established. Other tables show a close relationship between the bright lines of the nebula and the dark lines in the so-called Orion stars, of which Rigel and Bellatrix are typical examples.

The following are the conclusions to which the investigation has led: (1) The spectrum of the nebula of Orion is a compound one, consisting of hydrogen lines, low temperature, metallic lines and flutings, and high temperature lines. The mean temperature, however, is relatively low. (2) The spectrum is different in different parts of the nebula. (3) The spectrum bears a striking resemblance to that of the planetary nebulæ and bright-line stars. (4) The suggestion, therefore, that these are bodies which must be associated in any valid scheme of classification is strengthened. (5) Many of the lines which appear bright in the spectrum of the nebula, appear dark in the spectra of stars of Groups II. and III., and in the earlier stars of Group IV.; a gradual change from bright to dark lines has been found. (6) The view, therefore, that bright-line stars occupy an intermediate position between nebulæ and stars of Group III. is greatly strengthened by these researches.

THE ECLIPSE OF THE MOON.—The earlier phases of the total eclipse of the moon on Monday morning were observed under very favourable circumstances in the neighbourhood of London. The penumbra was not distinctly visible until about ten minutes before contact with the shadow, and the whole disc of the moon remained clearly visible, even at the middle of totality, until clouds stopped observations about 4 a.m. The parts deeply immersed in shadow were intensely red throughout, and with the telescope all the principal formations could be easily distinguished. During totality the sky was exceptionally clear, and numerous occultations were observed without difficulty. For half an hour after the commencement of totality the following edge of the moon was pretty brightly illuminated, and presented a striking contrast with the redness of the advancing edge; at mid-eclipse, however, the whole of the disc was very red.

It is reported that nearly 140 observations of disappearances or reappearances of eighteen stars were secured at the Royal Observatory on Monday morning by the eleven observers who watched the progress of the eclipse.

THE NAUTICAL ALMANAC, 1898.—In the recently published volume of the British Ephemeris for 1898, we note several valuable additions. The places of eleven close circumpolar stars, four of which are in the northern hemisphere, are given for each day of the year, and the mean places of fifty two additional stars for navigational purposes have been added. The improved explanations of the contents will also no doubt be generally appreciated. The publication of an abridged edition for the use of seamen is a step in the right direction.

PHYSICAL WORK OF HERMANN VON HELMHOLTZ.[1]

I

THE career we are to consider this evening was a career of singular distinction. In days when the range of "natural knowledge" is so vast that most workers are compelled to be content if they can add something to one or two of the subdivisions of one of the main branches of science, von Helmholtz showed us that it is not impossible to be at once a great mathematician, a great experimental physicist, and, in the widest sense of the term, a great biologist.

It was but eight months yesterday since he delivered his last lecture; it is six months to-day since he died, and the interval is too short for us to attempt to decide on the exact place which will be assigned to him by posterity; but making all allowance for the fact that each age is apt to place its own great among the greatest, making all allowance for the spell which his name cast over many of us in the lecture-rooms where we ourselves first gained some knowledge of science, I am sure that I only express the views of all those who know his work best, when I say that we place him in the very front rank of those who have led the great scientific movement of our time. This opinion I have now to justify. I must try to convey to you in some sixty minutes an outline of the work of more than fifty strenuous years, to give you some idea of the wide range of the multifold activities which were crowded into them, of the marvellous insight with which the most diverse problems were attacked and solved, and, if it may be, some image of the man himself. The task is impossible, and I can but attempt some fragments of it.

The history of von Helmholtz is in one respect a simple tale. There are no life and death struggles with fate to record. His work was not done with the wolf at the door, or while he himself was wrestling with disease. He passed through no crises in which success or failure, immortality or oblivion, seemed to depend on the casting of a die. He suffered neither from poverty nor riches. He was a hale strong man on whom external circumstances neither imposed exceptional disabilities, nor conferred exceptional advantages, but who, by sheer force of the genius that was in him, passed on from success to success till he was recognised by all as the admirable Crichton of modern science, the most widely cultivated of all students of nature, the acknowledged leader of German science, and one of the first scientific men in the world.

