ments of the spectra. In his first paper he examined the spectra of a group of elements of atomic weight between calcium and zinc. He showed that a similar spectrum consisting of two strong lines was emitted by each of these elements, and proved that the frequency of the corresponding lines in the spectra was proportional to the square of a whole number which varied by unity in passing from one element to the next. This number, which was closely connected with the atomic number of the element, was considered to represent the nucleus charge. He next proceeded to make a systematic study of a great majority of the solid elements, and showed that a similar result held for them all. Since the frequency of a given line in the spectrum varied by definite jumps in passing from one element to the next, he was able to draw the deduction that there could only exist three unknown elements from aluminium to gold, and he was able to predict the atomic number and spectra of these missing elements. This new and powerful method of attack was of especial importance in connection with the much debated question of the number of the rare earth elements.

The fundamental importance of these discoveries was immediately recognised. Prof. Urbain came from Paris to Oxford in order to utilise Moseley's new method to decide the nature of the elements present in the numerous preparations he had made of the rare earths. The results of the investigation of the rare earths have not been published, but it is to be hoped that sufficient data will be available later.

Moseley's fame securely rests on this fine series of investigations, and his remarkable record of four brief years' investigation led those who knew him best to prophesy for him a brilliant scientific career. There can be no doubt that his proof that the properties of an element are defined by its atomic number is a discovery of great and far-reaching importance, both on the theoretical and the experimental side, and is likely to stand out as one of the great landmarks in the growth of our knowledge of the constitution of atoms.

It is a national tragedy that our military organisation at the start was so inelastic as to be unable, with few exceptions, to utilise the offers of services of our scientific men except as combatants in the firing line. Our regret for the untimely end of Moseley is all the more poignant that we cannot but recognise that his services would have been far more useful to his country in one of the numerous fields of scientific inquiry rendered necessary by the war than by exposure to the chances of a Turkish bullet.

E. RUTHERFORD.

THE BRITISH ASSOCIATION AT MANCHESTER.

WRITING on the eve of the British Association week, it may be said that the prospects of a good number of members and associates are much brighter now than they were a few weeks ago. The experiment of shortening the meeting and of cutting out of the programme the long-distance excursions was one that threatened to reduce the numbers considerably, but we can be assured now that a very large proportion of those who attend the meeting of the British Association are primarily attracted by the scientific programme.

The interest taken in the meeting by the citizens of Manchester and the surrounding district has very noticeably increased during the past week, and a large number of students and teachers are enrolling themselves as associates on the half-fee terms that were offered by the Council for this meeting.

The discussions that will probably attract the largest attendances from among the local members are those on industrial harmony in Section F and on military education in Section L. Mr. Balls's lecture on the application of science to the cotton industry in Section K will also attract a good audience of local people.

In addition to the afternoon sectional excursions already announced, the agriculturists have arranged visits to the Agricultural Institution of the Cheshire County Council at Holmes Chapel and to another large farm in the district.

The arrangements made by the local executive committee for the reception of the Association have now been completed, and it will be found that ample accommodation has been provided for the comfort and convenience of the visitors. In time of war there are some subjects that cannot be discussed advisedly in open section, particularly in the sections of the physical sciences; but it may be anticipated that some important work will be done in the private discussions of smaller groups of scientific men in the smoking and conversation rooms. The university and the high school for girls will between them provide facilities for such informal discussions on a larger scale, probably, than in any previous meeting of the Association.

INAUGURAL ADDRESS BY PROF. ARTHUR SCHUSTER, D.SC., SC.D., LL.D., DR.-ÈS-SC., F.R.S., PRESIDENT.

The Common Aims of Science and Humanity.

UNDER the influence of the diversity of pursuits imposed upon us by the conditions of modern life, different groups of the community—men of business, men of science, philosophers, or artists—have acquired detached and sometimes opposing interests. Each group, impressed by the importance of its own domain in the life of the nation, and focussing its vision on small differences and temporary rivalries, was in danger of losing the sense of mutual dependence. But in the shadow of a great catastrophe it has been brought home to us that the clash of interests is superficial, and the slender thread of union which remained has grown into a solid bond. What is the fibre from which the bond is twined? Patriotism may express its outward manifestation, but its staple is the mental relationship which remains continuous and dominant even in normal times, when each of us may peacefully go to earn his living and enjoy the course of his intellectual life.

Outwardly the community is divided into heterogeneous elements with mental attitudes cast in different moulds, and proceeding along separate roads by differing methods to different ideals. Yet as we eliminate the superficial, and regard only the deep-seated emotions which control our thoughts and actions, the differences vanish, and the unity of purpose and sentiment emerges more and more strongly. Mind and character, no doubt, group themselves into a number of types, but the cleavage runs across, and not along, the separating line of professions.

Were it otherwise, the British Association could not perform one of its most important functions—a function not, indeed, originally contemplated, but resulting indirectly from the wise and democratic provisions in its constitution, which enabled it to adapt itself to the changing needs of the time. Our founders primarily considered the interests of scientific men; their outlook was restricted and exclusive, both as regards range of subject and membership. In the words of Sir David Brewster, who gave the first impulse to its formation, it was to be "an association of our nobility, clergy, gentry, and philosophers."

The meetings were intended to promote personal intercourse, to organise research, to advocate reform of the laws hindering research, and to improve the status of scientific men. The right of membership was confined to those who already belonged to some learned society, and William Whewell, one of the principal supporters of the movement, even suggested that only authors of memoirs published by a learned society should be admitted.[1] He emphasised this proposal by the recommendation[2] "in some way to avoid the crowd of lay members whose names stand on the List of the Royal Society." The reform of the Patent Laws and the introduction of an International Copyright were suggested as subjects suitable for discussion, not apparently from the point of view of general advantage, but merely in the interests of one section of the community.

Whatever the objects of the founders of the association may have been, it is obvious that questions of public importance could not be permanently excluded from meetings the success of which depended on the interest stimulated in the community. The Statistical Section, which owed its origin to the visit, at the first Oxford meeting (1836), of Quetelet, the Belgian astronomer and economist, was the first to assert itself by engaging in a discussion of the Poor Laws. Whewell deeply resented this violation of academic neutrality: "it was impossible," he wrote, "to listen to the Proceedings of the Statistical Section on Friday without perceiving that they involved exactly what it was most necessary and most desired to exclude from our Proceedings,"[3] and again: "Who would propose (I put it to Chalmers, and he allowed the proposal to be intolerable) an ambulatory body, composed partly of men of reputation and partly of a miscellaneous crowd, to go round year by year from town to town and at each place to discuss the most inflammatory and agitating questions of the day?"[4]

Fortunately for our association, this narrow-minded attitude did not prevail, and our records show that while not avoiding controversial and even inflammatory subjects, we have been able to exercise a powerful influence on the progress of science. The establishment of electric units, universally accepted throughout the world, originated in the work of one of our committees; the efforts which led to the foundation of the National Physical Laboratory, one of the most efficient and beneficial organisations in the country, received its first impulses from us; and the organisation of the first world service for the systematic investigation of earth tremors was established by the late Dr. Milne, working through one of our committees.

The success of these enterprises alone is sufficient to show that we are not merely a body promoting social intercourse between men of science and the rest of the community. Nevertheless, it may be admitted that our efforts have been spasmodic, and the time has arrived to consider whether it may be possible to secure not only a greater continuity in our work but also its better co-ordination with that of other scientific organisations. The present juncture affords the opportunity, and the changed conditions, which in the near future will affect all our institutions, render it indeed incumbent upon us once more to adapt ourselves to the needs of the times. Proposals for a move in that direction have already been made, and will no doubt be carefully considered by the council. In the meantime, I may direct your attention to the important discussions arranged for by our Economic Section, which alone will justify the decision of the council not to suspend the meeting this year.

It must not be supposed that, even in the early days of the association, Whewell's ideas of its functions were universally accepted. It is pleasant to contrast the lamentations of the omniscient professor of mineralogy with the weightier opinion of the distinguished mathematician who then held Newton's chair at Cambridge. At the concluding session of the second meeting of the association, Babbage expressed the hope "that in the selection of the places at which the annual meetings were to be held, attention should be paid to the object of bringing theoretical science in contact with the practical knowledge on which the wealth of the country depends." "I was myself," he said, "particularly anxious for this, owing as I do a debt of gratitude for the valuable information which I have received in many of the manufacturing districts, where I have learned to appreciate still more highly than before the value of those speculative pursuits which we follow in our academical labours. I was one of those who thought at first that we ought to adjourn for our next meeting to some larger manufacturing town; but I am now satisfied that the arrangement which has been made will be best adapted to the present state of the association. When, however, it shall be completely consolidated I trust we may be enabled to cultivate with the commercial interests of the country that close acquaintance which I am confident will be highly advantgeous to our more abstract pursuits."

