erate amount of attention might greatly modify these conditions, and might do for the eyes what is done by athletic games and exercises for the muscles.

A still different explanation of the phenomenon is given by G. B. Buckton (*Nature*, No. 801, p. 407). The same amount of light entering the eyes of different individuals produces widely different effects, according to health or age. A student becoming accustomed to see objects from a short distance, will permanently accommodate himself to a short focus, and hence become short-sighted. Such modifications can be conceived of as being hereditary, and long-sight might be brought about in a race by the opposite use of the eye.

Apropos to this discussion, it may be well to notice briefly a discussion upon near-sightedness, which was started by Lord Rayleigh's article. The investigation of the question of the increasing prevalence of short-sight, which has recently been carried on in Germany, has led to legislative restrictions in the schools. The numerous statistics from the German schools have shown that the proportion of short-sighted boys continually increases from form to form; and from this fact it has been argued that the continued use of the eyes for the perception of near objects, is the essential, if not the only, factor in the production of short-sight. This view is again supported by the statistics, which allot the largest proportion of short-sighted individuals to those branches of industry, or those pursuits, which constantly call for near vision. In this connection, Mr. George A. Berry (*Nature*, No. 800, p. 387) suggests that two points have been forgotten in arriving at such a conclusion. In the first place, there is an undoubted tendency to grow short-sighted with age alone, up to the period of cessation of growth. This has been shown to be due to the elongation of the antero-posterior axis of the eye, and is no more a disease than is the attainment of more than an average height by certain individuals. It is merely a type; and, as such, is governed by the laws of heredity. A small proportion of cases are, however, due to disease; and these are as frequent among the illiterate as the educated, and are not hereditary. In many cases, people drift into literary and similar pursuits because they are near-sighted, and not well adapted for other occupations. Further, as a man's circle of acquaintance is, for the most part, amongst individuals having similar interests in life, intermarriage in myoptic families must frequently occur, and would tend to perpetuate, and perhaps increase, the defect. In savages, on the other hand, where the great principle of survival of the fittest is not frustrated to the same extent as among civilized races, every thing would be against the perpetuation of a myoptic type.

CHEYNE'S OBSERVATIONS ON THE CHOLERA MICROBE.

IN connection with the work of Van Ermengem upon the cholera bacillus (*Science*, No. 133), that of Cheyne, recently published (*British med. journ.*, April 25–May 23, 1885), deserves attention. This gives the results of his investigations at Paris during the epidemic of cholera, and afterwards at his own laboratory. In eight cases investigated, he found the curved bacilli in larger or smaller numbers: in a ninth case, supposed to be cholera, but turning out not to be, no curved bacilli were found. He failed to demonstrate these bacilli in the walls of the intestines in almost all cases; and, when he succeeded in finding them at all, they were very indistinct. He very justly observes, however, that Koch may succeed in such a demonstration where others fail; for Koch's technique is unquestionably superior to that of any other worker in this field.

Having sent his slides and cultures to Koch, and having the latter's assurance that they were pure, and made up of the curved bacillus of Asiatic cholera, Cheyne made various experiments with them in culture-media of different kinds and at different temperatures, the results of all of which were in conformity with what was already known. In particular, he found no difficulty in repeating Koch's observation, that drying rapidly destroys the vitality of these organisms: 'in three hours they are completely dead.'

His conclusions are, that the comma bacillus was present, and generally in large numbers, in all the cases of cholera which were examined; and that he has never met with the cholera bacillus except in cholera, and that the other curved bacilli described (Finkler and Prior's, Lewis's, and Dencke's or Flügge's), differ from it in important particulars.

Inoculation experiments were performed on seventeen guinea-pigs, with successful results in only two. Two other animals died, but were not examined, because destroyed. (We would suggest greater care of his inoculated animals, for these misfortunes seem to be but a repetition of those that happened in this observer's work on tuberculosis; see *Practitioner*, April, 1883.)

The last part of Mr. Cheyne's article is devoted to an able refutation of Klein's arguments against the specific nature of the comma bacillus. He shows the hasty work of this observer, which has led him to conclusions so entirely at variance with those of Koch and his supporters. He (Cheyne) thus summarizes his opinion of the work of the English cholera commission: "The two errors which, in my opinion, lie at the root of the work of the English cholera commission are, first, that, acting on the idea that Koch diagnosed the cholera bacilli by the microscope alone, they proceeded to investigate the matter by microscopic examination; and, secondly, that, seeing the stress which Dr. Koch laid on the cultivation appearances, they concluded that he meant to say that the organism was pathogenic, because it grows in a particular manner: and, therefore, they naturally proceeded to inquire whether the appearance of the cultivations, as compared with cultivations of other bacteria, could warrant this conclusion; and, of course, they found that it could not."