It is the more fitting that this evening should have been set aside for the consideration of the work of Helmholtz, in that England may claim some share in his greatness. Before her marriage his mother bore an English name—Caroline Penn; she was, as her name implied, of English descent. His father was a Professor of Literature in the Gymnasium at Potsdam, so that his early days were passed amid that plain living and high thinking which are characteristic of intellectual circles in Germany. The boy did well at school, and when the time came for choosing a profession, his passion for mathematics and physics had already developed itself. The course of his love for these sciences did not run quite smooth. The path of his ambition was crossed by the hard necessity which in some cases checks, in others fosters, but in all chastens the aspirations of youth. He had to make his livelihood. Science must be to him what the Germans happily call a "bread-study." Medicine offered a fair prospect of prosperity. Physics, in those days, was but an intellectual pastime. And so the young man took his father's advice, and became an army doctor. In this, as in so many other cases, "the path of duty was the way to glory."

It is possible that if von Helmholtz had been what—with a sad consciousness of the limitations it implies—I may call a mere physicist, he would have played a greater part in the development of some of those subjects, the study of which he initiated or helped to initiate, but did not thereafter pursue. It is possible that had he been a biologist, and nothing more, he would have followed up the early investigation in which he disproved the old theory that putrefaction and fermentation are chemical processes only, clearly indicating, if he did not actually demonstrate, that the decay which follows death is due to an outburst of low forms of life.

He might thus under other circumstances have done work for which he showed his competence, but which is now chiefly associated with other names; but it is certain that without the unusual combination of wonderful mathematical power and a professional knowledge of anatomy, he would never have accomplished the special tasks which it is his special glory to have achieved.

His first three papers, however, hardly displayed the fusion between his various powers which was afterwards so remarkable a characteristic of his work. The first two were on biological subjects. The third was the famous essay on the "Conservation of Force." I have told elsewhere the story of the dramatic circumstances under which it was given to the world, of the interest it excited among the members of the Physical Society of Berlin, the refusal of the editor of *Poggendorff's Annalen* to publish it, and the final triumph of the author and his views. (*Fortnightly Review*, November 1894.) Helmholtz was not, and did not claim to be an original author of the doctrine of the conservation of energy; but two young men, Sir William Thomson in England, and Helmholtz in Germany, independently, and within a month of each other, were the first persons who compelled the scientific world to regard it seriously.

There is one interesting fact which connects this essay directly with the Royal Institution. Four years after it was published, it was placed by Du Bois Reymond in the hands of one who was lost to science in the same year as von Helmholtz himself—the late Prof. Tyndall. He was much impressed, and has spoken of the incident as bringing him face to face with the great doctrine of the "Conservation of Energy." ("Introduction to Popular Lectures by Helmholtz," translated by E. Atkinson, 1873.) He translated the essay into English, and

[1] A discourse delivered at the Royal Institution, by Prof. A. W. Rücker, F.R.S., on Friday, March 8.

for many years made it his habit to place every physical paper published by Helmholtz within the reach of English readers.

And now, having brought you to the point at which Helmholtz may be said to have been fairly started on his life's work, let me first briefly describe his official career, before I consider his work in greater detail.

When his extraordinary abilities became evident, he was permitted to sever his connection with the army. At twenty-seven years of age he became Teacher of Anatomy in the Academy of Arts at Berlin. In the next year he was appointed Professor of Anatomy and Physiology at Königsberg, and he held similar posts in the Universities of Bonn (1855-58) and Heidelberg (1858-71). It was not till 1871 that his early love for physics was finally rewarded. When the chair of Physics was to be filled in the University of the newly-founded German Empire, in Berlin, it was felt that even in Germany—the land of specialists—no better occupant could be found than one who was then in his fiftieth year, and had been all his life a teacher of anatomy and physiology. The choice was universally approved and completely justified, and von Helmholtz held this post till his death.

