relation of disordered actions to those of health, is here as in all cases the best; whether we look at the theory of disease or at its treatment in daily practice.

Let us see then what an examination of the physiological relations of the pulse rate will bring to light. Omitting in the meantime all reference to the experiments which have been made on the nervous supply of the heart, as pertaining to the mechanism rather than to the causation of
of the modifications of the heart's action, we find in all works on Physiology a list of things which act on the heart so as to modify the pulse rate. The rate of the pulse varies at different periods of the day, gradually diminishing from morning to night, and notably declining during sleep. It is quickened during exercise and digestion. It is slower when we lie down than when we sit, slower when we sit than when we stand" (Sewer: Physiology of common Life, Vol. Page 339). An examination of the causes here enumerated reveals the fact that amid much apparent diversity there is considerable uniformity. Take exercise, for example. Every form of exercise, running, leaping, rowing and the like, has the same effect on the heart. In all of them we have a common factor and a uniform result. The common factor is expenditure of muscular power, and the uniform result is the more rapid contraction of the heart. Viewed in this way we see that the variations observed in standing, sitting, and lying, are mere instances of the more general law. The expenditure
expenditure of muscular power is greater in sitting than in lying, and is greater still in standing than in sitting. The importance of these facts lies in their invariability. They show us that there exist certain unchangeable relations between certain actions of the body and the action of the heart. We may not know by what mechanism this action is produced; still, in spite of this blank in our knowledge the fact is of infinite importance. It assures us that the heart's action is regulated by fixed law and is in no sense the seat of what Professor Huxley calls "disorderly mystery." This assurance is calculated to inspire us with faith in the possibility of the discovery of the laws which the heart obeys. It should moreover teach us that if we can see no order the fault must lie in our vision, not in the facts which are before us. That there exist invariable connections between certain abnormal conditions of the body and certain actions of the heart is rendered in the highest degree probable by what we see to be the law in health. What are these invariable relations? If any serious attempt
attempt was ever made to discover them, it must have been very soon abandoned, for there seems to be a consensus of Medical opinion, that the more the pulse rate is investigated, the less trustworthy does it appear to be. Thus there are no two ideas more intimately associated in the lay, as well as in the Medical mind than those of fever and quickened pulse. A man takes cold, or sore throat, and forthwith the pulse is found to have risen fifteen or twenty beats. That a correlation observed so often and in such simple cases should be preserved in the more serious febrile affections is no more than we might expect. This however is not found to be the case. For though usually there is a correspondence between the rapidity of the pulse, and the gravity of the case, yet there are serious cases in which the pulse rate does not appear to be affected. The temperature may rise far above the normal; there may be utter prostration of strength; death may be impending; yet the heart may continue beating regularly the number
of its beats scarcely altered, apparently unaffected by the ruin with which it is surrounded. Now of this apparent caprice in the manifestation of disease there exists so far as I have been able to discover no attempt at an explanation. The dogmatism of Dr. Graves already quoted — a dogmatism of hopelessness, seems to have cast a spell alike over thought and enquiry. The pulse rate has come to be regarded as a mystery of mysteries, the explanation of which can only be expected as a final triumph of the study of disease. Yet this cannot but appear strange if we reflect a little on what the pulse rate means. Of all modifications of the heart's action it is the most readily appreciated. That the variations can be determined with perfect accuracy would seem to go half way towards solving the mystery of their causation. Moreover the simplicity and breadth of all natural law renders it improbable that there can be very many causes for the same phenomenon. The less we know of any phenomenon the greater the number of causes to which it
is usually attributed, guided by these principles I have tried to show how many apparently different causes are in reality the same under different forms. To show this it is necessary to prove that between certain abnormal conditions of the body and certain variations in the pulse rate, there exist unchangeable relations. These relations which if true will be found to explain nearly all the modifications of the pulse rate observed in disease, are three in number and are as follows.

First. A rise in the pulse rate in a case in which the temperature of the body is not elevated indicates Exhaustion of Vital Energy. This might be called the Law of Exhaustion.

Second. An interference with the contractile power of the Heart as in Tatty or Fibrous degeneration is compensated for by a decrease in the pulse rate. This might be called the Law of Diminished Contractility.

Third. A rise in the pulse rate is the invariable accompaniment of an increase in the temperature of the body.
This might be called the Law of Pyrexia.

The points in which these laws differ from what is ordinarily received are in making absolutely true what has hitherto been regarded as only partially so, and in attempting to reduce to a few elementary principles the puzzling variations of the pulse rate so familiar to all clinical observers. "Principia non sunt multiplicanda" has been the motto steadily borne in mind throughout.

Of the three laws there can be no doubt as to which we must consider first. If we compare them, we see that they differ from each other in one most important particular. Their areas of applicability are very different. The Law of Diminished Contractility, depending as it does on the condition of the walls of the heart, is of comparatively limited application. With the majority of cases of disease it has no concern. The area of the Law of Pyrexia is more extensive, but very many cases run their entire course with little or no rise in the heat of the body. It is different
however with the condition mentioned in the first law. This is of a much more general kind. Exhaustion of Vital Energy characterises every protracted case of disease. It is the most general of all the conditions. This of itself would give it a prior claim to consideration; but there is another reason of even greater cogency. It is not only of more general application than the other laws, but it includes within its scope cases which fall within their domain. For these reasons then, we proceed now to consider what is implied in Diminution, or Exhaustion of Vital Energy.

To exhaust, in ordinary language means to draw off, to drain, to empty. When a man says his funds are exhausted, we understand him to mean that his money is all gone. When he says his strength is exhausted, he means that he is very much fatigued. His strength has not all gone in the same sense as his money has gone. For, if an emergency, say a fire in his house, or the sudden illness of his child were to happen, to one who could very justly describe himself as exhausted.
he would find that he had a reserve stock of energy. When applied to loss of strength therefore the word exhaustion has a more limited meaning than its etymology would lead us to suppose. In this connection it might be defined as a depression of the vital powers which if continued would end in entire loss of strength. Exhaustion as thus defined may be a physiological, as well as a pathological condition. The difference between exhaustion in health and exhaustion in disease springs mainly from the circumstances under which each is produced. In health, exhaustion is produced by voluntary efforts; consequently the efforts can be put an stop to at will before any harm is done. But in disease the case is different. The exhaustion depends on conditions which are beyond the patient's control. If these are continued, then the state becomes dangerous, and then it is that the rapidity of the pulse becomes a sign of importance. The essential fact is, therefore, that continuance of the condition is dangerous to life. Having thus defined exhaustion, it is necessary in order to prevent misconception, that I should explain
the meaning which is here attached to the word force. By vital force we understand what Mr. James (Physiology of common Life, Vol. II page 415) calls the dynamical condition of the organism. Vital force is not a metaphysical entity, separable in any way from matter. Consequently by the vital force of the organism nothing more is meant than the sum of the forces of the various organs.

So far nothing has been said which is not quite in accordance with what every Medical Man has ample experience of, and understands perfectly. No phrase is more frequently heard than "the pulse of exhaustion." The pulse of exhaustion I apprehend is only discovered when the lamp is well nigh burned down, where the pitcher has gone to the well for the last time. The conditions on which the exhaustion depends have been continued, and will continue to the end. The difficulty of proving the Law of Exhausted Vitality does not consist then in any novelty presented by the association of exhaustion and quickened pulse. It lies rather in an apparently universal disinclination—a disinclination as
Illogical as it is curious to allow that exhaustion can be the cause of the quickened pulse in any case where another explanation is feasible. The idea has never been rigidly analyzed, so that most writers seem to have a vague notion, that in speaking of the pulse of exhaustion, they are using a phrase which rather conceals ignorance than conveys knowledge. To explain how this has come about would form a very interesting chapter in the History of Medicine. To write such a chapter is altogether foreign to our present purpose, but two reasons may be briefly noted which will partly account for the lack of favour with which exhaustion as an explanation is usually regarded. In the first place, that exhaustion and rapidity should go together is quite contrary to what we might expect. It is a conjunction of phenomena apparently deducible from no premises with which we are acquainted. But not only is this conjunction unlikely, it also runs counter to many received doctrines! Thus Dr. Austin (stimulants and narcotics page 100) points out that the ordinarily received doctrine of stimulus
assumes that "all increase whether in the force or frequency of the heart's action is caused by, and is a proof of, a stimulant action upon the organism." The grounds on which Dr. Austin rejects this assumption bear directly on the subject we are considering that I shall quote the passage entire. Increased rapidity of the heart's action is one of the most common items of evidence appealed to as indicating the operation of a stimulant on the organism. The subject involves considerable difficulties; for there is little doubt that a moderate increase of the heart's action beyond its normal rapidity often does prove to be of real advantage to the vigour of the faculties for the time, and is followed by no depressive reaction whatever. On the other hand, no fact is now more familiar to Practitioners than that the most purely deliriating influences are precisely those which most greatly increase the rapidity of the pulse. No exact line of demarcation can probably be laid down, but we are entitled to believe that an increase of a few beats per minute when the strength
of the heart's action is simultaneously maintained, or increased, is a healthy phenomenon, one which is within the order of nature and so far entitled to be described as an effect of stimulation; but that on the other hand too great an increase of frequency always implies delirium, more especially when coupled with diminution in force or irregularity in rhythm" (Stimulants and Narcotics page 105). In this extract there are many points of interest. To some of these, especially to the difficulties which are involved in the subject we shall by and by return. Meanwhile it may help to illustrate what has been said if we note how very little attention a fact, than which none is more familiar to Practitioners, has received at the hands of the authoritative expounders of Medical Science. No author except Mr. Austin, whose reference to it is merely incidental, has thought its consideration worth even two lines. Some even do not seem ever to have heard of it. Thus Dr. Barclay in his work on Medical Diagnosis has a chapter (the III) on the means of ascertaining
the "general condition of the Patient" in which this most familiar fact is never mentioned. A number of conditions of the pulse are detailed, but this condition which according to Dr. Austin "always implies debility is not considered worthy of a place in the list. Comment on this omission is hardly necessary. It shows very clearly however, the necessity which exists for a clear definition of the conditions on which rapidity of pulse as connected with exhaustion depends. Such a definition if complete should bring to light the causes of this phenomenon, and should accurately mark the limits within which the action of these obtain. In attempting to perform this task I have thrown the main points into the form of propositions. In the first two propositions nothing is assumed except the fact that rapidity of pulse is often observed in cases which afterwards prove fatal.