Since then, as we all know, our most successful meetings have been held in manufacturing centres; but it should be observed that, while Babbage laid stress on the benefit which would accrue to pure science by being brought into contact with practical life, scientific men of the present day have more and more insisted on the services they, on their part, are able to render to the industries. The idealistic motive has thus given way to the materialistic purpose. Both aspects are perhaps equally important, but it is necessary to insist, at the present time, that the utilitarian drum can be beaten too loudly. There is more than one point of contact between different activities of the human mind, such as find expression in scientific pursuits or commercial enterprises, and it is wrong to base the advantages to be derived from their mutual influence solely, or even mainly, on the ground of material benefits.

I need not press this point in a city which has given many proofs that a business community may be prompted by higher motives than those which affect their pockets. It was not for utilitarian objects that

[1] Others were allowed to join on recommendation by the General Committee. It was only in 1906 that this restriction, which had become obsolete, was removed.

[2] "Whewell's Writings and Letters," vol. ii., p. 128.

[3] *Loc. cit.*, p. 289.

[4] It is much to be desired that the documents relating to the early history of the British Association should be published in a collected form.

repeated efforts were made since the year 1640 to establish a University in Manchester; it was not for reasons of material gain that the Royal Institution and Owens College were founded; nor was it because they increased the wealth of the district that the place of honour in our Town Hall has been given to Dalton and Joule.

When we glance at the various occupations of the working parts of a nation, comprising the student who accumulates or extends knowledge, the engineer who applies that knowledge, the geologist or agriculturist who discloses the store of wealth hidden in the soil, the commercial man who distributes that wealth, it seems as if we ought to be able to name the qualities of intellect and temperament which in each pursuit are most needed to carry out the work successfully. But on trying to define these qualities we soon discover the formidable nature of the task. Reasoning power, inventive power, and sound balance of judgment are essential attributes in all cases, and the problem is reduced to the question whether there are different varieties of the attributes which can be assigned to the different occupations.

Among all subjects mathematics is perhaps the one that appears most definitely to require a special and uncommon faculty. Yet, Poincaré—himself one of the clearest thinkers and most brilliant exponents of the subject—almost failed when he attempted to fix the distinguishing intellectual quality of the mathematician. Starting from the incontrovertible proposition that there is only one kind of correct reasoning, which is logical reasoning, he raises the question why it is that everybody who is capable of reasoning correctly is not also a mathematician, and he is led to the conclusion that the characterising feature is a peculiar type of memory. It is not a better memory, for some mathematicians are very forgetful, and many of them cannot add a column of figures correctly; but it is a memory which fixes the order in which the successive steps of reasoning follow each other without necessarily retaining the details of the individual steps. This Poincaré illustrates by contrasting the memory of a chess-player with that of a mathematician. "When I play chess," he says, "I reason out correctly that if I were to make a certain move, I should expose myself to a certain danger. I should, therefore, consider a number of other moves, and, after rejecting each of them in turn, I should end by making the one which I first contemplated and dismissed, having forgotten in the meantime the ground on which I had abandoned it." "Why, then," he continues, "does my memory not fail me in a difficult mathematical reasoning in which the majority of chess-players would be entirely lost? It is because a mathematical demonstration is not a juxtaposition of syllogisms, but consists of syllogisms placed in a certain order; and the order in which its elements are placed is much more important than the elements themselves. If I have this intuition—so to speak—of the order, so as to perceive at one glance the whole of the reasoning, I need not fear to forget its elements: each of these will take its right place of its own accord without making any call on my memory."[5]

Poincaré next discusses the nature of the intellectual gift distinguishing those who can enrich knowledge with new and fertile ideas of discovery. Mathematical invention, according to him, does not consist in forming new combinations of known mathematical entities, because the number of combinations one could form are infinite, and most of them would possess no interest whatever. Inventing consists, on the contrary, in excluding useless combinations, and therefore:—"To invent is to select—to choose. . . . The expression 'choose' perhaps requires qualifying, because it recalls a buyer to whom one offers a large number of samples which he examines before making his choice. In our case the samples would be so numerous that a lifetime would not suffice to complete the examination. That is not the way things are done. The sterile combinations never present themselves to the mind of the inventor, and even those which momentarily enter his consciousness, only to be rejected, partake something of the character of useful combinations. The inventor is therefore to be compared with an examiner who has only to deal with candidates who have already passed a previous test of competence."

[5] "Science et Méthode," pp. 46 and 47.

All those who have attempted to add something to knowledge must recognise that there is a profound truth in these remarks. New ideas may float across our consciousness, but, selecting the wrong ones for more detailed study, we waste our time fruitlessly. We are bewildered by the multitude of roads which open out before us, and, like Poincaré when he tries to play chess, lose the game because we make the wrong move. Do we not all remember how, after the announcement of a new fact or generalisation, there are always many who claim to have had, and perhaps vaguely expressed, the same idea? They put it down to bad luck that they have not pursued it, but they have failed precisely in what, according to Poincaré, is the essence of inventive power. It may be bad luck not to have had a good idea, but to have had it and failed to appreciate its importance is downright incapacity.

An objection may be raised that before a selection can be made the ideas themselves must appear, and that, even should they arrive in sufficient numbers, the right one may not be among them. It may even be argued that Poincaré gives his case away by saying that "the sterile combinations do not even present themselves to the mind of the inventor," expressing in a negative form what may be the essence of the matter. Moreover, a fertile mind like that of Poincaré would be apt to place too low a value on his own exceptional gifts. Nevertheless, if Poincaré's more detailed exposition be read attentively, and more especially the description of how the discoveries which made him famous among mathematicians originated in his mind, it will be found that his judgment is well considered and should not be lightly set aside. New ideas seldom are born out of nothing. They most frequently are based on analogies, or the recollection of a sequence of thoughts suggested by a different branch of the subject, or perhaps by a different subject altogether. It is here that the memory comes in, which is not a memory of detail, but a memory of premises with their conclusions, detached from the particular case to which they were originally applied. Before we pronounce an adverse opinion on Poincaré's judgment, we must investigate what constitutes novelty in a new idea; but the subject is too vast to be dealt with here, nor can I attempt to discuss whether an essential distinction exists between mathematical invention and that more practical form of invention with which, for instance, the engineer has to deal.

If Poincaré, by this introspective analysis of his own powers, has dimmed the aureole which, in the eyes of the public, surrounds the mathematician's head, he removes it altogether by his definition of mathematics. According to him, "mathematics is the art of calling two different things by the same name." It would take me too far were I to try to explain the deep truth expressed in this apparently flippant form: physicists, at any rate, will remember the revolution created in the fundamental outlook of science by the application of the term "energy" to the two quite distinct conceptions involved in its subdivisions into potential and kinetic energy.

Enough has been said to show that the peculiar powers necessary for the study of one of the most abstruse branches of knowledge may be expressed in terms which bring them down to the level at which comparison with other subjects is possible. Applying the same reasoning to other occupations, the same conclusion is inevitable. The commercial man, the politician, and the artist must all possess the type of memory best suited to concentrate in the field of mental vision their own experiences as well as what they have learned from the experience of others; and, further, they must have the power of selecting out of a multitude of possible lines of action the one that leads to success; it is this power which Poincaré calls the inventive faculty.

The argument must not be pushed too far, as it would be absurd to affirm that all differences in the capability of dealing successfully with the peculiar problems that occur in the various professions may be reduced to peculiarities of memory. I do not even wish to assert that Poincaré's conclusions should be accepted without qualification in the special case discussed by him. What is essential, to my mind, is to treat the question seriously, and to dismiss the vague generalities which, by drawing an artificial barrier between different groups of professions, try to cure real or imaginary defects through plausible though quite illusory remedies. All these recommendations are based on the fallacy that special gifts are associated with different occupations. Sometimes we are recommended to hand over the affairs of the nation to men of business; sometimes we are told that salvation can only be found in scientific methods—what is a man of business, and what is a scientific method? If you define a man of business to be one capable of managing large and complicated transactions, the inference becomes self-evident; but if it be asserted that only the specialised training in commercial transactions can develop the requisite faculties, the only proof of the claim that could be valid would be the one that would show that the great majority of successful statesmen, or political leaders, owed their success to their commercial experience. On the other hand, every method that leads to a correct result must be called a scientific method, and what requires substantiating is that scientific training is better than other training for discovering the correct method. This proof, as well as the other, has not been, and, I think, cannot be, given. When, therefore, one man calls for the conduct of affairs "on business lines" and the other clamours for scientific methods, they either want the same thing or they talk nonsense. The weak point of these assertions contrasting different classes of human efforts is that each class selects its own strongest men for comparison with the weakest on the other side. Where technical knowledge is required, the specialist should be consulted, but in questions of general policy he is seldom the best judge.

