bestow our warmest praise on the manner in which the author has executed the task entrusted to him. The "Vegetable Physiology" is one of the most instructive, and, at the same time, entertaining books with which we are acquainted; the style is pleasing; the illustrations apt; every page is a happy combination of the *utile* and the *dulce*. The volume, moreover, contains about sixty-four woodcuts. If the succeeding numbers of the Popular Cyclopædia sustain the reputation which the one before us must attain, they will form an instructive and highly useful series of works, "popular" in every sense of the word.

A Series of Anatomical Sketches and Diagrams, with Descriptions and References. Part IV.
By THOMAS WORMALD and ANDREW M. McWHINNIE. Highley, London, 1841.

As we have only seen one part (IV.) of the present work, we are unable to give any idea of the plan upon which the authors propose to conduct it, or of the extent, &c., to which it may probably reach. We must, therefore, confine our observations to the number now before us. This number, or part, contains four anatomical sketches which are well calculated to fulfil the double purpose of refreshing the ideas of the practitioner upon anatomy, and of aiding the pupil in his dissecting-room labours.

In the first plate we have two sketches, representing the course of the thoracic duct in the neck, and the distribution of the laryngeal nerves. In the second plate is represented the axillary plexus; in the third, we have the anatomy of the axilla; and, in the fourth, we find two very excellent views of the elbow-joint.

The sketches in the present part are executed in a careful manner, and with much fidelity; and we have no doubt but that the work will prove an acquisition to students and junior practitioners.

ON SLEEP;

WITH ARGUMENTS IN FAVOUR OF THE DOCTRINE OF AFFLUX OF BLOOD TO THE BRAIN DURING THIS STATE.

(Read before the Royal Physical Society of Edinburgh.)

By LEWIS ASHENHEIM, M.D. L.R. C.S.E., &c.

BEFORE entering on his subject, Dr. Ashenheim examines the various theories respecting sleep, which have been advanced by Scaliger, Blumenbach, Cullen, Ferriar, Darwin, Combe, and other writers. He then proceeds to deliver his own views in the following terms:—

"Although, as before stated, I have at present no satisfactory theory to offer, yet I have my opi-

nions as to what may be the cause of sleep; and although these opinions may perhaps have been precipitately formed and ill digested, yet, in some points at least, they are borne out by analogy. It is my belief, that in sleep there is an afflux of blood to the brain, notwithstanding the high authorities that are opposed to this view; and my reason for so believing, is the occurrence of some of the symptoms which present themselves in compression of the brain, which is often owing to a determination of blood to, or an effusion of the same on the surface, or in the substance of the brain. Let us now examine some of these symptoms; in compression of the brain, the pulse is slow, so is it in sleep; in compression of the brain, the surface is often natural, so is it in sleep; in the disease, the limbs are relaxed, so are they in sleep; in compression, there is insensibility to light and sound, so is there in sleep; in the former, there is stertorous breathing, so is there sometimes in the latter. Now, although I cannot at present prove to you that there is really an afflux of blood to the brain during sleep, and drawing my conclusions, as I do, from the above corresponding circumstances, yet it cannot be deemed presumptuous in me to advance, that such *may* actually be the case. Blumenbach has endeavoured to prove, that the circulation is diminished, and the quantity of blood less in the brain during sleep, and in doing so, he quotes a case of a patient of his, who had been trepanned, and in whom the brain was observed to sink during sleep, and to swell when he was awake. McNish and others join Blumenbach in his conclusions. But they seem to have forgotten one important fact, and that is, that the brain was subjected to the atmospheric pressure. The question may then be asked, why did it swell when the man was awake? *that* I answer thus, that as in the waking state, the circulation is more rapid than in the sleeping, the brain is in a fitter condition to resist the pressure. In a conversation which I lately had with a friend upon this subject, it was argued by him that there is *not* an afflux of blood to the brain during sleep, and, in endeavouring to prove this assertion, he quoted cases of children and others, who, after losing a great quantity of blood, had died in a state of complete sopor, and in whom he said that there possibly could not have been an accumulation or pressure of the vital fluid upon the brain, as *that* had, before death, been literally drained off; the little remaining, having been inadequate for the carrying on of the vital functions, which had been the cause of death. If then, he argued, there was not sufficient blood to sustain life, if the organs did not receive the quantity essentially necessary for their existence, how can you account for the profound sopor which ensued, sleep, according to your view, being caused by an afflux of blood to the brain? I confess that, at first, this startling argument somewhat staggered my belief in the theory which I had adopted, but on reflection I found that the very facts brought forward to weaken my views were actually the means of strengthening them. In Abercrombie's work on the brain, we find allusion made to the observations and experiments of the late Dr. Kellie of Leith. That much lamented gentleman bled animals to death, and almost invariably found, that while all the other organs were found completely blanched or drained of their blood, the