The first proposition is as follows:

"The may discover the pulse of exhaustion a considerable time before death. In illustration of this take the following case. Mr. W. an
old lady, 95 years of age, had an attack of syncope from which she recovered. For five days she continued very weak supported chiefly on brandy. Her pulse, which was feeble, never fell below 120. On the fifth day she had another attack in which she died. Now, no Physician would hesitate to describe the rapid pulse of the last five days as the "pulse of exhaustion." This however is not the fact to which I wish to draw attention. I had been in attendance on the case for more than three months before the first attack of syncope occurred, and during all that time the only abnormality presented by her was a pulse varying between 96 and 107. I had two opportunities of counting it when the patient was asleep and found its rate the same as when awake. There was no increase of temperature. The most careful and repeated examinations of the different organs revealed no disease; but all the time the pulse continued as described. Although able to take a certain quantity of food she was very weak; and there was
a tendency to faintness on sitting up. Now as no post mortem was made it is impossible to state precisely what was the cause of death in this case. But if the pulse rate of the last few days can be ascribed to her exhausted condition, it is difficult to see why the pulse rate of the preceding three months should not be attributed to the same cause. If exhaustion can produce a pulse of 120 why should it not produce one of 102. As a practical fact and one on which to base a prognosis I found this way of looking at the pulse rate very valuable. Regarding the rapidity as a serious condition I gave a grave opinion of the case long before syncope supervened.

Connected with this case I may mention two others in which the cause of death was undoubtedly old age. Of these one was nearly 80; the other nearly 90. I had daily opportunities of observing both for a considerable time before death. In both for weeks before the end the pulse varied from 110 to 120. Although
there was no rise in temperature. The more aged of the two cases I visited daily for upwards of eight weeks, and the end was heralded by a pulse which never fell below 120.

In contrast to these cases in which exhaustion and rapidity are almost the only symptoms, we may look at another in which disease was ascertained to exist.

A lady about 45 years of age had suffered from childhood from bronchitis. About a year before her death it was ascertained that she had also waxy kidney. This condition which was most probably due to the excessive discharge from the lungs, had in all likelihood existed for some time before it was discovered. At that time her condition might be described as that of a person in delicate health. She attended to her household duties, and in fine weather drove out. She took a fair amount of food and no immediate danger was apprehended. This condition continued for about a year, when an attack of influenza prostrated her for nearly three weeks. From this however she
recovered. The cough improved so that it was no worse than it had been for years. An examination of her chest revealed no new complication. With careful nursing her appetite improved. Her kidneys were acting well, but still there was no return of strength. Day by day she grew weaker and it became evident that for her the Rubicon was passed. Now with the exception of the weakness the only appreciable difference between her condition at this time, and what it had been a year before was a rise of the pulse rate. In seven weeks this rise was steady from 107 to 115. Week by week it gained a beat or two. At first it could not be described as weak but as it gained in rapidity it lost in strength. There was no difficulty in understanding what she was dying of. But the cause was as well known twelve months before as it was then. The difference was in the patient, and that difference was clearly marked by the increased pulse rate. The temperature was normal at least till very near the end which I did
not see. Two days before death Uraemic poisoning set in. Now this case illustrates clearly in what direction the pulse rate gives information of value. It tells us the condition of the patient. Its rapidity marks a diminution of the vital energy. From this case too we see, that under certain circumstances the rapidity increases pari passu as the strength ebbs away. In the language of chemistry it may therefore be not only a qualitative but also a quantitative sign. These considerations lead us naturally to the second proposition which is—

That a rise in the pulse rate may be the only appreciable sign of approaching death.

So a certain extent the last case illustrates the truth of this. We knew all along that conditions existed in this patient which must in a comparatively short time bring about her death. But as we have seen this result was preceded by no signs directly traceable to the organs affected. The patient gradually got weaker, and her pulse rose—that was all. These signs
gave information quite independent of the diseases from which she suffered. Even had we known nothing of these diseases the signs would have been just as valuable. They did not tell us what she was dying of, but they told us she was dying. In the present state of our knowledge there are many diseases which our means of diagnosis fail to detect. That they ever will be detected in life is very doubtful. How it is in such cases that the value of any sign which indicates the condition of the patient, as distinct from those which flow from the disease become of the highest value. The spectacle of a Physician standing by the bedside of a patient, ignorant alike of the cause and the issue of the phenomenon before him, is not creditable to our profession. A great step towards putting our art on a scientific basis would be gained, if while frankly acknowledging the mysteries of many cases to be beyond our present means of investigation, we could yet in such a case foretell from general signs what the issue must inevitably be.
That many cases of this kind must occur can hardly be doubted. But the records of such cases are very few. We need feel no surprise at this, if we reflect what an amount of moral courage it requires to put on record the details of our ignorance and failure. Only men of the very highest intellectual and moral calibre dare do it. Difficult as the task no doubt is, it has been performed and it is from the record of such a case, that I intend to support the proposition under consideration. In his Clinical Medicine Vol. II page 149 Dr. Latham gives the details of the following case. 

"J. R. was not far short of seventy. For the two or three last years of his life I knew him well and saw him often, but never heard him complain of any infirmity. He was an accomplished scholar; conversed and more than habitually cheerful; he was even habitually jovial. This I mention to show the great probability that he really felt nothing of his fatally progressive disease with such a constant complexion of mind surely he could not have been a constant
On Saturday the 7th May 1831 he became alarmingly ill, and I was called to visit him. I found him seated in his chair, his countenance blanched, and full of anguish, his breathing hurried, his skin chilly, and his pulse very frequent and very feeble. He spoke in catchs, now running on rapidly and now coming to a stop, as people do when they have not breath enough to carry them through what they have to say. He muttered something about striking and fainting.

All this looked near akin to death. And my present business was to keep him from actually dying by any stimulus I could administer.

From his history I conceived simple exhaustion was almost enough to account for what I saw. I had him put into a warm bed, and desired that warm gruel mixed with brandy should be given him pretty frequently, until I saw him again. In an hour or two he was relieved in his general feelings. He had thrown
off his death-like aspect. But still his respiration was fearfully hurried and his pulse very frequent and very feeble. And now in running my ear over his chest I heard a small respiration at various parts of the lungs. It was upon the whole of great extent, and proceeding (as it seemed to do) from the lesser bronchial ramifications I looked upon it as the cause of the present dyspnea. Accordingly I took it as my indication of present treatment and ventured to apply a few leeches. They drew but little blood and that very tardily, owing probably to the dullness of the surface and the emptiness of the cutaneous vessels. Nevertheless he was relieved, and during the night a poultice was spread all over the front of his chest which obtained a little more blood from the leech-bites, and more relief. With this local treatment I was still upholding him with cordials and ammonia.

The next day, Sunday, I saw him several times. There was less about him to occasion alarm, but enough of hurry and impediment in his breathing to call
for a large blister. This was applied to the chest and allowed to remain on twenty-four hours. It raised very little vesication but acted well as a salutary, and further relief followed.

On Monday this relief was apparent and when my ear was now applied to the chest, it found the respiratory murmur passing through every part of both lungs unmixed with any unnatural sound except below the scapula. Here the respiration remained. His skin was now constantly bedewed with a warm gentle perspiration. Every thing about him was promising except his pulse, which was still very frequent (much above 100) and extraordinary feeble.