The most fatal distinction that can be made is the one which brings men of theory into opposition to men of practice, without regard to the obvious truth that nothing of value is ever done which does not involve both theory and practice; while theory is sometimes overbearing and irritating, there are among those who jeer at it some to whom Disraeli's definition applies: the practical man is the man who practises the errors of his forefathers. With refined cruelty Nemesis infects us with the disease most nearly akin to that which it pleases us to detect in others. It is the most dogmatic of dogmatics who tirades against dogma, and only the most hopeless of theorists can declare that a thing may be right in theory and wrong in practice.

Why does a theory ever fail, though it may be sound in reasoning? It can only do so because every problem involves a much larger number of conditions than those which the investigator can take into account. He therefore rejects those which he believes to be unessential, and if his judgment is at fault he goes wrong. But the practical man will often fail for the same reason. When not supported by theoretical knowledge he generalises the result of an observation or experiment, applying it to cases where the result is determined by an altogether different set of conditions. To be infallible the theorist would have to take account of an infinite number of circumstances, and his calculations would become unmanageable, while the experimenter would have to perform an infinite number of experiments, and both would only be able to draw correct conclusions after an infinite lapse of time. They have to trust their intuition in selecting what can be omitted with impunity, and, if they fail, it is mainly due to the same defect of judgment. And so it is in all professions: failure results from the omission of essential considerations which change the venue of the problem.

Though theory and practice can only come into opposition when one of them is at fault, there is undoubtedly a contrast in character and temperament between those who incline more towards the one and those who prefer the other aspect; some like a solitary life at the desk, while others enjoy being brought into contact with their fellows. There have at all times been men predestined by nature to be leaders, and leadership is required in all branches of knowledge—the theoretical as well as the more active pursuits; but we must guard against accepting a man's estimate of his own power to convert his thoughts into acts. In the ordinary affairs of life a man who calls himself a man of action is frequently only one who cannot give any reasons for his actions. To claim that title justly a man must act deliberately, have confidence in his own judgment, sufficient tenacity of purpose to carry it through, and sufficient courage to run the unavoidable risks of possible failure. These risks may be trivial or they may be all-important. They may affect the reputation of one unit of creation or involve the whole life of a nation, and according to the greatness of the issue we shall honour the man who, having taken the risk, succeeds. But whether the scale be microscopic or interstellar, the essence of the faculty of blending theory and practice is the same, and both men of books and men of action are to be found in the philosopher's study and the laboratory, as well as in the workshop or on the battlefield. Modern science began, not at the date of this or that discovery, but on the day that Galileo decided to publish his Dialogues in the language of his nation. This was a deliberate act destined to change the whole aspect of science which, ceasing to be the occupation of a privileged class, became the property of the community. Can you, therefore, deny the claim of being a man of action to Galileo, can you deny it to Pasteur, Kelvin, Lister, and a host of others? There are, no doubt, philosophers who cannot manage even their own affairs, and whom it would be correct to call pure theorists, but that proves nothing, because their defect makes them worse philosophers as well as worse citizens.

In his presidential address, delivered to this Association in 1899, Sir Michael Foster summarised the essential features of the scientific mind. Above all other things he considered that its nature should be such as to vibrate in unison with what it is in search of; further, it must possess alertness, and finally moral courage. Yet after enumerating these qualities, he arrives at the same result which I have tried to place before you, that there are no special peculiarities inherent in the scientific mind, and he expresses this conclusion in the following words:—

"But, I hear someone say, these qualities are not the peculiar attributes of the man of science, they may be recognised as belonging to almost everyone who has commanded or deserved success, whatever may have been his walk in life. That is so. That is exactly what I would desire to insist, that the men of science have no peculiar virtues, no special powers. They are ordinary men, their characters are common, even commonplace. Science, as Huxley said, is organised common-sense, and men of science are common men drilled in the ways of common-sense."

This saying of Huxley's has been repeated so often that one almost wishes it were true, but unfortunately I cannot find a definition of common-sense that fits the phrase. Sometimes the word is used as if it were identical with *uncommon* sense, sometimes as if it were the same thing as common *nonsense*. Often it means untrained intelligence, and in its best aspect it is, I think, that faculty which recognises that the obvious solution of a problem is frequently the right one. When, for instance, I see, during a total solar eclipse, red flames shooting out from the edge of the sun, the obvious explanation is that these are real phenomena caused by masses of glowing vapours ejected from the sun; and when a learned friend tells me that all this is an optical illusion due to anomalous refraction, I object on the ground that the explanation violates my common-sense. He replies by giving me the reasons which have led him to his conclusions, and, though I still believe that I am right, I have to meet him with a more substantial reply than an appeal to my own convictions. Against a solid argument common-sense has no power and must remain a useful but fallible guide which both leads and misleads all classes of the community alike.[6]

If we must avoid assuming special intellectual qualities when we speak of groups of men within one country, we ought to be doubly careful not to do so without good reason in comparing different nations. So-called national characteristics are in many cases matters of education and training; and, if I select one as an example, it is because it figures so largely in public discussions at the present moment. I refer to that expedient for combining individual efforts which goes by the name of organisation. An efficient organisation requires a head that directs and a body that obeys; it works mainly through discipline, which is its most essential attribute. Every institution, every factory, every business establishment is a complicated organism, and no country ever came to prominence in any walk of life unless it possessed the ability to provide for the efficient working of such organisms. To say that a nation which has acquired and maintained an empire, and which conducts a large trade in every part of the world, is deficient in organising power is therefore an absurdity. Much of the current self-depreciation in this respect is due to the confusion of what constitutes a true organisation with that modification of it which to a great extent casts aside discipline and substitutes co-operation. Though much may be accomplished by co-operation, it is full of danger in an emergency, for it can only work if it be loyally adhered to; otherwise it resembles a six-cylinder motor in which every sparking-plug is allowed to fix its own time of firing. Things go well so long as the plugs agree; but there is nearly always one among them that persists in taking an independent course and, when the machine stops, complains that the driver is inefficient. The cry for organisation, justifiable as it no doubt often is, resolves itself, therefore, into a cry for increased discipline, by which I do not mean the discipline enforced at the point of the bayonet, but that voluntarily accepted by the individual who subordinates his own convictions to the will of a properly constituted authority.

This discipline is not an inborn quality which belongs more to one nation than to another; it is acquired by education and training. In an emergency, it is essential to success, but if it be made the guiding principle of a nation's activity, it carries dangers with it which are greater than the benefits conferred by the increased facility for advance in some directions.

If there be no fundamental difference in the mental qualifications which lead to success in our different occupations, there is also none in the ideals which move us in childhood, maintain us through the difficulties of our manhood, and give us peace in old age. I am not speaking now of those ideals which may simultaneously incite a whole nation to combined action through religious fervour or ambition of power, but I am speaking of those more individual ideals which make us choose our professions and give us pleasure in the performance of our duties.

Why does a scientific man find satisfaction in studying Nature?

Let me once more quote Poincaré[7]:—

"The student does not study Nature because that study is useful, but because it gives him pleasure, and it gives him pleasure because Nature is beautiful; if it were not beautiful it would not be worth knowing and life would not be worth living. I am not speaking, be it understood, of the beauty of its outward appearance—not that I despise it, far from it, but it has nothing to do with science: I mean that more intimate beauty which depends on the harmony in the order of the component parts of Nature. This is the beauty which a pure intelligence can appreciate and which gives substance and form to the scintillating impressions that charm our senses. Without this intellectual support the beauty of the fugitive dreams inspired by sensual impressions could only be imperfect, because it would be indecisive and always vanishing. It is this intellectual and self-sufficing beauty, perhaps more than the future welfare of humanity, that impels the scientific man to condemn himself to long and tedious studies. And the same search for the sense of harmony in the world leads us to select the facts in Nature which can most suitably enhance it, just as the artist chooses among the features of his model those that make the portrait and give it character and life. There need be no fear that this instinctive and unconscious motive should tempt the man of science away from the truth, for the real world is far more beautiful than any vision of his dreams. The greatest artists that ever lived—the Greeks—constructed a heaven, yet how paltry that heaven is compared to ours! And it is because simplicity and grandeur are beautiful that we select by preference the simplest and grandest facts, and find our highest pleasure, sometimes in following the gigantic orbits of the stars, sometimes in the microscopic study of that minuteness which also is a grandeur, and sometimes in piercing the secrets of geological times which attract us because they are remote. And we see that the cult of the beautiful guides us to the same goal as the study of the useful."

[6] Since writing the above, I find on reading Prof. J. A. Thomson's "Introduction to Science" a similar criticism of Huxley's dictum. Prof. Thomson's general conclusions are not, however, in agreement with those here advocated.

[7] *Loc. cit.*, p. 15.

