the attacks are sometimes due to indigestion, emotion, or cold, and all that we can say is that in the absence of cardiac and vascular disease the prognosis is good. The hysterical type or pseudo-angina is not difficult to diagnose as the mental condition dominates the picture. On one occasion I saw three women suffering from angina at the same time—the mother died in the attack and one daughter had typical spasmodic angina, while the other daughter suffered from hysterical or pseudo-angina. The syncopal type, described by Nothnagel as vasomotor angina, in my experience has been marked by coldness and numbness of the limbs. The pain is as intense as that of angina of effort, but the cases are usually in women at the menopause.

It would be unfortunate if the term "angina innocens" were used without a further separation according to the clinical picture.—I am, etc.,

Beckenham, June 4. A. E. BLACKBURN.

Animal Pathology

SIR,—In the annotation at page 1165 of the *Journal* of June 5 it is stated: "We may reasonably ask ourselves whether disease in animals is being adequately studied in this country. It might have been supposed that the common diseases of our principal fauna were well known, but we may now wonder whether there are not other species, especially among wild animals and perhaps birds, whose entire pathology is an unexplored field."

I was privileged to share with the late Dr. J. G. Adami a deep interest in wild birds. Adami told me how this and his work with Metchnikoff had quickened his desire to compare human disease with disease in our domesticated and wild fauna, a desire he had found little opportunity of satisfying. He shared with the late Sir Clifford Allbutt the conviction that more knowledge of comparative pathology was required for our understanding of disease in man, but he feared that for such pioneer work there would be no security and little or no prospects. Encouraged by Adami I embarked on comparative pathology, and I have acquired sufficient experience unhesitatingly to assert: (1) that there is a considerable corpus of knowledge of disease in our domesticated horses, cattle, pigs, sheep, goats, dogs, cats, and poultry; (2) that a large amount is known of disease occurring naturally among laboratory animals; (3) that during the past ten years several workers have contributed much to our knowledge of disease among the animals farmed in Great Britain for fur (for example, silver fox, mink, marten, fitch, rabbits, and nutria); and (4) that to those familiar with the subject there is a not insignificant amount of data on disease in the wild fauna of this country. There are species of our wild fauna of whose pathology little appears to be known—for example, voles, shrews, dormice, field-mice. Charles Elton and his colleagues are to be congratulated on their pioneer work in the ecology of these small rodents. My own experience of disease in our *wild* fauna has been derived mainly from birds; however, I am not unfamiliar with some of the pathology of squirrels, fox, rabbit, hare, blue hare, and stoat; while occasional specimens of wild cat, hedgehog, and badger have been received.

I have had the temerity to mention my work not because I see clearly what to make of it all, but because it has brought me into close touch with other workers on the wild fauna. They, like myself, do not earn their living by such pursuits; this may explain why those unfamiliar with the subject regard it as "an unexplored field." If the writer of the annotation should reply that the work done and proceeding is "inadequate" to the needs of pathology I should agree with him. Some day, perhaps, those in authority will recognize that there is something after all in Adami's notions, and in the work of John Hunter, Tegetmeier, Bland-Sutton, Wilson, Frank Colyer, Pillers, Henry Gray, and many others. Some day, perhaps, finance will be made available to establish an institute of comparative pathology where disease in our domesticated and wild fauna can be studied for the service of the science of medicine.—I am, etc.,

Veterinary Research Laboratories, London, N.W.3, June 7. TOM HARE.

Intermittent Venous Occlusion

SIR,—I am much interested in Brown and Arnott's account of treatment by intermittent venous occlusion (*Journal*, May 29, p. 1106), in which the earliest date they mention is 1925. As I was treated by this method in Germany in 1917 when a prisoner of war, and during my subsequent medical course, 1919–23, I never heard any mention of this method, can any of your readers tell me if it was used in the military hospitals on our side of the line?

I was wounded and taken prisoner near Cambrai on November 30, 1917, and in three or four days arrived at Le Cateau. On December 17 my ankle was *x*-rayed, and on the 18th I was removed to a ward in another empty factory. It was the best ward in the town; there were sixty beds, four day nurses, and two night nurses, under the care of Professor "Blue-Nose" (I know no other name), who operated on my ankle forthwith and removed a metal fragment. The theatre was primitive and the anaesthetist unqualified.

Five or six hours later a nurse fitted an intermittent venous occlusion ring on my leg above the knee. She told me it would remain on for seventy-two hours. All the fifteen beds in my row were similarly fitted. A gaspipe ran along the wall at the head of the beds, and a T-piece with a tap came off at each one. The apparatus working this was in the middle of the row, and so far as I can remember it consisted of a couple of oxygen cylinders and a larger cylinder, measuring four or five feet by two feet in diameter, with a pressure gauge and a clock. I thought it worked by the boiling of liquid oxygen. From each T-piece came a thick rubber tube attached to a thin rubber ring, 1½ in. wide, enclosed in a stout canvas case. The period was sixty seconds with fifteen seconds complete relaxation and fifteen seconds maximum pressure.

All wounds were septic in every ward I was in owing, doubtless, to the lack of staff and materials. This apparatus was applied to all the patients who were with me, all picked cases—I was there because I was an officer and the only prisoner who spoke German—and I understood it was to help the patient to overcome sepsis. I spoke of this apparatus during my subsequent hospital training to various members of the staff, but no one seemed to have heard of it.—I am, etc.,

G. D. SUMMERS, M.R.C.S., L.R.C.P.,
Late Lieutenant 7th Battalion The Norfolk Regiment, B.E.F., France.

Lincoln, May 29.

Orthopaedic Conditions

SIR,—I feel that a reply to your unfavourable review of my book on *Elements of Orthopaedic Surgery* is justifiable. It seems that your reviewer has read it from the point of view of the specialist, for whom the book was not, of course, intended, and has little appreciation of the real needs of the auxiliary workers in orthopaedics,