On Wednesday, having allowed him to leave his bed, I found him up and dressed and seated in his chair, reading Horace, and as joyous as ever, and intent upon going to the Eton dinner the next week. At this time he believed himself almost well, and indeed he looked so. Even his respiration no longer suffered impediment from the exertion of walking about the room.
still the feebleness of his pulse was extreme and its frequency was not at all reduced.
The two next days, Thursday and Friday, brought him in his own consciousness and to all appearance nearer and nearer to health. But his pulse was as weak and as frequent as ever. It had also something strange in its character which I cannot describe. In my examinations of the chest from time to time I had found the heart's impulse exceedingly feeble, yet perceptible in a larger space than natural; and it was feeble still. All this did (I confess) occasion me some apprehension, and I begged upon the plea of the peculiarity of his case, that he would allow me the benefit of consulting with some other physician before I granted him the rights and privileges of health. Accordingly it was agreed that Dr. Waton should see him with me on Sunday or Monday.

On the morning of the next day, Saturday, I found him in bed and was told that he had passed the night uncomfortably. He had himself however been quite unconscious of it at the time, but he was now sensible
enough of a more hurried respiration. Yet to my ear the air passed freely and without unnatural sound through the entire lungs except the lower part of the right behind. Here there was pectoration, and here I put on a blister. I saw him again at 8 o'clock at night, when he was much more at ease in his chest. Three hours afterwards I was summoned to him on an alarm of his being worse. I went and found him dead.

The account given me was this. His attendants had been absent from his room a few minutes. On their return they found him with his body stretched towards a table by his bed-side, lying motionless, and holding a cup of barley-water in his hand. They thought he had fainted and endeavoured to rally him, but without success. He was probably then dead, although, when I arrived an hour afterwards, they did not seem sure that he was so.

The body was examined after death. The cavity of each pleura contained about a pint and a half of clear serum, without a vestige of inflammation on the surface of
the membrane. The lungs were pervious to air and crepitous at every part. Wherever they were divided, there followed a small quantity of frothy serum a little tinged with blood. At its posterior and lower part the right lung was somewhat denser and of a darker colour than elsewhere. But here to it was quite crepitous, only, where it was divided, a larger quantity of serum and more mucus mixed with blood followed than from other parts.

There was no unusual amount of fluid in the bag of the pericardium; but the membrane was unusually vascular. The heart was large and at one part of its wall (a space of about two inches) constituting the left ventricle, it was pale and very soft and gave to the touch the notion of an abscess approaching the surface from within.

All its cavities were perhaps slightly increased in their capacity, but its lining membrane presented no visible trace of disease, save where and what will be mentioned presently. Its whole muscular structure was flabby, pale, and lacerable.
a condition which seemed to arise from its partial conversion into fat. The fat in some parts occupied the place of the muscular fibres, the external layers especially; in other parts it was intermingled with them, now one and now the other being predominant.

That portion of the left ventricle already mentioned, which in its external aspect gave suspicion of an abscess, presented the following conditions of disease. There the heart was so attenuated as not to exceed the breadth of a half-crown piece, and rupture or ulceration preparatory to rupture was in progress. The internal lining was destroyed, and to the rough surface that it left a large irregular shaped clot of blood was adherent. What remained exterior to the clot had lost all recognizable organization, it hardly cohered together and was torn like wet paper.

The aorta throughout its course within the chest (for as far only it was examined) was dotted with little earthy and atheromatous deposits. The omentum was loaded with fat. The liver was twice the natural size and
full of blood, apparently not diseased. Its red structure predominated."

This case is graphically and candidly told affords ample material for reflection. It is quite plain that Dr. Latham had no idea from what disease his patient was suffering. The symptoms he says had no diagnostic character. It is now nearly half a century since the case occurred, and I doubt whether, were any of us to meet with a similar case, we should be able to make any more of it than he did. His modern discovery has gone any way towards identifying the symptoms with any particular disease. But not only was the disease a mystery to him, the condition of his patient was no less so. In a vague way he seems to have felt alarmed, but he was within a little of granting his patient "the rights and privileges of perfect health." If, however, the principle be true, that a continuous rise in the pulse rate in a non-pyrexial case, is always indicative of exhaustion, he the cause what it may, what a different
complexion does this case assume. Dr. Latham says of the symptoms that "as to the nature of what was going on they told us absolutely nothing". If by "what was going on" Dr. Latham meant what organ was affected, and how his statement is no doubt true; but if we interpret these words in a wider sense, so as to include the condition of the patient, and the probable result of the phenomena the statement is very far from being true. The only symptom which persisted through the whole illness was the frequency of the pulse. Five different times is the fact recorded. It was therefore no lack of observation which prevented a true prognosis being made. It was not the facts which were wanting but the key to the facts. That key is found in the principle which affirms that while all the other organs may give false evidence as to the real state of the patient, the modifications of the heart's action will often reveal the secret. Consciousness gave no hint of the death which was
as near. He proposed being present at
the "Eton dinner next week". His mental
faculties were unimpaired nor did he suffer
at all from depression of spirits. He was
reading Horace and was "as joyous as ever"
three days before his death. His powers
of locomotion remained as he could walk
about the room. Unfortunately we have
no record of the temperature but the
presumption is in favour of its having
been normal. How amid such apparently
conclusive evidence of recovery the pulse
alone by its frequency indicated that the
danger was not yet over.

Hitherto we have been considering
the pulse of exhaustion in cases which
have terminated fatally. We have seen
that even when looked at in this limited
sense this symptom gives information of
value. We proceed now to consider on
what grounds rapidity of pulse may be
taken as indicating exhaustion in cases
which do not end fatally. This brings
us to the third proposition, which is-

That the typical example of the
effects of exhaustion on the organism is seen in the effects of loss of blood.

Let us begin the consideration by an examination of the phenomena of a fatal case of hemorrhage. With these every physician is more or less familiar. Besides such symptoms as the icy coldness and the marble like pallor of the surface, the cessation of which we can easily understand, there are others which depend on conditions by no means easily fathomed. Such are the extreme restlessness, the sighing respiration, and that symptom with which we are here more immediately concerned, the rapidity of the pulse. In order to see the full significance of this rapidity, let us analyze the conditions which obtain in a fatal case of hemorrhage. The first fact which strikes us is that we have here a morbid agent which can produce death without any change in the structure of any organ whatever. A post mortem would reveal no change, only an erysungs state of the tissues. How this is a much more
important fact than it first sight appears. For what does it imply? It implies that we can produce death by the mere diminution in the bulk of one of the organs of the body. The blood is an organ necessary in some way or other to the carrying on of the vital processes. This may be said with equal truth of the lungs, the liver, or the stomach. But the blood differs from these in being in the liquid form. Hence we can do to the blood what we cannot do to them. We can by abstracting a part of it reach the very centres of life itself. Now the importance of this simplicity of causation can hardly be overrated. The result moreover is as certain as the cause is simple. This certainty and simplicity make death from haemorrhage as much an experiment as anything involving vital processes can well be. Let us apply these principles to the subject under consideration. We are seeking to establish the cause which cuts off the heart so as to increase the number of its beats in a given time.
Now the difficulty which lies in one way is the great variety of circumstances in which this phenomenon is observed. To pick out the conditions on which it depends would appear to be impossible. Our only way is to observe carefully the cases in which the conditions appear to be the fewest in number. Having found these we are entitled to conclude that among these few conditions must lie one of the causes of which we are in search. Now as applied to haemorrhage this is all important. We see from what occurs in a fatal case that rapid pulse may be produced without fever, or inflammation, without any disease of the heart, without any disease of any organ whatsoever. Death follows from the alteration in the mass of the blood. It may be said and indeed has been that this modification of the heart's action is to be attributed to some condition peculiar to haemorrhage. Marey has formulated a law which I suppose is intended to explain the phenomenon. I have been unable to procure
Mary's work on the circulation and only
know his ideas at second hand from
Scaria's "Etudes de Medecine Clinique".
Consequently it will not surprise me if
I afterwards learn that I have miscon-
ceived the scope of the law which he
laid down. According to this law the
Motor forces being equal the heart beats
more rapidly in proportion as it experiences
less difficulty in emptying itself (D'uley,
de la science Medeciale Arts, d'agron.) It will
be noticed that the particular modification
of the heart which follows haemorrhage
is conditional. If the Motor forces remain
equal the heart will beat more rapidly.
This is a most important limitation, so
important indeed that it destroys the
whole force of the law. For how can we
tell that the Motor forces which act on
the heart after haemorrhage are equal
to those which acted before the haemorrhage?
By what means are we to measure the
variations in the Motor forces. Such power
of measuring is necessary before we can say
that of any of two states their Motor forces
are equal. Does it not seem highly probable that the haemorrhage itself will occasion a disturbance in the motor forces whatever these may be. For who can pretend to say that he knows what are the motor forces of the heart. Consequently though this law may be perfectly true yet it is hampered by a condition which renders it practically of no value. We have moreover no right to assume special causes for special cases until it has been proved that general causes fail to explain the circumstances. The Law of the Parsimony of causes forbid us. How if we compare the phenomena of a fatal case of haemorrhage with those of the three cases which were given in detail when treating of the two preceding propositions we shall find that although they differ in very many particulars yet there are several points of similarity common to them all. In all we have rapid pulse and in all we have exhaustion of vital energy. Are we not entitled to assume that phenomena which are found conjoined amidst such diversity are in some
way intimately connected. We are at least justified by these facts in assuming as a hypothesis that exhaustion is the sole cause of the rapid pulse in a fatal case of haemorrhage. This hypothesis will be found to help towards the solution of so many difficulties as almost to make its correctness a certainty. Thus if exhaustion explains the rapid pulse in a fatal case, why not also in one which just misses being fatal. So long as the passage from life to death is complicated by complex and unknown pathological processes, it is in the power of any one to object that the rapid pulse may only supervene when a fatal issue is certain. Our inability to separate the causes and give to each its due share in the production of the symptoms necessarily renders us powerless to answer this objection. When however we find this phenomenon following haemorrhage in fatal and non-fatal cases alike we are bound to conclude that its causation in both must be the same. For is this all, for if exhaustion is the meaning of the rapid pulse which
follows loss of blood we get in the phenomenon of haemorrhage a standard by which to measure the amount of exhaustion. For the modifications of the heart's action which follow haemorrhage are not the same in all cases. The variations depending as they do on the amount of blood lost, must correspond to the amount of exhaustion. One would naturally expect that the greater the exhaustion the more rapid the pulse, but as will be seen from the following classification of the results of haemorrhage this is true only within certain limits.