"Whence comes this harmony? Is it that things that appear to us as beautiful are simply those which adapt themselves best to our intelligence, and are therefore the tools which that intelligence handles most easily; or is it all the play of evolution and natural selection? In that case, those races only survived whose ideals best conformed with their interests, and while all nations pursued their ideals without regard to consequences, some were led to perdition and others achieved an empire. One is tempted to believe that such has been the course of history, and that the Greeks triumphed over the barbarians, and Europe, inheritor of Greek thought, rules the world, because the savages cared only for the sensual enjoyment of garish colours and the blatant noise of the drum, while the Greeks loved the intellectual beauty which is hidden beneath the visible beauty. It is that higher beauty which produces a clear and strong intelligence." If the mathematician's imagination is fired by the beauty and symmetry of his methods, if the moving spring of his action is identical with that of the artist, how much truer is this of the man of science who tries by well-designed experiments to reveal the hidden harmonies of Nature? Nor would it be difficult, I think, to trace the gratification inherent in the successful accomplishments of other intellectual pursuits to the same source.

Though Poincaré was, I believe, the first to lay stress on the connection between the search for the beautiful and the achievement of the useful, the æsthetic value of the study of science had previously been pointed out, and well illustrated, by Karl Pearson in his "Grammar of Science." As expressed by him: "It is this continual gratification of the æsthetic judgment which is one of the chief delights of pure science." Before we advance, however, any special claim for the pursuit of science based on these considerations, we must pause to think whether they do not equally apply to other studies or occupations. For this purpose, the nature of the æsthetic enjoyment involved must be remembered. We do not mean by it, the pleasure we feel in the mere contemplation of an impressive landscape or natural beauty, but it resembles more the enjoyment experienced on looking at a picture where, apart from the sensual pleasure we are affected by the relation between the result of the representation and that which is represented. The picture, quite apart from what it may be trying to imitate, has a certain beauty due to its contrast of colours or well-balanced arrangement. We have in one case a number of pigments covering a space of two dimensions, and in the other the natural object in three dimensions made up of entirely different materials and showing an infinite variety of detail and appearance. By itself alone either a mere photographic representation, or a geometrical arrangement of colour and line, leaves most of us cold; though both have their own particular beauty, the art consists in bringing them into connection. Bearing in mind the æsthetic value of the relationship of the work of our brain or hand to external facts or appearances, it might easily be shown that what has been said of science equally applies to other studies, such as history or literature. We may even go further, and say that any occupation whatever, from which we can derive an intellectual pleasure, must possess to a greater or smaller degree the elements of combining the useful with the beautiful.

In order to trace in detail the part played by purely emotional instincts in directing the course of our lives, we should have to study the causes which influence a child, free to select his future profession. Having eliminated secondary efforts, such as early associations, or the personal influence of an inspiring teacher, we should probably be brought to a standstill by the dearth of material at our disposal, or led into error by taking our own individual recollections as typical. Nevertheless it is only through the record of each man's experience that we may hope to arrive at a result. If every man who has reached a certain recognised position in his own subject—it need not be pre-eminence—would write down his own recollections of what led him to make the choice of his profession, we might hope to obtain facts on which a useful psychological study might be based. Scientific men as a class are not modest, but they share with other classes the reluctance to speak of their early life, owing to a certain shyness to disclose early ambitions which have not been realised. It requires courage to overcome that shyness, but I think that we need feel no shame in revealing the dreams of our childhood and holding fast to them despite the bondage of our weakness, despite the strife ending so often in defeat, despite all the obstacles which the struggle for existence has placed in our path. In some form they should persist throughout our lives and sustain us in our old age.

But the account of our early life should be simple, detached from any motives of self-depreciation or self-assertion, and free from any desire to push any particular moral or psychological theory. We want to trace the dawn of ambition, the first glimmering in the child's mind that there is something that he can do better than his fellows and reminiscences of early likes and dislikes which, though apparently disconnected from maturer tendencies, may serve as indications of a deep-seated purpose in life. It may be difficult to resist the temptation of trying to justify one's reputation in the eyes of the world; but it is worth making the effort. The only example that I know of such an autobiographical sketch is that of Darwin, which is contained in his "Life and Letters," published by his son, Sir Francis Darwin.

The ambition of a child to be better, cleverer, or more beautiful than its fellows is in the main, I think, a wish to please and to be praised. As the child grows up, the ambition becomes more definite. It is not a sordid ambition for ultimate wealth or power, nor is it an altruistic ambition to do good for the sake of doing good. Occasionally it takes the form confessed to by Darwin, when he says: "As a child I was much given to inventing deliberate falsehoods, and this was always done for the sake of causing excitement." This desire to be conspicuous was, in Darwin's case, consistent with extreme modesty, amounting almost to a want of confidence in himself, as appears in this passage: "I remember one of my sporting friends, Turner, who saw me at work with my beetles, saying that I should some day be a Fellow of the Royal Society, and this notion seemed to me to be preposterous."

We next come to the stage where a child is attracted by one subject more than another, and, if his choice be free, will select it for his life's career. What guides him in this choice? If it be said that a boy gravitates towards that subject which he finds easiest, we are led to the further question, why does he find it easiest? It is on this point that more information is required, but I am inclined to answer in accordance with Poincaré's views that it is because its particular beauty appeals most strongly to his emotional senses. In questions of this kind everyone must form his own conclusions according to his personal recollections, and these convince me that the emotional factor appears already at an early age. It is the strong attraction towards particular forms of reasoning, more perhaps even than the facility with which reasoning comes, that carries us over the initial difficulties and the drudgery that must accompany every serious study.

I have already alluded to the different tendencies of individuals either to prefer solitary reflection or to seek companionship. Almost in every profession we find men of both types. Darwin's autobiography furnishes a good example of the man who prefers to learn through quiet reading rather than through lectures, but to many men of science the spoken word is inspiring and contact with congenial minds almost a necessity.

From our present point of view the most interesting passages in Darwin's autobiography are those indicating the æsthetic feeling which, like Poincaré, he connects with scientific research. Referring to his early studies we find this passage: "I was taught by a private tutor, and I distinctly remember the intense satisfaction which the clear geometrical proofs gave me. I remember with equal distinctness the delight which my uncle gave me by explaining the principle of the vernier of a barometer." To a man who apparently had no pronounced facility of mastering mathematical difficulties this feeling of satisfaction is especially remarkable. The combination of scientific ability with leanings either to music, or art, or poetry, is very common, and examples are to be found in almost every biography of men of science. It is difficult indeed to name an eminent scientific man who has not strong leanings towards some artistic recreation: we find the poetic vein in Maxwell and Sylvester, the musical talent in Helmholtz and Rayleigh, and the enthusiastic though amateurish pictorial efforts of less important men. That the similarities are to be found also in temperament may be noticed on reading Arnold Bennett's article on "The Artist and the Public,"[8] where many passages will be seen to be applicable to students of science as well as to writers of fiction.

If we look for distinctions between different individuals, we may find one in their leanings either towards the larger aspects of a question or the microscopic study of detail. The power of focussing simultaneously the wider view and the minute observation is perhaps the most characteristic attribute of those who reach the highest eminence in any profession, but the great majority of men have a notable predilection for the one or other side. Though it is indispensable for a scientific man to study the details of the particular problem he is trying to solve, there are many who will lose interest in it as soon as they believe they can see a clear way through the difficulties without following up their solution to its utmost limits. To them detail, as such, has no interest, and they will open and shut a door a hundred times a day without being even tempted to inquire into the inner working of the lock and latch.

There is only one feature in the operation of the intelligence by means of which a sharp division may possibly be drawn between brain-workers showing special capabilities in different subjects. In some persons thought attaches itself mainly to language, in others to visualised images, and herein lies perhaps the distinction between the literary and scientific gift. Those who, owing to external circumstances, have resided in different countries are sometimes asked in what language they think. Speaking for myself, I have always been obliged to answer that, so far as I can tell, thought is not connected with any language at all. The planning of an experiment or even the critical examination of a theory is to me entirely a matter of mental imagery, and hence the experience, which I think many scientific men must have shared, that the conversion of thought into language, which is necessary when we wish to communicate its results to others, presents not only the ordinary difficulties of translation, but reveals faults in the perfection or sequence of the images. Only when the logic of words finally coincides with the logic of images do we attain that feeling of confidence which makes us certain that our results are correct.

A more detailed examination of the instinctive predilections of a child would, I think, confirm Poincaré's conclusion that a decided preference for one subject is in the main due to an unconscious appeal to his emotions. It should be remembered, however, that the second step of Poincaré's philosophy is as important as the first. The mere emotional impulse would die out quickly, if it were not supplemented by the gratification experienced on discovering that the search for the beautiful leads us to results which satisfy our intellect as well as our emotions. There may still be bifurcations in the second portion of the road. Some may rest content with achieving something that supplies the material needs of humanity, others may be inspired to search for the deeper meaning of our existence.