brain presented its natural appearance; nay, sometimes have the superficial veins been found so distended, that one writer has proposed the paradox, that animals which have been bled to death die of apoplexy. This, in my opinion, is an additional circumstance which warrants our belief in the doctrine of afflux. Let us suppose a case: here we have an individual in whom a violent inflammatory or any other disease, renders the taking away of a large quantity of blood from the system imperative; the system cannot safely meet the great demand made on it, the patient sinks into a state of sopor, and dies. The *post-mortem* examination shows us every part of the body bloodless, or nearly so; the heart, lungs, and great blood-vessels do not contain a material quantity of the fluid so absolutely necessary for the performance of the functions of life; the brain alone preserves its natural appearance; its veins and sinuses, however, are gorged with blood. Why this is the case in the brain, and not in any other organ of the body, is a subject which this is neither the time nor place to discuss, as any view which we, in the present state of our knowledge, could advance, must be purely hypothetical. Those, however, who wish to inform themselves on this point, will find this matter discussed in the work I have just mentioned, and also in the first volume of the *Transactions of the Medico-Chirurgical Society of Edinburgh*. I was in hopes to have brought the results of numerous experiments which I have made on some of the lower animals before you this evening, and to have proved to you that in sleep there is an *increased* quantity of blood in the brain; but owing to the many difficulties which must be surmounted before being able to do so, and at the same time to silence all important objections which might be raised against them, I have thought it better to institute a new set of experiments which now engage my attention, and with the results of which I trust soon to make you acquainted. I have now given you my opinion, such as it is, and hope that any gentleman disposed to criticise it, will deal mercifully; as it is *now* stated only as a hypothesis, and not as an absolute fact. Having, therefore, at present, nothing truly convincing to offer you in the shape of a theory, I now proceed to the definition, phenomena, and remote causes of sleep."

Dr. Ashenheim next describes the phenomena, varieties, and remote causes of sleep, dwelling particularly on cold and opium. He relates, among others, a case in which 102 grains of opium had been taken, and had produced stimulating effects. Protracted sleep is considered in the concluding section of this interesting paper; and two remarkable cases are quoted, in one of which a person slept for six weeks, and the other for five, without interruption.

ACADEMY OF MEDICINE.

Paris, May 4.

PREVENTION OF THE PLAGUE.

By M. AUBERT.

M. AUBERT commences his paper by asking how has the plague been driven from Europe? by

civilization and hygienic measures, or by quarantines and lazarettos? Such are the questions which the author proposes to solve. After having defined civilization to be the progress made in agriculture, science, and industry, M. Aubert proceeds to examine the circumstances under which the plague is developed in eastern countries. In Egypt, for example, what most strikes the observer is, in addition to the great filth, the total neglect of all measures of public health. Hence the plague rages with peculiar fury in that country, but its progress is in some sort regulated by certain circumstances, which vary at different times and in different places. Thus it has certain periods of invasion, of decline, and of disappearance; it affects certain localities only, and the mortality which it occasions is always increased in times of public distress. It raged in Alexandria in 1834; in Smyrna, in 1837; but as the individuals of the same nation are much better off in one of these cities than the other, it was easily seen how considerably the disease was modified by comfort, good food, &c. This is so much the case, that the plague disappears of itself in localities where the proper measures of hygiene can be adopted. It has often happened, in the hospital of Alexandria, that the plague attacked some one patient, in spite of the strict quarantine observed, and then disappeared, without affecting a single other patient.

The author next examines lazarettos and the sanitary laws. These commenced towards the end of the fifteenth century, a period of ignorance when the most absurd opinions found credit, and when even a common cough was thought to be contagious. If lazarettos were useful, the results obtained from them must prove this; but what does history teach us?

The first lazaretto was established at Venice in the year 1403; before that time there had been 11 epidemic attacks of plague in 365 years; but after it there were 16 attacks in 227 years. At Marseilles the same thing occurred.

Again, during 1,700 years of civilization, Egypt was free from plague, which began to reign about the year 263 A. D. Greece, also, furnishes a similar example. Italy was ravaged by the same pest in its seasons of barbarism; they had nearly disappeared under the effects of civilization when the irruption of the barbarous tribes, and the fall of the Roman empire, restored the disease until the time of the Medici.

In France lazarettos were established in the year 1467, but they did not prevent epidemic attacks of the plague until the reign of Henry IV.

The histories of England, Germany, and Spain, teach us the same lesson. In recapitulation, M. Aubert claims attention for these two facts, viz. in ancient times the plague was extinguished by the mere progress of civilization, without lazarettos; the disease continues still in certain countries, in spite of lazarettos, but is everywhere eradicated as civilization makes a certain progress.