I find in present case histories, and indeed in the literature, illustrations which are not wanting in quality, but their legends and descriptions sometimes are lacking in accuracy, apparently because the condition illustrated is ill-understood. Included in a large number of cases in a series, illustrations of conditions are recorded which are not of the series, and often it is this erroneously included illustration which is chosen for resemblance by other writers adding to the series. Very few of the multitudinous photographs possessed by those who have shown enthusiasm for medical photography are put to any general use. Good as they are and interesting to the particular observer, he is given little chance of using them, and, as photographs are made to show the features the interested observer wishes, only he may appreciate their value and what they were made to show. It is said that the photograph cannot lie, but those who understand their possibilities, even with the cinema, know that a very false impression can be created, though it was neither designed nor wished.

How many of the teaching cinema films indicate to the student the difficulties of their productions, quite apart from the photography? The film may appear to indicate a skill, dexterity, or result far different from the actual. The very simplicity of a procedure in skilled hands may entice the novice to try his hand. With such education and practice the novice in sculpture would not be allowed to maltreat a piece of marble.

Medical photography results in the accumulation of material which can be of little material value. Like the snapshots of the amateur they become so numerous as time goes on that they fill every available space, and only removal or death secures their destruction. While writing this letter I have received a hospital report which records under this heading: "It is interesting to make comparison with previous years:

1945	1,119 negatives		4,506 prints
1946	3,360		6,996
1947	4,958		10,652

What a magnificent increase! For what?—I am, etc.,

Edgbaston, Birmingham.

JAMES F. BRAILSFORD.

Health of Students

SIR,—Dr. Emily A. C. Wilson (April 10, p. 708) was puzzled about the conclusion in my article (March 13, p. 490) on "The Health of 407 New Students" with regard to previous dental treatment: "It seems probable that financial considerations had prevented some of the students from the poorer social groups from obtaining dental treatment."

It would not be correct to assume, as I think Dr. Wilson does, that students entering a university in October, 1946, had all had free dental inspection and treatment offered them in their secondary schools. In the report of the Chief Medical Officer of the Ministry of Education for the year 1939-45¹ it is stated that "under the Education Act of 1921, relatively little was done to provide dental treatment for secondary school pupils." The Education Act of 1944, which was not implemented until April, 1945, obliges local authorities to provide dental treatment facilities for secondary school children, but owing to the call-up of school dental officers during the war, which in some areas amounted to half the dental staff, there are still secondary schools which have not yet been visited.

Mr. Sidney Pappworth (April 3, p. 663) finds it difficult to believe that 16% of the Sheffield freshmen had postural defects of the spine, yet Parnell² found an incidence of 28% in a group of Oxford students. In asking for controls he perhaps misunderstood the purpose of my survey, which was to describe the health of a group of university students with a view to discovering the health needs of this group. No attempt was made to show that their state of health differed from that of non-students of a similar age, and therefore no control series was necessary.

Dr. F. J. Kelleher (March 27, p. 617) objects to my psychiatric terminology. The term "marked introversion" conveys, I believe, to the ordinary medical reader the type of person meant, and "nervous breakdown" referred to a recent history of this condition, as the text of the article indicates. It would have been wrong, I think, for a non-psychiatrist to have attempted to translate that expression into an "accepted psychiatric syndrome." The students were not subjected to the Rorschach or other psychiatric test. As your leading article (March 13, p. 504) states, special mental health examinations have been tried in one university in the United States of America, but were discontinued as it was found that "results failed to justify the additional organization, work, and cost." The physician in charge of a student health scheme certainly needs the assistance of a psychiatrist in the diagnosis and treatment of the more severe cases of mental ill-health, which, as you state,

are not rare. He also needs the help of an orthopaedic surgeon, oto-rhino-laryngologist, dermatologist, and other specialists for the treatment of established illness in students. In the University of Sheffield students' health service we have been fortunate in receiving much valued help of this kind from the consultant staffs of the Royal Sheffield Infirmary and Hospital.

Those who have not themselves had the opportunity of carrying out routine health examinations on unselected population groups are often surprised at the high incidence of defects and disorders reported in surveys of this sort. Morris,³ for example, examined 1,592 workers in a Midlands corporation and found that 7% had a major disorder, and that minor disorders such as "bad teeth, dyspepsia, hernia, chronic bronchitis, defects of hearing and vision, anaemia, varicose veins, and deformed feet were legion." Pearse and Crocker⁴ found that of the members of the Peckham Health Centre only 4% of the women and 14% of the men could be classed as free from disorder. Parnell's findings in examining a group of university students included 33% with myopia, 32% with skin lesions, 15% with ear, nose, and throat conditions, and 13% with minor anxieties. At routine health examinations illnesses such as hypochromic anaemia and pulmonary tuberculosis can be detected at an early and usually curable stage. At the same time deviations from the average, such as symptomless flat foot or transient systolic hypertension, are discovered, the significance of which is not yet fully understood. By following up individuals showing deviations of this sort it should be possible to determine which signs can be regarded as normal variations and which constitute the earliest evidence of disabling illness such as essential hypertension, peptic ulcer, and neurotic or psychotic breakdown. Possibilities in the prevention of the development of these illnesses might then emerge.—I am, etc.,

University of Sheffield.