In this connection I am, by the kindness of Sir Henry Roscoe, enabled to show to you a relic of remarkable interest. It is a photograph of the great teacher and investigator, taken at the very last lecture that he delivered—that, namely, on July 7, 1894.

For some years, that is, from the date of its foundation, von Helmholtz was the president of the Physikalisch-Technische Reichs-Anstalt in Charlottenburg. This institution, founded partly by the munificence of the late Dr. Werner Siemens, partly by funds supplied by the State, has no precise analogue in this country. It is devoted to the carrying out of systematic researches on questions of fundamental importance to which a long time must be devoted.

The most characteristic work of Helmholtz was, as I have already hinted, that in which his knowledge of physics and his knowledge of anatomy were both directed to a common end. He dealt in turns with the external physical phenomena, with the mechanism of the organs which the phenomena affect, with the relations between the mechanical effect on the organ and the sensations which it excites, and, lastly, with the connection between the sensations in those simple cases which can alone be investigated in the laboratory, and the complex laws of æsthetics and art.

The two books in which these problems were chiefly treated were the "Physiological Optics," and the "Sensations of Sound." It is impossible to do more than lay before you a sample which may afford some idea of the intricacy of the problems with which he dealt, and of the pitfalls amongst which he walked so warily. For this purpose I have chosen one branch of his work on "Sound."

I have deliberately selected that particular portion which has been most questioned, that on which the verdict of most of those who have sat in judgment on his views has been against him.

In discussing this question I must give a general description of the principal phenomena; but if I were to attempt an exhaustive catalogue of all the facts disputed and undisputed, and of all the theories which have been based upon or upset by them, not only would time fail me, but those who have not given special attention to the subject would, I fear, become hopelessly confused amid the chaos of opposing statements and views. Another reason which urges me to be brief, is that a few years ago Prof. Silvanus Thompson explained the whole subject to the members of the Royal Institution, having kindly consented to act as the mouthpiece of the celebrated instrument maker, König, who has played so large a part in these controversies.

Among the chief achievements of Helmholtz was an explanation of the physical difference between pairs of notes which we recognise as concords and discords respectively. When two neighbouring notes are sounded, alternate swellings and fallings off of the intensity are heard, which are called beats. These produce an unpleasant effect, which depends partly on their number, partly on the relative pitches of the beating notes. When two notes beat badly, they form an intolerable discord. When they become separated by a wider interval, the beats are so rapid that they cease to be unpleasant.

The sense of dissonance produced by many of these wider intervals, such as the seventh (4 : 7), requires further explanation. In general, the fundamental musical note is only the first and loudest of a series of so-called partials, whose vibration frequencies are 2, 3, 4, &c., times that of the fundamental, and the consonance and dissonance of two notes is shown to depend on the presence or absence of beats between important members of these series. Thus in the case of the seventh the frequencies of the octave of the lower note and that of the upper note would be in the proportion 8 : 7, which are sufficiently near to make the beats very prominent and disturbing.

In cases where the notes are pure, that is, are not accompanied by upper partials, the explanation of dissonance is based upon another phenomenon.

When two notes are sounded simultaneously a third tone is often perceived, the frequency of which is equal to the difference of their frequencies. The number of vibrations of this tone is equal to the number of beats, and as there has been controversy as to whether the beats when they become rapid can produce a note, and if so, whether this note is or is not the same thing as the difference tone, it is necessary to distinguish between the two. This distinction is to be found in the mode of their production; but for the moment it is sufficient to remember that they may be distinguishable, and to reserve for them two names, viz. the beat-note, and the first difference tone respectively.

Helmholtz drew attention to the fact that together with the difference tone there is also produced a note, the frequency of which is equal to the sum of those of the two primaries, and this he called the first summation-tone.

Together with these he believed that there existed summation and difference tones of higher orders, the whole series being included under the name of combination tones. Our sense of dissonance between pure notes was explained as dependent on beats produced by the combination tones.