So far as the rate of the heart's action is concerned the effects of haemorrhage may be thus classified:

1. The pulse rate may not be affected at all. This happens when the drain takes place steadily and in small quantities. The quantity may however be considerable. I lately attended a lady in whom metritis came on when six weeks pregnant. After recovery from the metritis very considerable flooding set in which continued more or less for six weeks. Neither during nor after
this flooding was the pulse rate ever affected.

2nd. The pulse may soon, or after a considerable interval, become rapid. Up to a certain point, there is usually a correspondence between the amount of haemorrhage and the rapidity; and the steps towards recovery in such cases are marked by a falling of the pulse rate.

3rd. If the amount be very large a different series of phenomena may result. These though more or less related, must be discriminated from each other. (1st.) The pulse may be rapid, but its rapidity does not equal what is frequently observed in haemorrhages of a much less amount. "Dans les grandes haemorrhages le pouls contrairement a ce que lon croit generalment n'a pas toujours une tres grande frequente" (Stain Etudes de Medicine Clinique, Le Pouls page 215). (2nd.) The pulse may quickly vary between considerable rapidity and the normal. At one moment it may be 90 at another 130.

For the details of a case of this kind see Lancet of 25th August 1849, case of post partum haemorrhage. (3rd.) There may be rapidity...
alternating with syncope. This form with which our ancestors were much more familiar than we are is very fully described by Dr. Marshall Hall in his work on the Morbid and Lurid effects of Loss of Blood.

What explanation we have to offer regarding these various modifications must be postponed till we have considered the other laws which affect the pulse rate. Meanwhile, our examination of the effects of haemorrhage has brought to light a most important and somewhat unexpected fact. The correspondence between exhaustion and rapidity of pulse fails at both extremes. If the drain of energy though considerable in the aggregate takes place in small quantities the heart may not be affected, while if the amount be large the pulse rate may not rise much above the normal. These limitations must be kept in view in any application we may make of the law of exhaustion.

Let us apply this law as thus limited to some cases in which it seems to be applicable. The difficulty in obtaining
such cases lies in the frequency with which exhaustion and pyrexia are combined. There can be no reasonable doubt that between a rise in temperature and quickened pulse rate, there is a very intimate connexion so that for the purpose of illustrating the law of exhaustion it will be necessary to obtain cases in which pyrexia can be excluded. In a case of Anemia we find an example. Now in Anemia we have not always a continuously rapid pulse. Many cases of Anemia occur without any such symptom. When however the pulse does become rapid even to the extent of a few beats it will be found that there is danger. I had this exemplified in the case of a gentleman who although anemic, and evidently not strong, continued to attend to his business till one morning when on stooping down he nearly fainted. He soon however recovered and went to bed. On examining him shortly after I found anemic murmurs all over the cardiac region and his pulse about 105. In a day or two
he got up and came down stairs; the pulse still continued rapid. On going up
to bed at night he again became faint.
I now confined him to his room where he continued about three weeks in much
the same condition. He had no pain, took his food moderately well, but felt unfit
for any exertion. The temperature which was frequently taken never showed any rise
above the normal. His recovery from this condition was nearly as sudden as had
been its beginning. One morning Dr. James
who, latterly saw the case for me, was assured by the gentleman that he felt
much better. The pulse on examination
was found to have fallen to about 80.
From that day he rapidly gained strength.
The Anemic murmurs gradually disappeared
and he is now quite well.

The cause of the rise of the pulse
in such a case has always been some-
what of a mystery; but if we grant that
in haemorrhage the cause of rapid pulse
is the exhaustion of energy the inference
is easy that deterioration in the quality
of the blood will have precisely the same effect. Up to a certain point just as in haemorrhage there is no effect, but beyond that point the effect is continuous rigidity. We often see the same causes in operation in obstetric practice. As frequently is the pulse rate increased after labour that a rule has been laid down that if the pulse be 100 we should not leave the patient as haemorrhage is likely to set in. That such a result does not always follow a rise in the pulse rate is matter of frequent observation. No attempt has hitherto been made to explain this apparent connexion between uterine and cardiac action; still less to explain why in some cases there is haemorrhage and in others none. If however we regard the increased pulse rate as giving evidence of the general state of exhaustion and the flooding the manifestation of that exhaustion by the organ while from the circumstances is of all organs of the body the most exhausted. We stand in no need of supposing that any direct connection exists between the heart and
the uterus. We can moreover see from this explanation why in some cases the organism may recover itself and no haemorrhage follow. Again, what makes a more severe drain upon our energy than a strong emotion. It is among the most potent of death-producing agencies. We have all heard of cases in which the shock of a sudden joy or a sudden grief has caused immediate death. In less powerful forms it uses up in a marvelously short time all the available energy of the organism. Were it not that in great emergencies sensation seems to be blunted our emotions would be too powerful to permit of our living.

The application of the law to physiological conditions leads naturally to the consideration of the difficulties of the subject which are referred to in the extract from Dr. Aestie's work on stimulants and narcotics quoted at page 28. From that passage it would appear that increased pulse rate is a symptom, which at one time can be described as a 'healthy phenomenon'
and within the order of nature" at another
time "always implies debility" and is con-
sequently outside "the order of nature". The
only difference between the two cases is
one of degree. The pulse rate in the
latter case is described as being excessive.
By being within the order of nature Dr.
Austie I presume meant physiological.
If so, the expression is far from being a
happy one, as it implies that a pathological
process is a disorder of nature. Now the
only thing which can with propriety be
described as a disorder of nature, or as
outside the order of nature, is a miracle.
That a symptom of disease could, even
by implication be so described by a
thinker so generally accurate as Dr. Austie
is very extraordinary. But not only is
the difficulty stated in terms which are
misleading and unphilosophical; it is
also incompletely stated. The phenomena
are more complex than Dr. Austie makes
them appear. That a symptom should
indicate a healthy and a morbid con-
dition of the organism is strange enough
but the problem is made stronger still by the fact that the same cause will produce in one individual the healthy phenomenon, and in another the morbid one. Hence as long as we merely look at the causes the difficulty in explaining the variations in the result is insuperable.

When we look at the pulse rate as not being the direct result of the causes but as indicating the condition of the body subsequent to their operation the difficulty vanishes. If every non-pyrexial increase of the pulse rate marks the expenditure of a certain amount of vital force above a certain point, then it is perfectly clear that in any given case the increase will be a healthy or morbid phenomenon just in proportion to the amount of force there is to draw from. It is a well known law of the body that to keep it in health a certain amount of force must be constantly expended and replaced. The organism while showing a marvellous power of storing force for emergencies is no miser. It accumulates up to a certain
point beyond which it will not go. Muscular exercise affords the readiest illustration. Here force is discharged and the pulse rate is increased in a manner which is not only healthy, but is absolutely necessary for the maintenance of health. Yet the body however it be brought low by disease and the same cause which was formerly required for the body's preservation becomes the means of its destruction. A draft which at one time helped to promote a healthy circulation brings about hopeless bankruptcy. The pulse rate in such cases then, must be taken as indicative of no particular disease but as the result of a condition more or less common to all diseases. We may now close what has been presented by saying regarding the law of exhaustion, to return to it when we have had under consideration the Law of Diminished Contractility.

Chapter 7

Law of Diminished Contractility

We proceed now to consider the law
of Diminished Contractility which affirms "that interference with the contractile tissue of the heart may be compensated for by decrease in the pulse rate." Just as in the law of exhaustion an attempt has been made to connect a certain condition of the body with a rise in the pulse rate, so in this law an attempt will be made to show the bond which connects a certain condition of the heart with a fall in the rate. The various ways in which the contractile power of the heart may be interfered with are: 1st. Fatty degeneration. 2nd. Fibroid disease of the heart. 3rd. Softening of the heart pyeophelial or non-pyeophil. Of these pyeophelial softening will fall to be considered under the law of pyeophelia; and the cases of non-pyeophelial softening are so few and so imperfectly reported that they may be dismissed from consideration. Cases of fibroid degeneration are also extremely rare so that the evidence available for deciding what effect on the heart's functional activity is produced by an interference with its contractility must be mainly furnished
by the phenomena presented by cases of fatty degeneration. These though neither so numerous nor so accurately recorded as might be wished will I think suffice to decide the question.