There remains, therefore, some justification for the question why we persist in studying science apart from the mere intellectual pleasure it gives us. It was once a popular fallacy to assume that the laws of nature constituted an explanation of the phenomena to which they applied, and people then attached importance to the belief that we could gauge the mind of the Creator by means of the laws which govern the material world, just as we might trace the purpose of a human legislator in an Act of Parliament. As this archaic interpretation was abandoned, philosophers went, in accordance with what politicians call the swing of the pendulum, to the other extreme. We can explain nothing, they said—in fact, we can know nothing—all we can do is to record facts. This modesty was impressive and it became popular. I know, at any rate, one scientific man who has acquired a great reputation for wisdom by repeating sufficiently often that he knows nothing, and, though his judgment may be true, this frame of mind is not inspiring. As a corrective to the older visionary claims, which centred round the meaning of the word "explain," the view that the first task of science is to record facts has no doubt had a good influence. Kirchhoff laid it down definitely that the object of science is to describe nature, but he did not thereby mean that it should be confined to recording detached observations; this would be the dullest and most unscientific procedure. Description, in the sense in which Kirchhoff uses it, consists in forming a comprehensive statement gathering together what, until then, was only a disconnected jumble of facts. Thus the apparently quite irregular motions of the planets, as observed from the earth, were first collected in tabular form. This was a necessary preliminary, but was not in itself a scientific investigation. Next came Kepler, who by means of three laws summed up the facts in their main outlines, and the description then took a more refined form substituting half a page of printing for volumes of observations. Finally, Newton succeeded in predicting the planetary movements on the assumption of a gravitational attraction between all elements of matter. According to Kirchhoff, the chief merit of this discovery would lie in its condensing Kepler's three laws into one hypothesis. This point of view is not necessarily opposed to that of Poincaré, because it is exactly the simplicity of Newton's explanation that appeals most strongly to our æsthetic sense, but there is an important difference in the manner of expression. However beautiful an idea may be, it

[8] *English Review*, October, 1913.

loses its effect by being placed before us in an unattractive form. This criticism also applies to Mach, according to whom the object of science is to economise thought, just as it is the object of a machine to economise effort. Logically, this definition is justified, and it may be the best that can be given, if we prefer using a technical expression to confessing an emotional feeling. But why should we do so? Is it not better to recognise that human intelligence is affected by sentiment as much as by reasoning? It is a mistake for scientific men to dissociate themselves from the rest of humanity, by placing their motives on a different, and, at the best, only superficially higher, level. When an adventurous spirit, for instance, desires to organise an expedition to unknown regions of the world, we try to induce our Governments to provide the necessary funds by persuading them, and incidentally ourselves, that we do so because important scientific results may be expected from the expedition. This may actually be the case, but we are mainly affected by the same motives as the rest of the community; if the truth be told, we are as curious as others to know what every corner of the earth looks like, and we join them in wishing to encourage an enterprise requiring perseverance and involving danger.

I fully realise that the wish to justify one's own work in the eyes of the world will always lead to fresh attempts to find a formula expressing the objects which we desire to attain. Enough, however, has been said to show that the definition must take account of sentiment, without insisting too much upon it. Nor can we hope, in view of the variety of intellectual and emotional pleasures which combine to create the charm of science, to include all points of view, but if I were forced to make a choice I should say that the object of science is to predict the future. The wish to know what lies before us is one of the oldest and most enduring desires of human nature; often, no doubt, it has degenerated and given rise to perverted and ignoble longings, but its accomplishment, when it can be achieved by legitimate inquiry, is a source of the purest and most satisfying enjoyment that science can give. We feel that enjoyment each time we repeat an old and perhaps hackneyed experiment. The result is known beforehand, but be it only that we expect the colour of a chemical precipitate to be green or yellow, be it only that we expect a spot of light to move to the right or left, there is always a little tremor of excitement at the critical moment, and a satisfying feeling of pleasure when our expectation has been realised. That pleasure is, I think, enhanced when the experiment is not of our own making, but takes place uncontrolled by human power. In one of Heine's little verses he makes light of the tears of a young lady who is moved by the setting sun. "Be of good cheer," the poet consoles her, "this is only the ordinary succession of events; the sun sets in the evening and rises in the morning." If Heine had been a man of science, he would have known that the lady's tears found a higher justification in the thought of the immutable and inexorable regularity of the sun's rising and setting than in the fugitive colour impression of his descent below the horizon, and that her emotions ought to be intensified rather than allayed by the thought of his resurrection in the morning—everybody's life contains a few unforgettable moments which, at quite unexpected times, will vividly rise in his mind, and there are probably some in this hall who have experienced such moments at the beginning of a total eclipse of the sun. They have probably travelled far, and gone through months of preparation, for an event which only lasts a few minutes. The time of first contact is approaching, in a few seconds the moon is about to make its first incision in the solar disc, and now the observer's thoughts come crowding together. What if there were a mistake in our calculations? What if we had chosen a spot a few miles too far north or too far south? What if the laws of gravitation were ever so little at fault? But now at the predicted time, at the calculated spot on the sun's edge, the dark moon becomes visible, and the feeling of relief experienced concentrates into one tense instant all the gratitude we owe those who have given precision to the predictions of celestial movements, leaving them expressible by a simple law which can be written down in two lines. It is this simplicity of the law of gravitation, and its accuracy, which some day may show limitations, but has hitherto withstood all tests, that gives to astronomy its pre-eminence over all sciences.

Indeed, if we classify the different sections into which science may be divided, I think it may be said that their aim, in so far as it is not purely utilitarian, is always either historic or prophetic; and to the mathematician, history is only prophecy pursued in the negative direction. It is no argument against my definition of the objects of science, that a large section of its subdivisions has been, and to some extent still is, mainly occupied with the discovery and classification of facts; because such classification can only be a first step, preparing the way for a correlation into which the element of time must enter, and which therefore ultimately must depend either on history or prophecy.

Latterly men of science, and in particular physicists, have given increased attention to the intrinsic meaning of the concepts by means of which we express the facts of nature. Everything—who can deny it?—is ultimately reduced to sense impressions, and it has therefore been asserted that science is the study of the mind rather than of the outside world, the very existence of which may be denied. The physicist has thus invaded the realm of philosophy and metaphysics, and even claims that kingdom as his own. Two effects of these efforts, a paralysing pessimism and an obscure vagueness of expression, if not of thought, seriously threatened a few years ago to retard the healthy progress of the study of nature. If the outside world were only a dream, if we never could know what really lies behind it, the incentive which has moved those whose names stand out as landmarks in science is destroyed, and it is replaced by what? By a formula which only appeals to a few spirits entirely detached from the world in which they live. Metaphysicians and physicists will continue to look upon science from different points of view, and need not resent mutual criticisms of each other's methods or conclusions. For we must remember that most of the good that is done in this world is done by meddling with other people's affairs, and though the interference is always irritating and frequently futile, it proves after all that our interests converge towards a common centre.

According to Poincaré, the pleasure which the study of science confers consists in its power of uniting the beautiful and the useful; but it would be wrong to adopt this formula as a definition of the object of science, because it applies with equal force to all human studies. I go further, and say that the combination of the search for the beautiful with the achievement of the useful is the common interest of science and humanity. Some of us may tend more in one direction, some in another, but there must always remain a feeling of imperfection and only partial satisfaction unless we can unite the two fundamental desires of human nature.

I have warned you at the beginning of this discourse not to beat the utilitarian drum too loudly, and I have laid stress throughout on the idealistic side, though the most compelling events of the moment seem to drive us in the other direction, and the near future will press the needs of material prosperity strongly upon us. I must guard myself, therefore, against one criticism which the trend of my remarks may invite. At times, when the struggle for existence keeps masses in permanent bondage, in a society in which a multitude of men and women have to face starvation, and when unfortunate, though purely accidental, surroundings in childhood drive the weak into misery, is it not futile to speak of æsthetic motives? Am I not, while endeavouring to find a common bond between all sections of the community, in reality drawing a ring round a small and privileged leisured class, telling them these enjoyments are for you and for you alone? Should I not have found a surer ground for the claims of science in its daily increasing necessity for the success of our manufactures and commerce?