Up to the time of Helmholtz it was generally thought that these tones were produced in the ear itself, and had no objective existence in the external air. They are thus often called subjective, but as that adjective is usually reserved for impressions produced in the brain itself, it is better to say that they were regarded as *ear-made*. Helmholtz himself gave a theory, which showed that it is probable that a membrane like the drum-skin of the ear, which is forced out of shape by pressure, and that bones, like those in the ear, which can rattle, would, if acted upon by two notes, manufacture by their own proper movements all the varied combinational tones which his theory postulated. He therefore believed that combinational tones were largely ear-made.

You will observe that his theory of discord is quite unaffected by the question whether the combination tones are or are not sometimes objective. Provided only they are produced at all, it is immaterial whether they are produced in the ear itself. Von Helmholtz admitted that the phenomena we observe are in most cases ear-made tones; but he also asserted that they were sometimes objective, and could set bodies tuned to vibrate with them in resonant motion. This latter statement has been denied with singular unanimity, sometimes, I think, without due regard to the limitations which Helmholtz himself placed on the conditions under which the objective character of the notes can be realised.

All ordinary calculations as to the production and mingling of different waves of sound are based upon the supposition that the displacements of the particles of air, or other body through which the sound is travelling, are very small. If this is so, the force which tends to restore each disturbed particle to its ordinary position of equilibrium is accurately proportional to the amount of the displacement.

In von Helmholtz' view, objective combination tones were in general produced when the disturbance was so great that this condition was no longer fulfilled. Violence is of the essence of the explanation. Hence the siren, where both sets of holes open into the same small wind-chest—the harmonium, in which two reeds alternately close and open slits in the same enclosure, are the instruments best suited to produce them. Of these the siren is the more efficient. Von Helmholtz convinced himself that the combination tones produced by the harmonium are for the most part ear-made. He expressly stated that "when the places in which the two tones are struck are entirely separate and have no mechanical connection, as, for example, if they come from two singers, two separate wind instruments, or two violins"—to which we may add two tuning-forks—"the reinforcement of the combinational tones by resonators is small and dubious." ("Sensations of Tone," translated by Ellis, p. 157.)

Now this reinforcement by resonators has been altogether denied by most of those who have taken an interest in the matter, while, if an exception is allowed, it is in favour of the beats of a disturbed unison, the observed effects being ascribed to the beats, and not to the difference tone.

Some writers make no exception whatever in their denial of the objective reality of what may be broadly termed secondary tones. Thus Mr. Bosanquet, who made a most careful series of experiments some fourteen years ago, stated that "the ordinary first difference tone . . . is not capable of exciting a resonator. . . . In short, the difference tone of Helmholtz . . . as ordinarily heard, is not objective in its character." (*Proc. Phys. Soc.* iv. 1881, p. 233.)

Prof. Preyer, too, using very sensitive tuning-forks, found that the differential tone given by two forks did not affect a fork the frequency of which corresponded with its own, except in cases where the difference tone was itself a partial of one of the forks.

It must be remembered that the assertions of Helmholtz as to the experimental proof of the objective nature of the tones were made with reference to those instruments which he regarded as most likely to produce objective notes, viz. the siren and the harmonium, and that, therefore, experiments with forks hardly affect his position.

Let us now try with the siren whether it is possible to confirm or to disprove the validity of his views.

For this purpose the rather bulky apparatus which you see before you has been constructed. I should hardly have been able to realise the idea embodied in it, at all events in time to show it to you this evening, if I had not been favourably situated in two respects. In the first place, I have had the zealous co-operation of one of my assistants, Mr. Edwin Edser, who has not only made all the parts of the apparatus that required to be newly made, but has thrown himself into the investigation with the utmost energy, working at it late and early, and making many valuable suggestions and improvements. In our joint work we have been helped by some of my senior students, and notably by Messrs. Cullen and Forsyth. In the second place, I have had at my disposal the magnificent collection of acoustical apparatus in the National Museum at South Kensington, some of which I am allowed, by the kindness of the Department of Science and Art, to bring here this evening.

wave-length of light, the path of the ray which falls upon it is shortened by a whole wave-length, and the position of each band is shifted to that previously held by its neighbour. If the fork vibrates with an amplitude of this almost infinitesmal amount, the bands will disappear, or will alternately appear and disappear according to circumstances. The fork may therefore be used to detect by resonance the presence of vibrations, the frequency of which is 64 *per second*.

