Robey, Jr., present some 'Studies in the Mechanism of Agglutination,' W. R. Brenckerhoff describes 'The Pathology of Azoturia,' and A. W. Balch notes 'A Possible Cause of Azoturia,' Allen Cleghorn discusses 'The Physiological Effects and the Nature of Extracts of Sympathetic Ganglia,' R. W. Lowett has a note on 'Movements of the Normal Spine in their Relation to Scoliosis,' and Harold C. Ernst treats of 'Actinomycosis of the Udder of the Cow.'

DISCUSSION AND CORRESPONDENCE.

THE MISUSE OF TECHNICAL TERMS.

WHEN a layman discourses upon a professional subject, as when a cobbler expounds electricity, he may be excused for lack of familiarity with technicalities, and for a consequent misuse of technical terms, though one may question the propriety of his posing as an authority on the subject. The case is different with a professional man who has acquired the right to speak by reason of high position or meritorious work, or both. A misuse of terms in speaking to others of his own profession would probably not affect them seriously, as they would perceive the error and discern the truth that is behind it, or the mistake, if there be any, in the conclusions. But when he addresses an audience of laymen, it is incumbent upon him to be careful of his language, especially in scientific matters, since these are loaded down with technical terms of great exactness in meaning, the wrong use of which may result to the uninitiated in error as well as confusion. To be sure, the use of abtruse or uncommon terms where common words would convey the true meaning is tiresome pedantry in professor and layman alike, but whether the words are common or uncommon, let them be used correctly.

In these days of specialties and specialists a highly trained authority in one profession is a layman among the members of another, and to some degree is at their mercy as to technical terms; still more so are those who, without special training, but with intelligence enough to appreciate the ability of scholars, and with a corresponding interest in such features of the various professions as touch upon their life and experience, must depend upon what they can get from the utterances of professional men in quasi-scientific or semi-professional form, to satisfy their thirst for knowledge. They constitute the greater portion of the readers of scientific journals which are not strictly technical, and they want expressions in regard to science that are more authoritative than those appearing in the daily newspaper, where, for example, we read from time to time that a man has taken an electric *current* of an incredible number of volts. The diffusing and popularizing of science has introduced its nomenclature to a wide circle of such interested but imperfectly trained readers, and has made accuracy in the use of terms more important now than ever before. There is no need to go to excess in technicalities. Now and then we encounter papers or even extended treatises in which the author delights in coining words, after the manner of a Heine or a Richter, apparently expecting thereby to enrich our vocabulary, but while occasionally a term thus introduced stays, most commonly it falls out of use very speedily. That is something that takes care of itself. Our only contention is that a technical term should be used correctly, if at all, and that this is the more imperative if the term is not a rare one.

It is true that in some branches of science, possibly in all branches, there are theories or hypotheses, not yet well enough established, phenomena not well enough understood to make it possible to give to the terms the exact significance they may come to possess later. This was illustrated in the discussions that abounded four years ago, regarding the X-rays, and the proper words to designate the radiant agent and the pictures produced by it. In response to a request for a suitable name for the latter, no fewer than twenty were suggested to *The Electrical World*, with special reasons for each one. Of these twenty none has been generally adopted, and today the pictures may be called by any one of several names without violating propriety or precision. It is quite different, however, in regard to certain names in subjects that have passed a transition stage, and probably in none has the nomenclature been more fully developed or better established than in mechanics. In this science new words are proposed from time to time, but not to change or to substitute those already in use. There was a time when the word 'energy' (not the idea) was new; when force was confounded with work; when power was not generally so completely distinguished from both force and work as it now is. There was then justification for Grove to write of 'The Correlation and Conservation of Forces,' and for Helmholtz to discuss the 'Conservation of Force' (*Erhaltung der Kraft*). But from the somewhat turbid and confused use of terms fifty years ago, there crystallized out a clear and well-defined meaning to go with each one of several names, and these have become so well established that there is little excuse for misuse of them. Of these force, work, energy, and power are or should be among the most familiar, and a scientific writer who confounds one of them with another is as gravely at fault as a literary critic who confounds will and shall, or lie and lay.

It was a little surprising, therefore, not very long ago to read in a leading educational journal, an article by the physical director of a leading university, discussing such exercises as jumping, running, and the like, seriously comparing the *force* necessary to raise the performer six feet with that required to lift him one foot, and implying that the force increased with the height. Just when the laws of gravitation were so changed as to make a man heavier at two feet above the earth than at a height of one foot it would be interesting to learn. That the impulse needed to give him the requisite velocity to rise a definite height, might be acquired by means of *any* force in excess of his weight according to the time during which it is exerted was overlooked or at least not taken into account. In other portions of the article, also appeared misconceptions of the relations of force exerted, time of its action, work expended, and energy acquired, as witnessed by such expressions as 'number of foot pounds lifted,' 'every pound of energy,' etc. It would be ungracious to doubt that the author knew what he meant, but he used technical terms in mistaken senses.