I have said nothing to indicate that I do not put the highest value on this important function of science, which finds its noblest task in surrendering the richness of its achievements to the use of humanity. But I must ask you to reflect whether the achievement of wealth and power, to the exclusion of higher aims, can lead to more than a superficial prosperity which passes away, because it carries the virus of its own doom within it. Do we not find in the worship of material success the seed of the pernicious ambition which has maddened a nation, and plunged Europe into war? Is this contempt for all idealistic purposes not the origin of the mischievous doctrine that the power to possess confers the right to possess, and that possession is desirable in itself without regard to the use which is made of it? I must therefore insist that if we delight in enlisting the wealth accumulated in the earth, and all the power stored in the orbs of heaven, or in the orbits of atomic structure, it should not be because we place material wealth above intellectual enjoyment, but rather because we experience a double pleasure if the efforts of the mind contribute to the welfare of the nation. When Joule taught us to utilise the powers at our disposal to the best advantage he did it not—and his whole life is a proof of it—to increase either his own wealth or that of the nation, but because, brought up in commercial life and deeply imbued with the deep insight and genius of science, he found his greatest delight in that very combination of æsthetic satisfaction and useful achievement which Poincaré has so well described. And again, when another of our fellow-citizens, Henry Wilde, showed how electrical power can be accumulated until it became an efficient instrument for the economic transmission of work, he found his inspiration in the intellectual gratification it gave him, rather than in the expectation of material gain. I am drawing no ring round a privileged class, but urge that the hunger for intellectual enjoyment is universal, and everybody should be given the opportunity and leisure of appeasing it. The duty to work, the right to live, and the leisure to think are the three prime necessities of our existence, and when one of them fails we only live an incomplete life.

I should have no difficulty in illustrating by examples, drawn from personal experience, the power which the revelations of science can exert over a community steeped in the petty conflicts of ordinary life; but I must bring these remarks to a conclusion, and content myself with the account of one incident.

An American friend, who possessed a powerful telescope, one night received the visit of an ardent politician. It was the time of a Presidential election, Bryan and Taft being the opposing candidates, and feeling ran high. After looking at clusters of stars and other celestial objects, and having received answers to his various questions, the visitor turned to my friend:—

"And all these stars I see," he asked, "what space in the heaven do they occupy?"

"About the area of the moon."

"And you tell me that every one of them is a sun like our own?"

"Yes."

"And that each of them may have a number of planets circulating round them like our sun?"

"Yes."

"And that there may be life on each of these planets?"

"We cannot tell that, but it is quite possible that there may be life on many of them."

And after pondering for some time, the politician rose and said: "It does not matter after all whether Taft or Bryan gets in."

Happy were the times when it could be said with truth that the strife of politics counted as nothing before the silent display of the heavens. Mightier issues are at stake to-day: for in the struggle which convulses the world, all intellectual pursuits are vitally affected, and science gladly gives all the power she wields to the service of the State. Sorrowfully she covers her face because that power, acquired through the noblest efforts of the sons of all nations, was never meant for death and destruction; gladly she helps, because a war wantonly provoked threatens civilisation, and only through victory shall we achieve a peace in which once more science can hold up her head, proud of her strength to preserve the intellectual freedom which is worth more than material prosperity, to defeat the spirit of evil that destroys the sense of brotherhood among nations, and to spread the love of truth.

SECTION A.

MATHEMATICS AND PHYSICS.

OPENING ADDRESS (SLIGHTLY ABRIDGED) BY SIR F. W. DYSON, M.A., LL.D., F.R.S., PRESIDENT OF THE SECTION.

ALTHOUGH at the present time our minds are largely absorbed by the war, the meeting of the British Association in Manchester indicates that we consider it right to make our annual review of scientific progress. I shall therefore make no apology for choosing the same subject for my address as I should have chosen in other circumstances. It is a subject far removed from war, being an account of the manner in which astronomers have with telescopes and spectroscopes investigated the skies and the conclusions they have reached on what Herschel called "The Construction of the Heavens."

Our knowledge of the fixed stars, as they were called by the old astronomers, is of comparatively recent origin, and is derived from two sources: (1) the measurement of small changes in the positions of the stars in the sky, and (2) the analysis of the light received from them and the measurement of its amount. To this end the numerous instruments of a modern observatory have been devised. The desire to examine fainter objects, and still more the necessity of increasing the accuracy of observations, has brought about a continuous improvement in the range and accuracy of astronomical instruments. Methods which had been perfected for observations of a few stars have been extended so that they can be applied to a large number. For these reasons the progress of sidereal astronomy may seem to have gone on slowly for a time. The more rapid progress of recent years arises from the accumulation of data, for which we are indebted to generations of astronomers, and from the gradual increase in power and perfection of our instruments.

The first insight into the stars as a whole naturally came from the survey of their numbers and distribution; and Herschel, who constructed the first great telescopes, explored the heavens with untiring skill and energy, and speculated boldly on his observations, is justly regarded as the founder of sidereal astronomy. In his great paper "On the Construction of the Heavens," Herschel gives the rules by which he was guided, which I should like to quote, as they may well serve as a motto to all who are engaged in the observational sciences:—

"But first let me mention that if we would hope to make any progress in an investigation of this delicate nature we ought to avoid two opposite extremes of which I can hardly say which is the most dangerous. If we indulge a fanciful imagination and build worlds of our own, we must not wonder at our going wide from the path of truth and nature; but these will vanish like the Cartesian vortices, that soon gave way when better theories were offered. On the other hand, if we add observation to observation, without attempting to draw not only certain conclusions but also conjectural views from them, we offend against the very end for which only observations ought to be made. I will endeavour to keep a proper medium; but if I should deviate from that I could wish not to fall into the latter error." In this spirit he discussed the "star gauges" or counts of stars visible with his great reflector in different parts of the sky, and concluded from them that the stars form a cluster which stretches to an unknown but finite distance, considerably greater in the plane of the Milky Way than in the perpendicular direction. He gave this distance as 497 times that of Sirius. He did not hesitate to advance the theory that some of the nebulæ were similar clusters of stars, of which that in Andromeda, judging from its size, was the nearest. Herschel had no means of telling the scale of the sidereal system, though he probably supposed the parallax of Sirius to be of the order of 1″.

Though some of the assumptions made by Herschel are open to criticism, the result at which he arrived is correct in its general outline. I shall attempt to give a brief account of some of the principal methods used to obtain more definite knowledge of the extent and constitution of this "island universe." The stars of which most is known are, in general, those nearest to us. If the distance of a star has been measured, its co-ordinates, velocity perpendicular to the line of sight, and luminosity are easily found. In the case of a double star the orbit of which is known the mass may also be determined. But only a very small proportion of the stars are sufficiently near for the distance to be determinable with any accuracy. Taking the distance corresponding to a parallax of 1″ or the parsec as unit—*i.e.* 200,000 times the distance of the earth from the sun—fairly accurate determinations can be made up to a distance of 25 parsecs, but only rough ones for greater distances.

For much greater distances average results are obtainable from proper motions, and the mean distances of particular classes of stars—for instance, stars of a given magnitude or given type of spectrum—can be found with confidence up to a distance of 500 parsecs, and with considerable uncertainty to twice this distance. The density of stars in space as a function of the distance, the percentage of stars within different limits of luminosity, the general trend of the movements of stars and their average velocities can also be found, within the same limits of distance.

For all distances, provided the star is sufficiently bright, its velocity to or from the earth can be measured. The general consideration of these velocities supplies complementary data which cannot be obtained from proper motions, and confirms other results obtained by their means. For distances greater than 1000 parsecs our knowledge is generally very vague. We have to rely on what can be learned from the amount and colour of the light of the stars, and from their numbers in different parts of the sky.

Parallax.

Let us begin with the portion of space nearest to us, within which the parallaxes of stars are determinable. The successful determination of stellar parallax by Bessel, Struve, and Henderson in 1838 was a landmark in sidereal astronomy. The distances of three separate stars were successfully measured, and for the first time the sounding line which astronomers had for centuries been throwing into space touched bottom. The employment of the heliometer which Bessel introduced was the main source of our knowledge of the distances of stars until the end of the nineteenth century, and resulted in fairly satisfactory determination of the parallaxes of nearly one hundred stars. For the part of space nearest to us this survey is sufficiently complete for us to infer the average distances of the stars from one another—2½ to 3 parsecs. The parallax determinations of double stars of known orbits lead to the result that the masses of stars have not a very great range, but lie between forty times and one-tenth of the mass of the sun.

When the absolute luminosities of the stars the distances of which have been measured are calculated, it is found that, unlike the masses, they exhibit a very great range. For example, Sirius radiates forty-eight times as much light as the sun, and Groombridge 34 only one-hundredth part. This does not represent anything like the complete range, and Canopus, for example, may be ten thousand times as luminous as the sun. But among the stars near the solar system, the absolute luminosity appears to vary with the type of spectrum. Thus Sirius, of type A, a blue hydrogen star, is forty-eight times as luminous as the sun; Procyon of type F5—bluer than the sun, but not so blue as Sirius—ten times; α Centauri, which is nearly of solar type, is twice as luminous. 61 Cygni of type K5—redder than the sun—one-tenth as luminous; while the still redder star of type Ma, Gr 34, is only one-hundredth as luminous. In the neighbourhood of the solar system one-third of the stars are more luminous and two-thirds less luminous than the sun. The luminosity decreases as the type of spectrum changes from A to M, *i.e.* from the blue stars to the red stars.