A priori, there were two difficulties of opposite kinds which made it doubtful whether the fork would be an efficient weapon for the purpose for which it was to be used.

In the first place it would feel tremors of any sort, and it was doubtful whether it would be possible to discriminate between mere shakes and the vibrations which were to be studied. This difficulty has been very largely overcome.

The table on which the apparatus stands rests on india-rubber. On the table are a pair of library steps; these support two pieces of wood, which are heavily weighted and rest on india-rubber balls. From these two beams hang steel wires, which carry india-rubber door-fasteners, and these in turn support two rods on which the paving-stone is placed. By this alternation of elastic and of heavy bodies we can make the bands absolutely steady, unless the disturbances are violent. The quiet movements necessary for working the apparatus, the blowing of the bellows, and the like, produce no effect. On the other hand, the shutting of a door in a distant part of the building, the rumble of a cart in the street, will cause the bands to disappear. A great deal of the work on which we rely has been done at South Kensington between midnight and three o'clock in the morning. Trustworthy observations have indeed been made at other times, but it is only in the still small hours that the apparatus is at its best.

The second doubt was of a different kind. It was certain that the instrument would be more or less shaken; it was not quite certain whether the fork would respond to vibrations of the given period. It is easy to set a tuning-fork in vibration by resonance when it is mounted on a sounding box, but in that case the vibrations of the enclosed mass of air are communicated through the box to the fork. When the stalk of the fork is held rigidly, a tuning-fork is notoriously difficult to excite by resonance. This objection is, of course, to some extent counterbalanced by the extraordinary sensitiveness of the means of detecting the vibrations, but it is necessary to supplement this by other devices. The instrument used is a siren (S). In front of it is placed a hollow wooden pyramid, the narrow end of

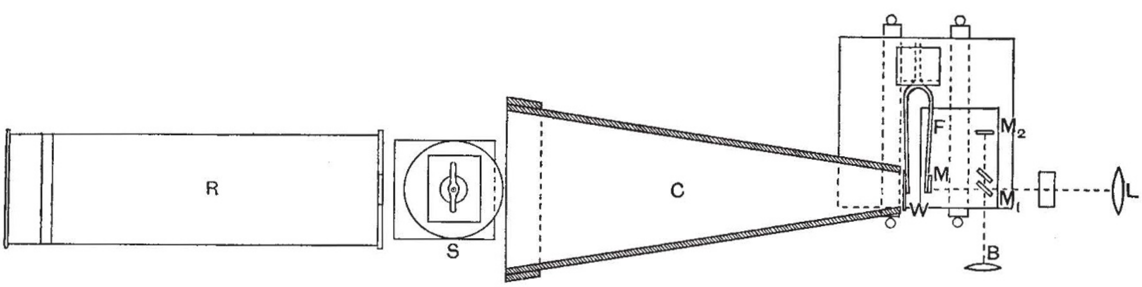

FIG. 1.

The essential part of the apparatus, Fig. 1, is a tuning-fork, F, to one prong of which is attached a mirror, M, and to the other a square of thin wood, strengthened by ribs, which is of the same weight as the mirror. The fork thus loaded has been compared with one of König's large standards by means of Lissajous' figures. Its frequency does not differ from 64 complete vibrations per second by more than one vibration in two minutes. The shank is supported by a mass of lead, which in turn is placed upon a paving-stone. Upon this stone also rest the other mirrors necessary for producing Michelson's interference bands. The mirror, M_1, is silvered so thinly that half the light which falls upon it is reflected, and half is transmitted.

A ray proceeding from the lantern, L, will be divided at M_1, into two, which follow the paths $LM_1M_2M_1B$ and LM_1MM_1B respectively. Interference bands are thus produced, which can be projected on to a screen, so as to be rendered visible to a large audience.