Before however turning to the cases themselves let us look for a little at the opinions generally held as to the connection between fatty degeneration and the pulse rate. Taking Reynolds's System of Medicine as the most recent work we find it stated in the Article - Fatty degeneration (Vol. IV page 779) that the pulse may be regular throughout but is often irregular. Sometimes it is frequent even to an extreme. It may be slower than natural and the diminution in frequency may proceed to a degree not met with in any other affection. We may therefore have in cases of fatty degeneration three main varieties of pulse. Speaking of these Dr. Yule's Diseases of the Heart, 1st edition page 341 says The mechanism of infrequent action has already been discussed and the mystery attached to it admitted. But it is yet more difficult to fix what
are the conditions in different cases especially productive of the three main varieties of pulse just described. The difficulty here marked is the one which if the law of Diminished Contractility is to be proved must now be faced.

That the conditions upon which these varieties of pulse depend have not been fixed, is mainly due I apprehend to the fact that they have not been sought for where they are likely to be found. In the few attempts which have been made to explain them it seems to have been assumed that the variations are to be attributed to different degrees in the structural change, or to the state of the heart before the degeneration began (Stokes' Diseases of the Heart, page 376). A little reflection however will show us that we must seek for these conditions in a much wider area. Fatty degeneration cannot be studied as a disease by itself. Of all pathological products it is the one which is found amid the widest diversity of accompanying circumstances. It may
be found with Arsenia, with Perniosis, with Phthisis, with Cancer, with chronic suppuration. It may be found with the various febrile affections, erysipelas, enteritis, fever, small pox, and measles. It may even complicate a case of miliaryosis. These are only a few of the many diseases in which post mortem examination may bring to light a fatty heart. So exceedingly frequent did the connection with other diseases appear to be that Dr. Ormerod doubted whether fatty degeneration could be considered a primary disease at all (London Medical Gazette for 1849, page 921). There are, however, a good many cases which would seem to prove that degeneration of the heart's fibres into fat may be the only departure from the normal state which is recognisable either during life or after death. For the consideration of the connection between the pulse rate and the state of the heart the distinction here drawn between primary and secondary degeneration is of great importance. It is evident that
those diseases or conditions which we know affect the pulse rate when there is no fatty degeneration, will not cease to do so when to the other morbid states there is added this change in the muscular structure of the heart. The effect on the pulse rate in such a case will be due to the action of the original disease or condition plus the effect of the fatty degeneration.

A very striking illustration of the effect of the action of two causes upon the pulse rate will be found in a case reported in the 3rd Volume of the Transactions of the Pathological Society of London (page 267). In this case the pulse is said to have become irregular and uncertain eighteen months before death. In an attack of Bronchitis which supervened this irregularity and uncertainty disappeared only to reappear when the Bronchitis had gone. Now had this patient been under observation only during this attack of Bronchitis and had she died during its course we might have had this case recorded as
one of fatty degeneration with regular pulse. The fact that she was a lady of rank probably accounts for the more accurate history of her case which we fortunately possess.

The practical issue of all this is manifest. If we are to fix the conditions on which the varieties of pulse rate observed in cases of fatty degeneration depend we must begin by excluding all those in which the pulse rate is due in part at least to conditions with which fatty degeneration has nothing to do. This will limit us in the first instance to those cases in which the degeneration seems to be primary. It must not be supposed however that cases in which the degeneration is secondary will not assist in the solution of the problem. Such cases will serve as the best test of the correctness of any hypothesis we may frame as to the law which obtains in the simpler cases. For if our hypothesis are correct we should be able to ascertain by deduction the effect which will be produced
by the action of two Laws in a complex case. In the meantime however we must examine the facts revealed by the simpler cases on which the Law of Diminished Contractility is based.

Of the records of one hundred and forty eight cases of Fatty degeneration which I have examined, a considerable number have no record whatever of the pulse rate. More especially is this true of those cases recorded within the last twenty five years. A still larger proportion are complex cases, in which the Fatty degeneration is accompanied by other conditions which deprive the pulse rate of any value for our present purpose. Of the remainder, twenty four in number, eleven are described as having a permanently slow pulse, eight as having an irregular pulse, three an intermitting pulse and two a rapid pulse. Before considering the relation which these various kinds of pulse bear to each other and to Fatty heart, let us look at the records of those cases in which there has taken
place the analogous change of part of the heart's muscular tissue into fibres. If interference with the muscular tissue of the heart is to explain any modification of its functional activity, it will matter little whether that interference be due to fatty or fibrous tissue. In both cases there is a substitution of non-contraction for contractile elements. Now of five cases recorded in the Transactions of the London Pathological Society, in which there was either this degeneration of the heart's muscular tissue, or partial displacement of it by fibrous deposits, two had a permanently slow pulse, two had an irregular pulse, and one an intermittent pulse.

It is unfortunate that of the words used to describe these various pulses one of them is not only ambiguous but doubtful. Irregularity as applied to the heart's action may mean irregularity in the force of the beats, in the intervals, or in the contractions. Seeing that these irregularities may occur with a pulse rate at, above, or below the normal it becomes
a question in permutations and combinations. How many varieties of pulse there may be which can be described as irregular. With these variations in the force of the beats we have no concern, so that the term irregularity must be understood here as meaning irregularity in the length of the intervals, or the contractions. What meaning we attach to these will be explained after we have considered what is implied by permanent decrease of the pulse rate.

If we examine the records of these cases which have a permanently slow pulse, one fact is brought into striking prominence. In several of them the degeneration seemed to have existed with a very tolerable amount of health. Thus in a case recorded by Dr. Traill, Fatty Diseases of the Heart (page 67, case 64), it is evident that the man must have followed his employment for at least two years with a pulse rate between 24 and 30. In another case referred to by Dr. Banon (in the Dublin Medical Press for 1849, page 373) in which the pulse was 28, the patient seemed to
enjoy tolerably good spirits the day before his death. In another case of Dr. Launay, recorded in his memoir [page 56 series 1st base 30], the patient seems to have worked up till his death, as he died while drawing a truck. This last might have been extended had several of the cases recorded been under observation for a longer period. The presumption is strongly in favour of the idea that a slow pulse and fatty Degeneration may co-exist long before they come under the notice of the Physician. I have been told by a medical friend of a lady whose pulse has been known to be 28 for several years. She enjoys tolerable health, but is unable for much exertion and becomes very breathless on ascending heights. I have lately through the kindness of my friend Dr. Affleck seen an old man whose pulse has been 28 for a considerable time. Though 70 years of age he carries on the trade of a Metal merchant and enjoys fair health. The only other noticeable feature in his case is the shortness of the first sound. This as we shall see,
when speaking of softened heart is a symptom of considerable value.

How this connection between change in the structure of the cardiac muscle, a slow pulse, and a fair amount of health can hardly be accidental. The only feasible interpretation of the facts is to regard the slowing of the heart’s action as a compensatory method of working by which it accommodates itself to the altered circumstances. The idea of compensation as applied to the heart is not new. Hitherto it has always been associated with hypertrophy. When hypertrophy takes place the heart is supposed to alter, at all events to add to its structure, in order to meet altered conditions of functional activity. There is more work to be done and there is in consequence developed more muscle. Now what takes place in a heart in which non contractile elements have more or less taken the place of contractile is the converse of this. There is less muscle and consequently there is less work done. In other words the impairment in structure is compensated for by diminution of functional
activity. It may perhaps be said that a much more simple explanation is open to us. A fatty heart beats slowly because it is unable to beat in any other way. Now a study of those cases in which this degeneration is secondary to other conditions shows us that, in point of fact, the majority of fatty hearts do beat fast. The explanation of inability therefore is inadmissible. This modification of functional activity implies a regulative action, a readjustment to new conditions. The most pregnant aspect in which to view the pulse rate in health is to regard it as a mean between two extremes. On the one hand it must be rapid enough to enable the organs to obtain blood in amount sufficient for the performance of their various functions; while on the other hand it must not be so rapid as to prevent the heart from obtaining the rest necessary for a due renewal of energy. Now in a case of permanently slow pulse the mean is altered by an alteration in both these extremes. The organs perform their functions no doubt, but there is an imperfect
ment of rigor. Clinical experience proves that health in such cases is maintained on condition of an abandonment of all extraordinary efforts. The effort of defecation is not without peril, as is proved by the frequency with which death in such cases takes place in the water closet. The period of rest is usually lengthened, or it may be that the time which the heart takes to contract is increased. It is not improbable that in cases which proceed far in degeneration list these changes take place. This corresponds with what is observed to take place in a piece of muscle which is made to contract by electricity. Exhaustion of muscular irritability is shown by a prolongation of the time taken by a muscle to contract, as well as by lengthening of the latent period preceding the contraction. The experiment is thus described in Dr. Burdon-Sanderson's Hand Book of the Physiological Laboratory (page 360): Having determined with a single induction shock the natural curve, exhaust the muscle by prolonged or repeated stimulation with the interrupted current, and then repeat
again with the same single induction shock as before. The curve will not only be of less height but will be longer, i.e., the contraction will be slower and the latent period especially will be prolonged. A muscle out of the body in which the contractility has been exhausted in part, i.e., as far as function is concerned, in the same position as a muscle in which there has taken place a substitution of non-contractile for contractile tissue. In both cases the sum of the contractile power has been reduced in such a way as to render its renewal up to the original amount impossible. The only difference between the two cases is, that in the exhausted muscle the loss of contractility is equally distributed among the elements; whereas in a fatty or fibrous heart, the distribution is usually unequal. It is a fair inference from the evidence afforded by this experiment, that just in proportion as the degeneration implicates equally the whole structure, the compensatory change in the heart's activity— if it take place at all— will manifest itself by a prolongation of both diastole and systole. Histologic anatomy shows
however that degeneration of all parts of the heart equally is very rare. It is probable that in some cases lengthening of the period of rest may be sufficient, while in others, for reasons which cannot even be guessed, the lengthened time occupied by the contraction may serve the same end. In the case of the old man noticed at page 73, the slow pulse is due to the lengthening of the interval.