These three results as to the density in space, the mass, and the luminosity have been derived from a very small number of stars. They show the great value of accurate determinations of stellar parallax. So soon as the parallax is known, all the other observational data are immediately utilisable. At the commencement of the present century the parallaxes of perhaps eighty stars were known with tolerable accuracy. Happily the number is now rapidly increasing by the use of photographic methods. Within the last year or two, the parallaxes of nearly two hundred stars have been determined and published. This year a committeee of the American Astronomical Society, under the presidency of Prof. Schlesinger, has been formed to co-ordinate the work of six or seven American and one or two English observatories. The combined programme contains 1100 stars, of which

400 are being measured by more than one observatory. We may expect results at the rate of two hundred a year, and may therefore hope for a rapid increase of our knowledge of the stars within our immediate neighbourhood.

Velocities in the Line of Sight.

The determination of radial velocities was initiated by Huggins in the early 'sixties, but trustworthy results were not obtained until photographic methods were introduced by Vogel in 1890. Since that time further increase in accuracy has been made, and the velocity of a bright star with sharp lines is determinable (apart from a systematic error not wholly explained) with an accuracy of ¼ kilometre per second. As the average velocities of these stars are between 10 and 20 kilometres a second, the proportional accuracy is of a higher order than can be generally obtained in parallax determinations or in other data of sidereal astronomy. A number of observatories in the United States and Europe, as well as in South America, the Cape, and Canada, are engaged in this work. Especially at the Lick Observatory under Prof. Campbell's direction, the combination of a large telescope, a well-designed spectroscope, and excellent climatic conditions have been utilised to carry out a bold programme. At that observatory, with an offshoot at Cerro San Christobal in Chile, for the observations of stars in the southern hemisphere, the velocities of 1200 of the brightest stars in the sky have been determined. Among the results achieved is a determination of the direction and amount of the solar motion. The direction serves to confirm the results from proper motions, but the velocity is only obtainable accurately by this method. This quantity, which enters as a fundamental constant in nearly all researches dealing with proper motion, is given by Campbell at 19·5 kilometres per second, or 4·1 times the distance of the earth from the sun per annum, though there is some uncertainty arising from a systematic error of unknown origin.

The observations of radial velocities have shown within what limits the velocities of stars lie and have given a general idea of their distribution. The most important result, and one of a somewhat surprising character, is that the mean velocities of stars, the motion of the sun being abstracted, increase with the type of spectrum. Thus the stars of type B, the helium stars, the stars of the highest temperature, have average radial velocities of only 6·5 kilometres per second; the hydrogen stars of type A have average velocities of 11 kilometres per second; the solar stars of 15 kilometres per second; while for red stars of types K and M it has increased slightly more to 17 kilometres per second. Further, the few planetary nebulæ—*i.e.* condensed nebulæ with bright line spectra—have average velocities of 25 kilometres per second. There can be no question of the substantial accuracy of these results, as they are closely confirmed by discussions of proper motions. They are, however, very difficult to understand. On the face of it, there does not seem any reason why stars of a high temperature should have specially high velocities. A suggestion has been thrown out by Dr. Halm that as the helium stars have greater masses, these results are in accordance with an equi-partition of energy. But the distances of stars apart is so great that it seems impossible that this could be brought about by their interaction. Prof. Eddington suggests that the velocities may be an indication of the part of space at which the stars were formed (*e.g.* stars of small mass in outlying portions), and represents the kinetic energy they have acquired in arriving at their present positions.

The stars the radial velocities of which have been determined are, generally speaking, brighter than the fifth magnitude. Fainter stars are now being observed. At the Mount Wilson Observatory, Prof. Adams has determined the velocities of stars of known parallaxes, as there are great advantages in obtaining complete data for stars where possible. Extension of line-of-sight determinations to fainter stars is sure to bring a harvest of useful results, and a number of great telescopes are engaged, and others will shortly join in this important work.

Proper Motions.

As proper motions are determined by the comparison of the positions of stars at two different epochs, they get to be known with constantly increasing accuracy as the time interval increases. The stars visible to the naked eye in the northern hemisphere were accurately observed by Bradley in 1755. Many thousands of observations of faint stars down to about 9·0m. were made in the first half of the nineteenth century. An extensive scheme of re-observation was carried out about 1875 under the auspices of the Astronomische Gesellschaft. A great deal of re-observation of stars brighter than the ninth magnitude has been made this century in connection with the photographic survey of the heavens. For the bright stars all available material has been utilised and their proper motions have been well determined, and for the fainter stars this is being gradually accomplished.

Proper motions differ widely and irregularly in amount and direction. Herschel observed a tendency of a few stars to move towards one point of the sky, and attributed this sign of regularity to a movement of the solar system in the opposite direction. But puzzling differences given by different methods remained unexplained until the difficulty was resolved by Prof. Kapteyn in a paper read before this section of the British Association at its meeting in South Africa ten years ago. He showed that the proper motions had a general tendency towards two different points of the sky and not towards one only, as would be expected if the motions of the stars themselves were haphazard, but viewed from a point in rapid motion. He concluded from this that there was a general tendency of the stars to stream in two opposite directions. It is interesting to notice that this great discovery was made by a simple graphical examination of the proper motions of stars in different regions of the sky, after the author had spent much time in examining and criticising the different methods which had been adopted for the determination of the direction of the solar motion. The subject was brought into a clearer and more exact shape by the analytical formulation given to it by Prof. Eddington, and after him by Prof. Schwarzschild.

This star-streaming is corroborated by observations of velocities in the line of sight. It applies—with the exception of the helium stars—to all stars which are near enough for their proper motions to be determinable. We may say with certainty that it extends to stars at distances of two or three hundred parsecs; it may extend much further, but I do not think we have at present much evidence of this. Prof. Turner pointed out that the convergence of proper motions did not necessarily imply movements in parallel directions, and suggested that the star-streams were movements of stars to and from a centre. The agreement of the radial velocities with the proper motions seems to me to be opposed to this suggestion, and to show that star-streaming indicates approximate parallelism in two opposite directions in the motions of the stars examined. As the great majority of these stars are comparatively near to us, it is possible that this parallelism is mainly confined to them, and indicates the general directions of the orbital motions of stars in the neighbourhood. An attempted explanation on these lines, as on Prof. Turner's, implies that the sun is some distance from the centre of the stellar system.

A discovery of an entirely different character was made by Prof. Boss in 1908. He spent many years in constructing a great catalogue giving the most accurate positions and motions of 6200 stars obtainable from all existing observations. This catalogue, which was published by the Carnegie Institute, was intended as a preliminary to a still larger one which would give the accurate positions and motions of all the stars down to the seventh magnitude. In the course of this work Prof. Boss found that forty or fifty stars scattered over a considerable region of the sky near the constellation Taurus were all moving towards the same point in the sky and with nearly the same angular velocity. He inferred that these stars were all moving in parallel directions with an equal linear velocity, and the supposition was verified, in the case of several of them, by the determination of their radial velocities. From these data he was able to derive the distance of each star and thus its position in space. The existence of a large group of stars, separated from one another by great distances, and all having the same motion in space, is a very remarkable phenomenon. It shows, as was pointed out by Prof. Eddington, how small is the gravitational action of one star on another, and that the movement of each star is determined by the total attraction of the whole mass of the stars. Several other interesting moving clusters have been found since. For all the stars belonging to these clusters, the distances have been found, and from them luminosities and velocities of individual stars, particulars which are generally only obtainable for stars much nearer to us.

Proper motions are the main source of our knowledge of the distances of stars which are beyond the reach of determination by annual parallax. If a star were known to be at rest its distance could be calculated from the shift of its apparent position caused by the translation of the solar motion. As the solar system moves 410 times the distance of the earth from the sun in a century, this gives a displacement of $1''$ for a star at the distance of 500 parsecs. This method has been applied by Kapteyn to determine the distances of the helium stars, as their velocities are sufficiently small to be neglected in comparison with that of the solar system. But generally it is only possible to find the mean distances of groups of stars of such size that it may be assumed that the peculiar motions neutralise one another in the mean. For example, the average distance of stars of type A, or stars of the fifth magnitude, or any other group desired may be found. In this way Kapteyn has found from the Bradley stars that the mean parallax of stars of magnitude m is given by the formula

$$\log. \pi_m = -1{\cdot}108 - 0{\cdot}125\, m.$$

In conjunction with another observational law which expresses the number of stars as a function of the magnitude, this leads to a determination of the density of stars in space at different distances from us, and also of the "luminosity law," *i.e.* the percentage of stars of different absolute brightness. Profs. Seeliger and Kapteyn have shown in this way that there is a considerable falling off of star-density as we go further from the solar system. It seems to me very necessary that this should be investigated in greater detail for different parts of the sky separately. A general mathematical solution of general questions which arise in the treatment of astronomical statistics has been given by Prof. Schwarzschild. His investigations are of the greatest value in showing the exact dependence of the density, luminosity, and velocity laws on the statistical facts which can be collected from observation. The many interesting statistical studies which have been made are liable to be rather bewildering without the guidance furnished by a general mathematical survey of the whole position.