If the prong of the tuning-fork moves through the eighty-thousandth of an inch, that is, through a distance equal to a half

which is near to, and is of the same area as the wooden plate attached to the tuning fork. This serves to collect the waves of sound, and to concentrate them on the fork. Behind the siren is a large resonator by König, timed to respond to 64 vibrations per second.

In some respects the apparatus requires careful handling. Of course if you blow down the collecting cone the fork may be disturbed, and sometimes a particular note of the siren appears to affect the fork for no very obvious reason. Probably the resonance of the air in the cone, or the vibrations of the wooden disk, may at times be the causes of such effects. We have, however, found that whatever they may be due to, they differ in appearance from those produced by vibrations synchronous with the periodic time of the fork, and they can in general be got rid of by a very slight readjustment of the apparatus. The fact that our main conclusions do not depend on any such nicety, is proved by the fact that the instrument has been set up twice in the laboratory, and once in the lecture-room in the College. In each case all the experiments have been successful, and on one occasion only were we troubled by a disturbance due to a note (of about 253 vibrations) when sounded alone. A slight readjustment of the cone, however, eliminated this effect entirely.

Such difficulties make it no easy matter to set up the apparatus in a hurry, and the most I can hope to do this evening is to demonstrate to you the methods of using it. I cannot undertake to make the actual measurements before you.

It is, however, desirable to illustrate the sensitiveness of the apparatus to vibrations of 64 per second, and its insensitiveness to other sounds.

Provided the current of air does not travel directly down the cone, organ-pipes may be blown just outside it without producing any effect. One of Konig's large tuning-forks may be bowed strongly without effect.

If, however, the exciting fork be tuned to 64 vibrations per second, and if it be struck as lightly as possible with the handle of a small gimlet, used as a hammer, the handle having been previously covered with india-rubber, the bands will immediately vanish, though the note produced is often quite inaudible, even to a person whose ear is placed close to the fork.

Let the weights on the fork be shifted so that it makes 63·5 vibrations per second, then the resonating fork beats. and the bands regularly appear and disappear every two seconds.

Having thus explained the construction and working of the apparatus, let me show you how we have tested whether it responds to a difference tone. When the proper rows of holes are opened, the siren will give simultaneously the *c'* of 256 and the *e'* of 320 vibrations. The interval is a major third, the difference tone is 64 vibrations. The pitch is determined by the beats between the upper note and a standard tuning-fork which gives *e'*. Sounding the upper note alone no effect is produced on the interference bands, as the beats first appear, then die out, and are finally heard again when the note given by the siren is too high.

It could be shown in like manner that the 256 note alone produces no effect, but if, when the standard fork of 320 vibrations and the upper note of the siren are judged to be in exact accord, the 256 note be also produced, the bands immediately disappear. Sometimes, of course, a small error is made in the estimate of the pitch, and the effect is not instantaneous, but in every case the bands disappear when the beats between the two notes are so slow that they cannot be distinguished.

It is therefore evident that Helmholtz was right when he asserted that the difference tone given by the siren is objective. It exists outside the ear, for it can move a tuning-fork.

(*To be continued.*)

JAMES WATT AND OCEAN NAVIGATION.[1]