Still more awry are the statements of a writer in a recent number of one of the best and most popular engineering magazines, in expatiating upon the power in a pound of coal. On their face these statements are striking, so much so as to be reprinted by newspapers culling the best extracts from the current literary and scientific magazines. Examined critically they are astonishing. The writer is at some pains in the beginning to explain the meaning of work and to point out that 'in all mechanical work we must consider the element of time,' and then follows with statements that ignore it, by contrasting the energy of the pound of coal with a man-power, or a horse-power. He recurs frequently to the idea 'that this pound of coal contains within it the power of 236 horses.' Its power might as well have been declared that of 472 horses, or of only 118 horses, or of any other number. 'It is interesting,' he continues, 'to follow this out in order to find how much of the power which nature has given us in this pound of coal we are able to get out of it.' Nature has given us not 236 horse-power in the pound of coal, but an indefinite power, and if we can get any work out of it the power is only limited by the rate at which the work can be obtained; for example, the number of foot-pounds per minute.

Apparently the reason in some of these instances for using the word power instead of energy is the idea that possibly the latter term may be less familiar to the reader than the former; but if both are to be used in a strictly technical sense, one is probably as generally understood as the other, and certainly if the author had taken as much pains to define energy in the beginning as he did to define power, he would have cleared the way for the use of the term in its proper sense instead of continually using the other word in a sense at variance with his own explanation. It is true that we do sometimes come upon the word power for force, as where the power and power arm, are compared with the resistance or the weight and its arm. The term *activity* has been introduced to get rid of the tendency to misuse the word power, but even if we admit the untechnical meaning of the word as it appears in the title of the article referred to, the moment it appears in the compound 'horse-power' it is inevitably technical in form and in meaning. As such it is not force, nor work, but rate of working.

One is not surprised at such confusion of meanings in the productions of men who appear from time to time with crude and hazy schemes for correcting or overthrowing all existing systems of science or philosophy, and who make professors weary by their importunities, but one does not look for it in an honored seat of learning. To find it there makes one wonder whether Helmholtz is unknown, or Maxwell has lived in vain.

Although mechanics as the oldest of sciences has been chosen for these illustrations it is likely that every other branch of science can show similar perversions. We have even known the 'parallels of longitude' to be referred to in all seriousness, but as that was not uttered by a scientific man it is rather to be smiled at than criticized.

D. W. HERING.

CORRECTION.

IN No. 277 of SCIENCE, I stated that the $MgCl_2$ solution used in my experiments on artificial parthenogenesis was a $20/8n$ solution. I have since found that the assistant who made the Mg-solution used and who has left the laboratory must have made a mistake as the solution contained only about 120 g. of $MgCl_2$ in a liter. This does not affect my results, but might be an obstacle to the successful repetition of my experiments by others.

JACQUES LOEB.

CHICAGO, June 8, 1900.

NOTES ON ELECTRICAL ENGINEERING.

SUBMARINE TELEPHONY.

IF waves were sent along a string stretched under water the effect of the water would be to *damp* the motion of the string causing the waves to become more and more attenuated as they travel along the string, and to *distort* the waves so that a wave initially complicated in shape would be smoothed and spread out more and more as it travels along the string.

Assuming the damping or frictional force of the water on the string at a point to be strictly proportional to the sidewise velocity of the string at that point, it can be shown that there is a certain relation between the tension of the string and its weight per unit length for which the attenuation of a wave is a minimum and for which a wave is not distorted as it travels along the string.

A submarine cable for telegraphy behaves in a manner entirely analogous to a string stretched through a viscous fluid as described above. Electrical impulses acting at one end of the cable produce electrical waves which travel along the conductor of the cable. The electrical resistance of the conductor is analogous to the frictional resistance of the water on the string, the self-inductance of the conductor is analogous to the weight of the string and the inductive capacity of the gutta-percha insulation is analogous to the tension on the string.

There is a certain relation between resistance of conductor, self-inductance of conductor, and inductive capacity of the gutta-percha covering for which electrical waves suffer minimum attenuation and no distortion as they travel along the cable. Oliver Heaviside first called attention to this condition for the *distortionless circuit*, as it is called and Dr. M. I. Pupin, in a paper read before the American Physical Society in December, showed that the distortionless condition can be realized practically by *distributed inductance* that is by connecting small coils of wire at intervals along a cable or land line.

The practical importance of the distortionless circuit is great inasmuch as such a circuit would greatly extend the possible speed of ocean telegraphy and perhaps even make ocean telephony possible.

The limit of speed of ocean telegraphy is set mainly by the distortion of the electrical impulses which pass along the cable. This distortion causes the impulses at the receiving end to overlap each other greatly.

The limitation of long distance telephony is set in part by the attenuation of the electrical waves and in part by the distortion of the waves. The first makes it difficult to produce an audible effect at the distant receiver and the second so changes the character of the waves that the sound in the distant receiver becomes more or less indistinct or inarticulate, consonant sounds are especially liable to become confused in this way.

W. S. F.