The time occupied by the contraction is that this might be compared with a case of probable fatty degeneration recorded by Dr. Kennedy (Dublin Medical Press 1849, page 375) in which the pulse is described as being "peculiar in this respect that it passes under the finger very slowly." To pursue the distinction any length is impossible on account of the imperfections in the reports of cases.

A curious question here arises as to whether this change in the rate of the pulse takes place gradually or suddenly. I have been able to find the record of only one case in which the change was noted at the time it occurred. It is a case recorded at great length by Dr. Murchison in his work on
Continued. Fever (page 260 second edition). Whether, in this case the contractile power of the heart was diminished by structural change or not, it is impossible from the history to say, but it is not at all improbable. The case is too long to give in detail, but the change in the pulse rate is described as having taken place suddenly and on two separate occasions. "One day in March 1858 after seeing some hospital patients, he suddenly felt his heart working in a strange manner--thumping slowly and at the same time he experienced a feeling of giddiness. The pulse was barely 40 its rate since attack of fever is uncertain but it is believed to have been normal." After a fortnight's illness the heart began to beat at its usual rate. The record continues "he continued well until the following October when one morning while sitting in a friend's house he suddenly felt his heart change its action and feeling become giddy. From that time to present date (1872) pulse has varied from 34 to 36 and once was as low as 33." Whether this be a case of disease of the heart's structure
or not it may be compared with the following abridged record of a case from the Third Volume of The Transactions of the Pathological Society of London (page 171). A gentleman aged 57 had served 30 years in India, where he suffered frequently from intermittent fever, and occasionally from diarrhoea. After his return to England he enjoyed tolerably good health till the 11th of January 1837, when having got over an attack of acute rheumatism supervened. On the 14th of the month he was seen by Dr. James Reid who reports the case. As there is nothing remarkable in any of the symptoms except the pulse rate it is enough to say that by the 20th the attack of rheumatism seems to have subsided. The variations in the pulse rate which took place from the 11th of January till his death on the 6th of February, are full of interest. On the 17th the pulse is described as being 115, irritable and jerking. On the 20th he had been excited the pulse was feeble and intermittent at every beat. On the 22nd he was recovering and the pulse was 98 but without intermission. On the 27th he complained of cardiac uneasiness.
the pulse only numbered 85 and was intermittent. On the 28th the oppression at the chest was gone, pulse 84 and regular. On the 1st and 2nd February he was apparently better, pulse 66. After having passed an excellent night and eaten breakfast with a relish, an attack of syncope supervened about noon in which he died. Post mortem revealed fibrous degeneration of the heart. This case illustrates strikingly the efforts which the heart sometimes makes to arrive at a mean, though unsuccessful. Death, consequently ensued when to all appearance the functions of the organs were being well performed. The appetite and digestion seemed perfectly good; and one examination of the pulse would have given no hint of anything being wrong. The day before his death there was neither intermission nor any apparent abnormality. This should make us cautious in founding anything on records of cases which contain only one notice of the pulse rate. This case moreover illustrates one form of irregularity which it is of importance to discriminate. Irregularity was defined as meaning inequality in the length
of the intervals or of the contractions. This inequality may be of two kinds. It may be in the length of individual beats, or a series of beats. Thus as in the above case where the inequality was of the latter kind, within a short time the pulse varied from 65 to 86. The importance of noticing this lies in the fact that, although both forms are usually combined, in which case one examination will reveal the irregularity, yet sometimes only the latter being present we become aware of the difficulty under which the heart is acting only after several observations of the pulse rate. Irregularity of pulse rate of both kinds, most probably indicates that the heart has failed to accommodate itself to new circumstances—to arrive at the mean between the two extremes.

Such being the meaning of slow and irregular pulse, what meaning are we to attach to an intermittent pulse. This will depend entirely on the regularity or irregularity with which the intermission occurs. An irregularly intermittent pulse has probably much the same significance as an irregular
pulse. A regularly intermittent pulse, provided the number of beats be below the normal rate, is much the same as a permanently slow pulse. One way in which to regard a permanently slow pulse, say 30, is to regard it as one which being originally 60 has dropped every second beat. That this is the way in which a permanently slow pulse sometimes arises is rendered probable by what is recorded in some cases of intermittent pulse. Thus in a case of fibr oid degeneration recorded by Dr. Ogilvie in the 8th Volume of the Transactions of the Pathological Society of London (page 118) he says the pulse fluttered at every third beat having then so very feeble as to be almost always, and often, quite imperceptible. This may be compared with a case of slow pulse recorded by Dr. Stokes in which he noted occasional semi-beats between the regular contractions, very weak, unattended with impulse and corresponding to a similar state of the pulse which probably amounts to about 36 in the minute; the evident beats being only 28 so that there must be about
eight of these semi-beats in the minute.

It only now remains for us to explain under what circumstances a heart in which there has been interference with the contractile tissue will beat rapidly. As this involves a comparison between the law of exhaustion and the law of diminished contractility we reserve it until we have considered in another chapter the relation between these laws.

Chapter III.


To any one who has followed the argument thus far it may appear that in the word exhaustion we have one, which is of a somewhat negative meaning and one to which it is somewhat difficult to attach very precise ideas. We shall therefore in this chapter attempt by contrasting it with the effect of loss of contractile power in the heart, to give greater exactness to
the term than has hitherto been possible.

Physiologists are fond of comparing
the human body to a machine. A more
just conception of the organism as a
whole would rather regard it as a series
of machines. Some of the conditions under
which these act, such as a due supply of
blood are common to them all; while other
conditions are peculiar to individual parts.
This is specially the case as regards the
relation which the amount and kind of
work bears to the rest which the various
organs obtain. In this respect the heart
differs strikingly from the brain or the
voluntary muscles. Unlike them, its periods
of rest are excessively short. It cannot make
up for an excessive expenditure of energy
by a correspondingly long cessation from
work. From the very dawn of the organism's
existence—while yet structurally incomplete,
till the close of life—it never ceases to
puelate. How this continuous activity can
only be carried on by a due supply of
energy. If the organism is to maintain
its existence, whatever part may suffer,
the force necessary for carrying on the heart action must be found. The function of the brain may for a time be almost in
disappearance. There may not be sufficient power to move a voluntary muscle, still the heart must go on beating. The minimum amount of force necessary for carrying on in their integrity purely vital functions is not determinable, but it is evident that a minimum there must be. How the state of exhaustion may be defined as being that in which the amount of force is below this minimum. By this it is not meant that in an exhausted organism there is no energy. There may be a considerable amount, but it may just fall short of that which is necessary for the proper performance of these vital functions. The causes which have reduced the amount below the minimum may be temporary as in exercise. If the drain be kept within due limits the organism rapidly regains its equilibrium and no harm ensues. It is when the causes are perma-
ment that the condition becomes serious
and of importance clinically. Now in the law of exhaustion an attempt was made to show that when the balance of the organism is thus disturbed, the pulse rate becomes rapid. The distinction between exhaustion and debility will help to illustrate the points with which we are dealing. Debility simply means great weakness, and this may exist without necessity there being any exhaustion in the sense in which the word is here employed. Thus a patient in whom the crisis of Syphilis Fever has just passed may be in an extreme state of debility but if with the fall in the temperature the pulse has also fallen to the normal rate, he is not in a state of exhaustion. Though it is perhaps with the greatest difficulty that he can move a voluntary muscle, yet the physiological balance has been maintained. How very near such a patient is to the state of exhaustion may be understood if we reflect on what would happen, were he to stand up. His pulse would immediately rise 40 or 50 beats; in other words by the mere
change of posture he would pass from a state of debility to a state of exhaustion. The establishment of the conjunction of rapidity of pulse and disturbance of the physiological balance is what mainly concerns us as clinical observers. Even had we no conception of the way in which this is brought about, the diagnostic value of the fact would hardly be of less value to us. Let us however see in what relation these results of clinical observation stand to current physiological doctrines. To which element in the heart are we to attribute this change in the rate of its action? Is it to its muscular structure, or to its nervous supply? The relation subsisting between these two are too complex and as yet too obscure to afford any complete answer to this question. It is possible, however, even in the present imperfect state of our knowledge to indicate the direction in which an explanation is likely to be found. A priori it seems improbable that a tissue should manifest deficiency of energy by increased functional activity. This consideration would of itself
lead us to presume that rapidity of pulse is not in the first instance at least, to be taken as evidence of any loss of contractile power in the heart. The Balbian doctrine of independent muscular irritability makes it intelligible how the balance of the organism may be disturbed, and yet a considerable amount of force remain in the muscle. A priori considerations would lead us to look to the nervous system in its relation to muscle for the desired explanation. The theory of this relation which seems to satisfy the requirements of physiology will I fear be found hardly broad enough to account for the facts with which disease brings the Physician into daily contact. All modes of expressing the relation seem defective in not giving sufficient prominence to the regulatory function possessed by the nervous system. Thus Dr. Foster (Text Book of Physiology page 679) says when a muscle is thrown into contraction by a stimulus applied to a nerve three distinct events take place:—