IF it be asked what James Watt did during his long, busy, and eventful life to improve ocean navigation, or to adapt the steam engine to the work of propelling ships, I am obliged to reply that I am not aware he personally did anything, or even that he concerned himself much about the matter. He took no active part that we know of in applying or adapting his steam engine to the propulsion of ships. The reason probably was that after his attention was first directed to the subject of the steam engine, or fire engine, in 1759, his whole energy was expended, first in improving the steam engine and making its manufacture commercially successful, and afterwards in executing the orders that came for pumping and other engines that were required for mines and manufactures. In the case of most of the greatest mechanical inventions—Watt's among the number—it has not been the ideas or the inventions by themselves that have brought success, prosperity, or even satisfaction to their owners. These results have had to be painfully and slowly evolved out of long and costly practical demonstrations and experience of the alleged merits of the invention. James Watt toiled, suffered and endured for more than twenty years after his discovery of separate condensation in 1765, before he could see that his steam engine would ever bring him anything but disappointment, loss, and misery. It is highly characteristic, however, of Watt's fertile and original genius, and significant of what he might have done to develop the marine engine at the commencement of its history, had he taken the matter up, that upon the two principal occasions we know of when he applied his mind to the subject, he made very pregnant suggestions. Thus, when Watt sent drawings of his engines to Soho in 1770 for Mr. Boulton to construct one for experiment, and had been told that it was intended to make an engine to draw canal boats, Watt wrote, "Have you ever considered a spiral oar for that purpose, or are you for two wheels?" and to make his meaning clear he sketched a rough but graphic outline of a screw propeller. This is, perhaps, the earliest suggestion of a screw propeller, except that it was proposed by Daniel Bernoulli, the mathematician, in 1752. Again, in 1816, four years after the first Clyde steamboat, the *Comet*, was built at Port Glasgow, when Mr. Watt was upon his last visit to Greenock, he went to Rothesay and back in a steamboat. At that time the engineer did not reverse his engines, but merely stopped them some time before the vessel reached her mooring-place, and let her gradually slow down. James Watt, then an old man of eighty, tackled the engineer of the boat, and showed him how the engine could be reversed. He tried to explain this with the aid of a foot rule, but not being successful in doing it to the complete satisfaction of the engineer, he is said to have thrown off his overcoat and given a practical demonstration. Although Watt never took up the subject of steam navigation and never made a marine engine, still he was in reality its originator, because he discovered and provided the means by which it could be applied with advantage to the propulsion of ships. Each of his great improvements upon the old engine that worked by atmospheric pressure and condensed its steam in the cylinder—such as the separate condenser, the working by steam pressure as well as by pressure obtained by vacuum, the double action of the steam in the cylinder on both sides of the piston, working the steam expansively, the centrifugal governor for automatically regulating the speed of the engine, and many others—was a direct adaptation for marine purposes.

There is one point in the history of shipping at which we can draw a definite line between old and new when changes were made so radical in their nature, and so rapid and universal in their operations, that all which came after is fundamentally different from what existed before. The period of transition falls in the early part of the present century, when the propulsion of ships by steam power was substituted for propulsion by the wind—the motive power that had been employed from time immemorial—and when the material out of which their hulls were built was changed from wood to iron. The lateness of this period and its near proximity to the present, is illustrated by the fact that it was not till after the accession of H.M. Queen Victoria that steamships and ships built of iron came to be regularly employed in ocean navigation. At the close of the first third of the nineteenth century, the over-sea trade of the world was carried on with ships that were all built of wood and propelled by sails. Only about 200 of these were over 500 tons in burden, or much over 100 feet long. Nothing approaching to such a rapid and complete revolution as these two great changes brought about in the dimensions, forms, and all the characteristics and qualities of ships, in the conditions of life on board ship, and in travelling by sea, was ever experienced before in the known history of shipping. All the old ships of which we have any knowledge—and by old ships I mean all that existed prior to the introduction of steam—were built and fashioned entirely by manual power, with the aid of very simple tools; and they were either propelled through the water by manual labour, or by sails that could be worked in the simplest manner by the crew. One of the broadest distinctions between the ships of the past that were built of wood and propelled by sails and those of the present that are built of iron or steel and propelled by steam, is that everything had to be done in the former by the hand of man, without any aid from machine tools or other modern labour-saving and labour-helping appliances. And this was so both in preparing the materials used in building the hull and shaping them to their requisite form, putting them in position, fastening them together, and in working the ship at sea and handling the sails so as to make the pressure of the wind most effective for propulsion. In modern ships, almost everything is, on the other hand, done by steam-power in its various applications. It is by this means the plates which form the hull are first of all rolled

[1] Abstract of the Watt Lecture, delivered by Dr. Francis Elgar at Greenock, on January 18.