JOHN PEMBERTON.

REFERENCES

- ¹ *The Health of the School Child*. London: H.M.S.O. 1947.
- ² *Lancet*, 1947, 2, 939.
- ³ *Ibid.*, 1941, 1, 51.
- ⁴ *The Peckham Experiment*. London. 1944.

Disk Lesions and Osteopathy

SIR,—Dr. James Cyriax (April 17, p. 754) has completely missed the point of my letter in reply to Dr. Craig and Mr. Lipmann-Kessel's letter. It was not my intention to draw an analogy between the osteopathic theory and protrusion of an intervertebral disk. Quite the contrary.

In order to make my point clear let me quote more extensively from Dr. Craig and Mr. Lipmann-Kessel's letter. They said, "It is generally accepted that on clinical evidence alone the level of a lumbar spine disk lesion cannot be confidently predicted. It may be that the present conception of sciatic pain as the result of root compression is responsible for this difficulty, and if it were regarded as a deep segmental pain due to joint disturbance this difficulty might be resolved. Over-simplified views of the pain mechanism in this condition are liable to follow a mechanical interpretation of the functions of the central nervous system."

While not denying that gross protrusion of an intervertebral disk accounts for a small proportion of cases of sciatica this broader conception of the probable underlying pathology not only of the majority of cases of sciatica but of many other conditions as well is exactly what has been suggested by the osteopaths for many years now. I repeat, in my opinion there is more truth in the osteopathic conception that has hitherto been admitted by the orthodox medical profession.

Many of Andrew Taylor Still's contentions have and will continue to be modified in the light of progressive knowledge, but this in no way detracts from his great contribution to medical progress. Certainly the modern conception of circulation differs considerably from that enunciated by Harvey, but he is nevertheless universally acclaimed the discoverer of the circulation of the blood.—I am, etc.,

London, W.1.

GEORGE MACDONALD.

Intra-abdominal and Intrauterine Pressures

SIR,—In his interesting dissertation on the mechanics of egg-laying (March 27, p. 585) Prof. H. A. Harris accepts the calculation by Houghton¹ of the maximum human intrauterine pressure during parturition. This reverend scientist was not, however, always near the mark with his calculations, as is shown by his figure for the maximum intra-abdominal pressure. From

measurements of the thickness of the muscles in the abdominal wall, the curvature of the linea alba, and the absolute muscle force, or maximum force per unit of cross section of the fibres, he arrived at the impressively exact figure of 32.926 lb. per sq. in. (1702 mm. Hg). The figure for the absolute muscle force which he used was 102.55 lb. per sq. in. (7.21 kg. sq. cm.), and this is nearly double my own figure,² which took into consideration many more factors.

Allowing for this discrepancy, however, Haughton's figure for the intra-abdominal pressure would still be many times greater than the systolic blood pressure. If this were actually the case the blood flow in the lower limbs would cease completely during periods of high abdominal tension. But a simple observation that the pulses at the ankles are still present during severe abdominal straining indicates that the intra-abdominal pressure must be less than the systolic pressure. This is confirmed by measurements which I made some years ago by introducing a balloon, connected to a manometer, through the opened sac of an inguinal hernia exposed under local anaesthesia, and then with a swab pressed firmly over the inguinal canal encouraging the patient to strain down with maximum effort. The best two readings in six cases were 75 and 80 mm. of Hg, and there was no doubt that these young men were trying hard. Barcroft and Millen³ have shown that the tension within a voluntary muscle at maximum effort can be high enough to stop its blood flow, but from the above readings it is apparent that the maximum intra-abdominal pressure is well below the normal systolic level.

It seems likely that this applies also to the maximum intra-uterine pressure, since a pressure greater than systolic levels would cut off the flow of maternal blood through the placenta and would arrest the foetal circulation. The figure of 3.4 lb. per sq. in. arrived at in Haughton's calculations is therefore likely to be well above the actual maximum pressure. His work illustrates well the fallacies which may result from inductive reasoning applied to body functions—the only way to get at the truth is to “try the experiment.”—I am, etc.

Manchester.

H. A. HAXTON.

REFERENCES

- ¹ Haughton, S. (1873). *Principles of Animal Mechanics*, London.
- ² Haxton, H. A. (1944). *J. Physiol.*, **103**, 267.
- ³ Barcroft, H., and Millen, J. L. E. (1939). *Ibid.*, **97**, 17.

Prompt Publication

SIR,—I have always had the greatest admiration for the department of the B.M.A. which is responsible for the publication of the specialist quarterly journals, but recently this department has quite excelled itself. In the issue of the *Journal of Neurology, Neurosurgery, and Psychiatry* dated November, 1947, there appears a long and valuable article which was received at the editorial office on Nov. 29, 1947. Such an achievement demands and must receive the warmest congratulations. As the journal cannot be expected merely to rest on its laurels in the future and must like all of us “raise its sights,” I suppose that we can confidently look forward to the time when it will be publishing articles some months before they are written.—I am, etc.,

London W.1.