1st. The generation and propagation of the
nervous impulse along the nerve.

2. The conversion of this nervous impulse into a muscle impulse and the propagation of the latter along the muscular fibre.

3. The contractions started by and following upon the muscle impulse.

How all this is no doubt perfectly true so far as it goes, but I can hardly help thinking that it leaves out of sight the most important element in the whole process. It shows us how a muscle may begin to contract, but how about the ending of the contraction thus begun. It will be said that the contraction will end when the muscle impulse — the transmitted nerve impulse — is exhausted. But what regulates the intensity of the impulse originally generated? It may be argued the nerve impulse has a relation to the stimulus which the afferent nerve conveyed to the centre. But this is true only to a comparatively limited extent, the number, intensity, characters, and distribution of the different impulses is determined chiefly by the events which take place in the protoplasm of the reflex...
centre (Huxley's Physiology, page 49). This seems to get nearer the root of the matter. It brings into prominence the essential facts in the relation between nerve centres and other organs. With reference to a muscle this relation may be analysed into two main elements:

1st. The initiation of the contraction or the liberation of the energy contained in the muscle.

2nd. The determination of the amount and direction of the energy which is thus liberated. This implies the power of bringing the contraction to an end.

Now of these two elements the latter seems by far the most characteristic and important. There are many things by which muscular contraction can be initiated but there is only one by which it can be controlled. Observation of disease supplies daily proof that deficiency of nervous power is manifested by defective control. The woman suffering from Hysteria or the man from Delirium Tremens finds no difficulty in generating motion. There is too
great a genesis; the difficulty is to keep the motion thus generated within bounds. The inadequate notion of the relation of nerve to muscle will be found to be mainly due to an imperfect appreciation of the Bellian doctrine. A glance at the terms in which this relation is described even by very able writers will bring this out with great clearness. Take as an example the following sentence from Sir James Paget's Brooman lecture on the rhythmic nutrition of the heart (Proceedings of the Royal Society of London for the year 1857, page 1479). The rhythmic action depends on certain nerve centres in the nerve ganglionic of the heart which centre by spontaneous discharge of nerve force cause the muscular structures to contract. Now to speak of a muscle as being made to contract by the discharge of nerve force into it, is no doubt in a certain sense true, but it is only a very small part of the truth. It is intelligible only on the analogy which is supposed to exist between the action of the nervous system and an electric battery. Such a conception of nervous
action is inconsistent with the facts of the case. Its inconsistency, even so far as it would represent the nerves as being analogous to conducting wires, has been exposed by W. Lewes in his recent Work, "The Physical Basis of Mind."

Nor would it be difficult to show how imperfect is the analogy between a nerve centre and a battery. If we are to employ analogy at all the nearest approach to a just conception of the relation between a nerve centre and a muscle will be found in the study of the action of a rider and a horse. Like all analogies it must not be pushed too far, but it will bear a much closer scrutiny than that of a battery. The control which the nerves exercise over the muscles, is at least equal if not greatly in excess, of what the most skilful rider has over his steed. The muscles too possess in themselves an innate power as independent of the nervous system, as is the power of the horse from that of his rider. It is true that the nervous system besides controlling
also initiates the contraction; but this does not destroy the analogy. Now if we try to describe the horse and his rider in the terms which are usually deemed sufficient to express the relation between muscle and nerve, we shall see how ludicrously inadequate these are. A horse has a centre of influence seated on his back, which by spontaneous discharges of force causes it to move. This cannot be said to be absolutely untrue; but the picture which it brings before the mind bears a much closer resemblance to a rocking horse, or a velocipede, than to that noble animal whose strength is so wondrously under man's control. Who could ever gather from such language, that the pace—the regulated motion—was the result of two forces acting in opposite directions. For control it must be remembered is not effected by the discharge of force into the object controlled, so much as by the agency of a contrary force of a superior kind.

If we turn now to the heart we shall find in the modifications of its action
considerable corroboration of the truth of this conception. So long indeed as we view the heart as the mere passive recipient of "spontaneous discharges," any explanation of the apparent paradox which cites rapidity of action of one organ as evidence of the weakness of all the others is plainly impossible. When however we look upon the heart as being habitually curbed up (Foster's Physiology page 126) as being supercharged with motility, the explanation becomes comparatively easy. The main duty of the nervous system is not the initiation of motion in this highly charged organ. It is to control the expenditure of its power. Its rate of action has to be moderated with a due regard on the one hand to meeting the requirements of the organism and on the other to the renewal of the energy which has been expended. Then therefore we find a rapidly beating heart produced by circumstances which have exhausted the energy of the organism; this must be understood as meaning a failure of controlling power. It may be asked:
why should the controlling power fail so much more frequently than the muscular. The reason for this will be apparent if we reflect, that while the muscle is resting in diastole, the nerve centres are probably as active then, if indeed not more so, than in the systole. On them in all probability depends the determination of the length of time during which the heart remains inactive. In other words the functional inactivity of the muscle is the result of the functional activity of the nerve centres. This corresponds with what is observed to take place in a heart from which the vitality is gradually exhausted. The mere motility is found to remain after all power of rhythmic motion i.e. of controlled motion has been lost.

"It is interesting to observe" says Sir James Paget (Proceedings of the Royal Society of London for 1857 page 477) in these experiments three different modes of action of the heart.

1st. The truly rhythmic in which the contractions of its several parts or of some of them when separated ensue spontaneously
i.e. without evident stimulus or change of external conditions and observe a definite order and proportion of time.

And such as may be excited by stimulus which beginning at the cavity stimulated whichever it may be, are affected simultaneously or with a scarcely appreciable difference of time by all parts of the walls of the cavity; then follow with a certain interval in other cavities; and then are not repeated in the same order but give place to the true rhythmic action which they may for once have prevented or inverted.

3rd. Those which ensue on stimulus when the heart or any of its parts is utterly exhausted and which commence at the point stimulated and thenceforth travel once to all other points connected with it by continuity of muscle.

We are now in a position to understand the relations between the law of Exhaustion and the law of Diminished Contractility. They stand in direct antagonism to each other. The law of Exhaustion has as
its condition disturbance of the physiological balance. The law of Diminished Contractility implies that this balance, if disturbed has been restored. The former implies complete or partial allegiance of the controlling power, the latter its complete integrity. Hence they cannot both be operative in one person at the same time. They are mutually exclusive of each other. If this be so the question at once arises as to what will happen in a case in which exhaustion is combined with interference with the contractile power of the heart. As we have just seen, the law of diminished contractility can have no force in such a case. There can be no compensatory slowing as that depends on an exercise of nervous power inconsistent with the conditions from which the exhaustion has arisen. Hence we see how universally applicable is the law of exhaustion. The structurally impaired heart need more or less from control acts much in the same way as if it were structurally sound. This is seen, for example in cases of fatty degeneration which are
secondary to other conditions, such as anemia. In the majority of such cases
the heart's action is rapid. While this is so as a general rule there may be
cases presenting all the symptoms of exhaustion in which the pulse rate may
not rise above the normal. The heart no
more than any other organ is free from
the condition that the amount of force
expended increases in geometrical rate
to the rate of expenditure. Consequently
it is conceivable and experience bears out
the idea, that a Fatty or Fibrous heart
may not possess sufficient power to
increase to any marked extent the rate
of its action, even when entirely freed from
control. This goes to show that as an
index to the condition of the body the
rate of the heart's action will sometimes
fail us. When properly looked at however
this apparent exception to the law is not
without practical value. For if amid
a number of symptoms which go to prove
that the organism is exhausted the heart
alone gives no sign there is strong reason
for suspecting some structural change in the walls of that organ. The observation of this want of correspondence between the heart's action and the other symptoms will often materially assist in the diagnosis of fatty degeneration. A somewhat analogous want of correspondence in symptoms in cases of fever will come under our notice in the next chapter.