When the proper motions are considered in relation to the spectral types of the stars, the small average velocities of the hydrogen stars and still smaller ones of the helium stars found from line-of-sight observations are confirmed. If stars up to a definite limit of apparent magnitude, say, to 6·0m., or between certain limits, say 8·0m. and 9·0m., are considered, then the solar stars are found to be much nearer than either the red or the blue stars. Thus both red and blue stars must be of greater intrinsic luminosity than the solar stars. As regards blue stars, this agrees with results given by parallax observations. But the red stars appear to consist of two classes, one of great and one of feeble luminosity, and it does not seem that a sufficient explanation is given by the fact that a selection of stars brighter than any given apparent magnitude will include the very luminous stars which are at a great distance, but only such stars of feeble luminosity as are very near.

The significance of these facts was pointed out by Prof. Hertzsprung and Prof. Russell. They have a very important bearing on the question of stellar evolution, a subject for discussion at a later meeting this week. From the geometrical point of view of my address these facts are of importance in that they help to classify the extraordinarily large range found in the luminosities of stars. Putting the matter somewhat broadly, the A stars, or hydrogen stars, are on the average intrinsically 5 magnitudes brighter than the sun, whilst the range in their magnitudes is such that half of them are within ¾ magnitude of the mean value. The stars of type M, very red stars, are of two classes. Some of them are as luminous as the A stars, and have a similar range about a mean value 5 magnitudes brighter than the sun. Others, on the contrary, have a mean intrinsic brightness 5 magnitudes fainter than the sun and with the same probable deviation of ¾ magnitude. Between the types M and A there are two classes the distance apart of which diminishes as the stars become bluer. The facts in support of this contention are very forcibly presented by Prof. Russell in NATURE in May, 1914. If this hypothesis is true, and it seems to me there is much to be said in its favour, then the apparent magnitude combined with the type of spectrum will give a very fair approximation to the distances of stars which are too far away for their proper motions to be determinable with accuracy.

In dealing with the proper motions of the brighter stars, the sky has been considered as a whole. Now that the direction and amount of the solar motion are known, we may hope that, as more proper motions become available, the different parts of the sky will be studied separately. In this way we shall obtain more detailed knowledge of the streaming, and also of the mean distances of stars of different magnitudes in all parts of the sky, leading to a determination of how the density of stars in space changes in different directions. A second line of research which may be expected to give important results is in the relationship of proper motions to spectral type. There is in preparation at Harvard College by Miss Cannon, under Prof. Pickering's direction, a catalogue giving the type of spectrum of every star brighter than the ninth magnitude. It would be very desirable to determine the proper motions of all these stars. If all the material available is examined it should be possible to do this to a very large extent.

Photometry and Colour.

For the more distant parts of the heavens proper motions are an uncertain guide, and we must depend on what can be learned from the light of the stars by means of stellar photometry, determinations of colour, and studies of stellar spectra. Speaking generally, we attempt to discover from the nearer stars sufficient about their intrinsic luminosities to enable us to use the apparent magnitude as an index of the distances of the stars which are further away. The most striking example is found in Prof. Hertzsprung's determination of the distance of the small Magellanic cloud. From a knowledge of the characteristics of the Cepheid variables found in this cloud by Miss Leavitt, and their apparent magnitude, he deduced the distance of the cloud as 10,000 parsecs.

Much attention has been given of late years to stellar photometry. In 1899 Prof. Pickering published the Revised Harvard Photometry giving the magnitudes of all stars brighter than 6·5m. In 1907 Messrs. Müller and Kempf completed a determination of 14,199 stars of the northern hemisphere brighter than 7·5m. In 1908 a catalogue of 36,682 stars fainter than 6·5m. was published at Harvard. These determinations derive additional importance as they give the means of standardising estimates of magnitude made by eye, particularly the many thousands of the Bonn Durchmusterung.

By the labours of Prof. Pickering and his colleagues at Harvard, Prof. Schwarzschild, Prof. Parkhurst at Yerkes, Prof. Seares at Mount Wilson, and others, the determinations of the magnitudes of stars by photography has made rapid strides. As yet no complete catalogues of photographic magnitude corresponding to the Revised Harvard Photometry have been published, though considerable parts of the sky and special areas, such as the Pleiades, have been carefully studied. The determination of the photographic magnitudes of any stars which may be required is, however, a comparatively simple matter when the magnitudes of sufficient standard stars have been found. A trustworthy and uniform scale has been to a large extent secured by the use of extra-focal images, gratings, and screens in front of the object glass, and the study of the effects of different apertures and different times of exposure.

At Harvard and Mount Wilson, standard magnitudes of stars near the north pole have been published extending to nearly the twentieth magnitude. In the part of the range extending from 10·0m. to 16·0m. these agree very satisfactorily. There is, however, a difference of 0·4m. in the scale between 6·0m. and 10·0m. which needs to be cleared up.

A uniform and accurate scale of magnitude is of fundamental importance in counts of the numbers of stars. Such counts aim at the determination of two things: (1) how the numbers vary in different parts of the sky, and (2) what is the ratio of the number of stars of each magnitude to that of the preceding magnitude in the same area of the sky. The counts of stars from the gauges of Sir William and Sir John Herschel, those of the stars contained in the Bonn Durchmusterung, those made by Prof. Celoria, and the recent counts of the Franklin-Adams plates by Dr. Chapman and Mr. Melotte, all agree in showing a continuous increase of stars as we proceed from the pole of the galaxy to the galaxy itself. The importance of this fact is that it shows a close connection between the Milky Way and the stars nearer to us. The Milky Way is not a system of stars beyond the others, but is the primary feature of our "island universe."

Photometric observations have acquired additional importance from the differences between photographic and visual magnitudes. The ordinary plate is more sensitive to blue light than the eye, and the difference between the photographic and visual (or photo-visual) magnitude of a star is an index of the colour. The colour index is found by observation to be related very closely to the type of spectrum. Prof. Seares has shown from the colour indices that as the stars become fainter they become progressively redder. Prof. Hertzsprung has found the same thing by the use of a grating in front of the object glass. Among stars of 17·0m. visual magnitude, Seares found none with a colour index less than 0·7; this is approximately the colour index of a star of solar type, *i.e.* near the middle of the range from blue stars to red stars.

There are three ways in which this may occur. The stars may be bright but very distant red stars; or they may be faint red stars, like those in the immediate neighbourhood of the sun; or there may have been an absorption of blue light. It is not possible to say in what proportion these causes have contributed. The red stars of 9·0m. and 10·0m. are nearly all very luminous but distant bodies, but it seems likely that stars of 17·0m. will contain a greater proportion of stars of small luminosity.

The absorption of light in space is very small, and as yet imperfectly determined. Prof. Kapteyn and Mr. Jones, by comparing the colour indices of stars of large and small proper motion, make the difference between the absorption of photographic and visual light as 1m. in 2000 parsecs. The question has been examined directly by Prof. Adams, who has obtained spectra of near and distant stars which are identical as regards their lines, and has examined the distribution of the continuous light. This direct method of comparison showed that the more distant star was always weaker in violet light. But as both these investigations show that very luminous stars are intrinsically somewhat bluer than less luminous stars of the same spectral type, the two causes require further research for their disentanglement. The question is of importance, as it may serve in some cases to determine the distances of very remote bodies the type of spectrum of which is known.

It must be admitted that we are as yet very ignorant of the more distant parts of the "island universe." For example, we can make little more than guesses at the distance of the Milky Way, or say what part is nearest to us, what are its movements, and so on. But, nevertheless, the whole subject of the construction of the heavens has been opened up in a remarkable manner in the last few years. The methods now employed seem competent to produce a tolerably good model showing the co-ordinates and velocities of the stars as well as their effective temperatures and the amount of light they radiate. Industry in the collection of accurate data is required, along with constant attempts to interpret them as they are collected. The more accurate and detailed our knowledge of the stellar system as it is now, the better will be our position for the dynamical and physical study of its history and evolution.

NOTES.

THE director of the Meteorological Office reports that information has been received from the Seismological Observatory at Eskdalemuir, Scotland, of the record of a large earthquake which occurred at 1 a.m. on Tuesday, September 7. The computed position of the epicentre is lat. 9° N., long. 86° W., with a possible error of 10°. The position mentioned is in the Pacific Ocean, about 70 miles from Cape Blanco