SAMSON WRIGHT.

The Problem of Caries

SIR,—Your annotation (April 10, p. 697) on this subject requires some clarification and correction. Does the writer maintain that caries does not, or that it need not, involve decalcification, or merely that it is not purely and simply decalcification? Or does he mean that proteolysis may or does precede decalcification either in enamel or dentine? What conception is it that recent work has shown to be untrue, and what decalcification hypothesis is it that Dr. P. Pincus has “shattered” with his earlier work?

The chemico-parasitic theory was not originally deduced from a “belief,” but from observation of now well-known facts, and it has since been amply confirmed by nearly all the work that has been done on the aetiology and prevention of caries. Frisbie's theory, which you refer to, depends on his observation¹ that some alteration of enamel occurs while its natural surface, and the surface of ground sections, is still “essentially hard in the physical sense. Examination by means of instru-

ments may or may not reveal a roughened or irregular surface.” This vaguely described observation seems to be his only reason for supposing that no decalcification had occurred. However, Applebaum² has shown that carious enamel becomes radio-lucent to Grenz rays before its surface continuity is lost and while it is still hard enough for the preparation of ground sections. This clearly indicates that decalcification has begun at that very early stage.

The earlier paper of Pincus,³ which is cited, does not specifically describe any destruction of the hard part of the enamel but merely some alteration in the surface of teeth which seem from the illustrations to be still covered with Nasmyth's membrane or the remains of the enamel organ. It is not stated how long the teeth had been erupted or whether they had erupted at all. No sections are described or illustrated, and the author only claims that the appearance suggests that “some of the enamel was disintegrated . . .” Few would agree with him, unless by “some of the enamel” he means only the organic matter lying on or at the surface of newly erupted or unerupted enamel. He makes no claim to have shattered any hypothesis, and the real value of his work as dental research is, I suggest, that after years of painstaking effort he has *not* been able to produce anything resembling caries in the absence of acid or acidogenic bacteria. Artificial or *in vitro* caries with acid-forming bacteria has of course been known for many years.

Perhaps the writer of the annotation was misled by the statement of Dr. Pincus, in his discussion of caries, that dentine is 23% organic matter. Enamel, where caries usually begins, has a much lower content—usually reckoned as about 2%. It has long been realized that proteolysis is an essential part of dentine caries, but there is still nothing to suggest that it is essential or important in the caries of enamel.

A glance at any number of the *Journal of Dental Research* will show that the theories of Pincus and Frisbie are not as your annotator suggests widely accepted by research workers in the U.S.A.; and his own views go even beyond the unorthodox theories of those two workers. The last paragraph of the annotation is also very puzzling. Why does caries occur at stagnation areas if stagnation areas are caused by caries? Surely it would be better if comment in the *Journal* on Pincus's theory was made from a more orthodox or impartial viewpoint, or at least that such extreme and obscure theories should not be presented as though they were generally accepted?—I am, etc.,

London, N.W.6.

R. B. D. STOCKER.

REFERENCES

- ¹ *J. dent. Res.*, 1947, **26**, 181.
- ² *Dental Cosmos*, 1935, **77**, 931.
- ³ *Brit. dent. J.*, 1937, **63**, 511.

SIR,—I read with interest and some surprise in the annotation (April 10, p. 697) on this subject that “many dental research workers still refer to dental caries as a process of decalcification”; that “recent work . . . indicates that this conception is far from the truth”; and that “it is on such lines that much of the research work on caries is now being done in the U.S.A.”

I have just returned from a five-and-a-half-months tour of research centres in New York, Ann Arbor, Mich., Rochester, N.Y., Minneapolis, Indianapolis, and others, for the express purpose of discovering the views of the research workers upon dental caries and its prevention. My impression is that the conclusions of the overwhelming majority of these men are diametrically opposed to the views expressed in your annotation, and that notwithstanding much research upon proteolysis.

I quote the findings of that large body of research workers known as the “Michigan Workshop,” whose 25th meeting was held from Sept. 8-13, 1947, as published in the *Journal of the American Dental Association* (1948, **36**, 4): “Dental caries is a disease of the calcified tissues of the teeth. It is caused by acid resulting from the action of micro-organisms on carbohydrates, is characterized by a decalcification of the inorganic portions, and is accompanied or followed by a disintegration of the organic substance of the tooth.”

Fosdick, Professor of Chemistry at the North Western University, states in the *New York Journal of Dentistry* (1947, **17**, 55), “Recent research in dental caries has primarily emphasized the early position of Miller and Black, in that the condition is an acid decalcification of the inorganic structure of the teeth accompanied or followed by a putrefaction of the organic portions.”

Volker, in the *Journal of the New Jersey State Dental Society* (September, 1947), says, “In conclusion it may be reiterated that our present knowledge of dental caries supports the view that we are