Chapter IV.
The Law of Pyrexia.

We proceed now to the consideration of the 3rd Law, which affirms the invariability of the connection between a rise in the temperature of the body and an increase of the pulse rate. We have on more than one occasion noticed the difficulties which lie in the way of accepting this law as being universally applicable. Though there is a general correspondence between increased temperature and quickened pulse, yet there are apparent exceptions which would seem to show that the temperature may rise
and yet the pulse rate be unaffected. The manner in which this fact is usually described by such authors as notice it would lead to the belief that they regard it as a real not an apparent exception. "Another fact," says Dr. Murchison (Continued Fevers, Second edition, page 579), not so generally known as it ought to be is that enteric fever may run its entire course with a pulse little, if at all, above the normal standard of frequency (60 to 80) the temperature however reaching 103° or 104°."

If we examine the phenomena which are presented by the severer forms of fever we shall see that the conditions implied in the three laws are found in many cases. There is increased temperature from the first. That many of the cases die from depression of the vital powers, i.e., from exhaustion, both before, and after the crisis, is beyond a doubt. There is diminution of the contractile power through the softening of the heart, which has been so frequently observed. What this softening
is was not very clearly understood till within a comparatively recent period. The presumption that it is due to disorgan-
isation of tissue, has been confirmed by microscopie observation. Moreover it has been shown that in some cases where consistence is not markedly altered there is a change in structure sufficient to interfere very seriously with the contractile power. A description of four such cases and their connection with vascular changes in the smaller arteries and capillaries is given by M. Rouyer in the 466, November 1869 of Archives de Physiologie. The most noteworthy fact in these cases was that although partial softening was observed in some of them yet the granular appearance of the fibres, and the changes in the blood vessels were not found to correspond with these softened spots. Hence in any given case of fever it is impossible to affirm, from a mere examination by the naked eye, that the heart is structurally sound.
forms of fever we have all the conditions implied in these laws let us see how these conditions act, and react, upon each other when found in the same case confining our attention in the first instance to two of the three conditions let us see how far we can determine the manner in which the heart's action will be affected in a case where to increased temperature, there is added exhaustion of vital energy. Will this addition merely make the already quick beating heart beat more quickly? Is the action cumulative? or, does the one cause supersede the other? Any a priori answer to these questions in our present state of ignorance as to the true meaning of an increase of temperature is impossible. The facts of the case however render it probable that like the law of diminished contractility the law of Pyrexia is subordinate to the law of exhaustion.

While acknowledging our inability to comprehend fully the relations between exhaustion and increase of temperature.
yet from the mere fact that there exist in cases of fever two causes producing the same effect precisely, we can draw several inferences of considerable practical value. In the first place it is manifest that in a particular case it is impossible to decide whether the pulse rate is attributable to the Pyrexia, the Exhaustion or to both combined. In other words till the crisis has been reached, the pulse rate has no diagnostic meaning. Secondly as exhaustion undoubtedly influences the pulse rate it would be unreasonable to expect that between the variations in the temperature, and the variations in the pulse rate, there should be an exact correspondence. It doubt in the milder cases, and the earlier stages of severe ones, such a correspondence may be looked for but as the gravity of the case increases a divergence will become more and more evident. That this is so is proved by clinical observation. Although at first the pulse and temperature mostly run together it is important to note that there is no
definite relationship between them, and indeed during the second week it often happens that the pulse is rising while the temperature is falling. (Dr. Murchison's Treatise on continued Fever page 140).

As long as we cannot determine whether the pulse rate is to be taken as indicating exhaustion or, Kupfer purport, it is as we have seen diagnostically of no value. With the fall of the temperature however the dulness ceases. If coincident with this fall the pulse rate also falls we know that severe, as the strain on the organism may have been, yet it has been able to bear it. If on the other hand the pulse continues rapid after the crisis, we know that the patient is still in a dangerous condition. It is usually said that with a rapid pulse under such circumstances complications are likely to arise; and that such is the probability is no doubt true. The explanation of this liability to the occurrence of complications is not im-

probably the same as that already
given of the tendency to haemorrhage which characterises labour cases, in which the pulse remains high. The organism gives way at its weakest part. In labour cases the part to yield is determined by the activity so recently manifested by a particular organ. What determines any special complication which follows a case of fever is much less easily apprehended; but there can be little doubt that the link which binds it to the high pulse is the exhaustion brought about by the gravity of the attack.

Let us now see what effect on the pulse rate is produced by that interference with the contractile power of the heart which has been already noticed. This will depend entirely on the absence or presence of exhaustion. As we have already seen when exhaustion is present the main condition influencing the rate of the heart's contractions, is its ability to contract. Consequently in the majority of severe cases, in which dissection shows a softened heart, the action during life
has been rapid. According to Dr. Walsh this is so not only of the majority but of all such cases when acutely produced under circumstances of purpura, such as these, softening is invariably attended with increased frequency of pulse. But surely this is a mistake, for the extremely softened state of some parts of the heart wall of itself make us doubt their ability to beat fast, and clinical observation confirms this doubt. Thus in his work on Syphoid Fraser Louis states that he had eight fatal cases in which the pulse never rose above 90 (Recherches de Fr. or Syphoid edition 1841 Volume p. page 137). How in the record of the post mortem of one of these cases (ibid Volume 2nd page 68) it is stated of the heart that the walls were thin and much softened. Suppose however that the law of exhaustion is not operative, only the law of purpura what effect will then be produced on the pulse rate by a softened heart? Is it possible under such circumstances for a compensatory modification of action
to take place? From the record of such a case as that already noticed at page 68 we see that it is possible for a fatty heart to have its action modified by increase of temperature, without of necessity the case proving fatal. A case still more to the point will be found in the Dublin Medical Press for 1849 (page 371). Dr. Kennedy who records the case as one of fatty degeneration, makes the following observation regarding the pulse rate: The patient had concealed the fact that he was subject to attacks of syncope so that fatty degeneration was not suspected. "The only point which appeared to me strange during his lifetime was this: that during the attacks of either the gout, or synanche, though the other symptoms of fever were very considerable, the pulse never rose above 76; the healthy standard being in this case about 56 and there being no intermission." How may it not be that this case gives the key to the apparently normal pulse which is so frequently seen in fever. A single exam-
ination of this case, while suffering from gout or syncope, would have shown us a heart beating apparently in a perfectly normal manner and yet the apparently normal pulse rate of 70 so far from being a normal one was in reality a complex result arising from two abnormal conditions. There was first of all the permanent slowing due to the fatty degeneration and to this was added the temporary increase due to the pyrexia. Now the analogy between this case and that of a case of fever with softened heart is very close. In both there is interference with the contractile power of the heart. In the softened heart it is true the interference is temporary but in cases which recover, may this recovery not he partly due to a temporary slowing of the heart's action by which the damage done to its walls is compensated for. Supposing such to take place the pulse rate but for the pyrexia might fall to any point between 60 and 80. Let 20 or 30 beats be added for the
pyrexia and we have the apparently normal pulse of fever arising as in Dr. Kennedy's case from two abnormal conditions. How were it possible to diagnose with precision cases of softened heart we could bring this hypothesis at once to the test. Cases of softened heart which sooner ought to be marked by a slow pulse, and the access of the softening by a fall in the pulse. Unfortunately however there are no pathognomonic signs of such a lesion. Dr. Stokes pointed out that diminution or cessation of the first sound very frequently characterised such cases. This however, is not invariably the case. It is a remarkable fact and one which goes a considerable way to confirm the views of the pulse rate here advocated. That in four cases which Dr. Stokes (Diseases of the Heart and Arteria pages 386, 389, 393, 407) diagnosed as having softened heart but which recovered there was in all of them a period during which the pulse rate did not appear to be much above the normal. Not only so but it fell
to this rate after having been much higher. Dr. Stokes looked upon this fall as a sign of improvement, and attributed it to the free use of stimulants. Whether this be so or not is immaterial to our present purpose. While an improvement is thus marked by a fall in the pulse, it is natural to expect that an opposite condition should be marked by a considerable rise. Suppose that a compensatory action of slowing has taken place in a case, but this action has failed. According to our theory, this failure ought to be marked in some cases by a leap from an apparently normal pulse to a high one. The record of a case in which such a leap was made from a pulse of 80 to one of 180 will be found in Dr. Murchison's treatise on Continued Fevers (page 124). Post mortem showed the texture of the heart softened and pale.

With this case I bring to a close this long paper. One chief difficulty has been to prevent it from being much
longer. To compass it within reasonable limits I have been compelled to indicate rather than to record in detail many cases which have a direct bearing on the subject.

Whatever be the fate of these speculations I cannot conclude without expressing my profound conviction—a conviction which the study necessary for the composition of this essay has only deepened—that the variations in the pulse rate are yet destined to be of the highest service to Medical Science.