at all resembling those of syphilis. Have these patients been subject to mercurial rheumatism? I conceive that irritability, phthisis, and diseases of the heart, can have no reference to the question, as those may arise from many causes—the venereal disease from *only one*. I can assure Mr E. that the case of rheumatism produced in my mind more surprise than alarm; nor does it in the slightest degree tend to convince me that his position is a correct one. He believes, I presume, that the mercury cured the rheumatism, and produced the caries; but he does not state whether the woman had syphilis at any former period; and unless the full particulars are given, the case can be of no value whatever.

My second question Mr E. treats in a very extraordinary manner! He says, "that if INVESTIGATOR will turn to my papers, he will find that *my conclusion* was, that there is no legitimate ground for believing that the venereal poison is ever absorbed into the body." I was anxious, Sir, to have something more than conclusions, and thought that facts were necessary to warrant such an inference; and as I saw no such facts in Mr E.'s papers, I was induced to ask him for his proof. He, however, wishes for mine first, and I give him the following:—A man has chancre, which is healed without mercury: in the course of a few weeks he has ulcerated sore throat, copper-coloured eruptions, &c. (I do not mention bubo, because this proves nothing; indeed, I will admit, for the sake of argument, that it is sympathetic),—how, I again repeat, can all this happen, if the poison be not absorbed into the body?

In Mr. E.'s first paper, he stated "that three persons having connection with the same female might have chancre, gonnorrhœa, or bubo." (If there be no breach of surface, how can the latter be produced without absorption?) I pointed out to him the fallacy of the usual proof given by those who maintain that the two diseases are produced by the same poison, and wished him to give me some additional grounds for this belief, and he refers me to the cases related at the commencement of his letter, as proofs of the correctness of his opinion. I believe, Sir, that these cases prove nothing of the kind; and I must remind Mr. E. that the statements of persons labouring under these diseases must be received with great caution; for, according to my experience (and I think the generality of practitioners will agree with me), very little reliance can be placed upon their assertions. The cases appear to me to prove only that which every medical man knows, viz. that an acrimonious discharge from the vagina of a female will sometimes produce excoriations on the glans and prepuce, and also discharge from the urethra; but I contend that these are not venereal, and that a woman who has leucorrhœa *only*, cannot communicate the venereal disease in any form. If it were otherwis I fear that but few married men (especial among the poorer classes, in large town where cleanliness is little attended to) wou escape the disease; for it is calculated b some recent writer "that nearly one-half the married women in England are affecte with leucorrhœa," and I think that few them are exempt from it at some period their lives.

In pursuing the above discussion, my ol ject has been only to elicit truth, and I a quite sure that Mr. Eagle's purpose is th same. I trust, however, that he will on thi occasion have something more than a shado to contend with. I am, Sir, your obedien Servant, INVESTIGATOR.

CAUSE OF APOPLEXY.

To the Editor of THE LANCET.

SIR:—Will you permit me to ask, through the medium of your excellent periodical whether any such explanation as the following has ever been advanced, regarding the well-known fact of "short-necked people being the most subject to apoplexy."

I would suggest that "the impetus of the circulating fluid is in the direct ratio of its proximity to the hollow muscular viscus which propels it in the first instance, and compared with which the muscular action of the arteries themselves must be insignificant." And I may here say, that I would on this reasoning explain the greater frequency of aneurysms, and especially dilitations observed in the arch of the aorta, the thoracic aorta, and its brachio-cephalic branches, compared with the remote parts of the arterial system of equal calibre, and, consequently, equal *arterial* muscularity. On this idea, that the nearer to the heart the greater is the force of the blood, we can explain how the more violent vis-a-tergo communicated to the medullary substance by a *short* than a *long* carotid artery, may account for its branches more frequently giving way in the one case than in the other. This opinion may be strengthened by the fact of apoplexy in those "physically predisposed" persons being generally sanguineous; and probably in many cases the rupture of the artery is not instantaneous, but preceded by an aneurysm. A beautiful specimen of aneurysm of, I think, the anterior cerebral artery, that thus terminated fatally, and which occurred in a young Quakeress, is mentioned by that talented and eminent pathologist, Dr. Mackintosh of Edinburgh, one of whose class I had the honour to be this last winter.

With every apology for the length of this, which I only trouble you with, knowing the accessibility to your pages of any idea connected with our profession, I remain, your constant reader,

GEORGE ALEXANDER COWPER, M.R.C.S.