course of pre-operative *x*-ray radiation before advising a patient to postpone operation for operable cancer a single day. On the other hand, I have for many years past advised my patients to undergo a course of post-operative *x*-ray radiation. There are these clinical facts about cancer: In some cases where the local origin is quite insignificant there is a great tendency to rapid and wide dissemination, and that quite apart from any operative procedure whatever. In other cases where the local origin is extensive the disease seems to expend itself locally, and the tendency to dissemination is much less. That is, so far as I know, one of the mysteries of this mysterious disease. That is why it is always worth while to remove cancer, however extensive, provided there is a reasonable chance of getting the palpable disease away. The most apparently hopeless case of carcinoma of the breast, from the point of view of prognosis, that I have ever operated upon—a young woman with a large and rapidly growing medullary carcinoma filling the breast, and secondary glands as large as walnuts in both the axillary and supraclavicular regions, advised, and apparently justifiably advised, against operation by a very eminent London surgeon—sent to me by Dr. H. W. Jeans of Portsmouth, is alive and well now, ten years after operation. In this case, wishing to give her every loophole possible in an apparently forlorn hope, I removed both her ovaries as well. Whether this had anything to do with the result of the operation I do not know. But the next case in which I tried it had a very rapid recurrence. The vagaries and uncertainties of this mysterious disease are untold, and conclusions and generalizations are apt to be consequently fallacious.—I am, etc.,

CHARLES P. CHILDE, B.A., F.R.C.S.

Portsmouth, March 14th.

CHRONIC RETENTION OF URINE ARISING FROM PROSTATIC DISORDER.

SIR,—I doubt whether Mr. Herbert T. Herring has told quite the whole story in his interesting paper (March 12th, p. 376). How does he account for the increased frequency of micturition which is a marked symptom of enlarged prostate long before there is anything like 10 oz. of residual urine? I believe the symptom is common before there is any residual urine at all. A majority of men over 60 pass water two or three times each night, which means that they can only hold some 5 or 6 oz. with comfort. It is not overflow from a distended bladder, for a catheter shows little if any residual urine. He says "prostatic resistance is generally augmented by enlargement." Why, then, does the subject of commencing enlargement pass water more frequently? He ought to have to wait until the "tonal bladder pressure" is sufficient to overcome the increased prostatic resistance.

One practical question for the practitioner is, When ought he to insist on passing a catheter? Should he insist if the urine is normal but passed only 3 or 4 oz. at a time, or should he wait until there are some signs of retention? The passage of a catheter is not devoid of danger.

Mr. Herring's treatment seems to me a bit risky. It might be done in hospital, but it would be dangerous in private practice, and a terrible ordeal for both patient and medical attendant. If the catheter is left to the patient "a breakdown in the technique adopted with regard to the preparation and passing of the catheter" is almost certain. The timing of the catheter would be difficult, for the secretion of urine is extremely irregular. Some nights a patient will secrete 20 oz., and on others as little as 4 oz. Most patients would prefer an operation, even a dangerous and tedious one.

If the practitioner succeeds in so increasing the muscular strength of the bladder as to overcome the increased prostatic resistance, surely he is dangerously near the production of back-pressure with fatal results to the kidneys?—I am, etc.,

O. CLAYTON-JONES, M.B.Oxon.

Silverton, nr. Exeter, March 14th.

THE DROOPING SHOULDER SIGN OF PHTHISIS.

SIR,—Let me reply briefly to the little discussion of the paper entitled as above. The first participator says I mention Krönig, but not his "isthmus of resonance." Hardly necessary, perhaps, for that is by way of being a classic sign, and every recent textbook, British or other, has it.

The second gives a British book in which, he states, the drooping shoulder phenomenon is fully described. I fear I cannot agree. There is no mention of *unilateral* drooping. What is described is sinking of both shoulders due to the long familiar fact that the *thorax paralyticus* has a position of expiration. Nothing is said of one scapula lying lower than its fellow, nothing of this asymmetry being probably due to the atrophic trapezius allowing it to sag; nothing of the probability of such atrophy being an end-stage of Pottenger's muscular spasm and subsequent pulpy degeneration; nothing of the smaller nipple on the affected side confirming the theory of nervous origin of the atrophy by pointing to a possible trophic influence. As there are four lines on lowering of the nipple I do not think the author can have read Krönig's untranslated little masterpiece in the *Deutsche Klinik*, or his ready mind would have appreciated the precedence given there to the drooping scapula over the lowered nipple. In this exegesis I am confirmed by his own entry into the discussion. He talks only of lowering of the external end of the clavicle, which he now associates with advanced or extensive disease, citing Walshe to the same effect. Thus it might now possibly be unilateral, although we are left in the dark as to that; but there is nothing even implicit to be gathered as to a connexion with muscular atrophy. In truth, changes in thoracic conformation from advanced lung disease are another thing altogether: witness the frequency of shoulder drooping with clinically early lesions or with the hilus affection of children. Since the bibliography of his book has not a single reference to a German or Austrian paper in the original, while, on the other hand, nearly a third of the articles in it are by Scotsmen, one of them mentioned repeatedly, my withers are not sore under his strictures as to authors and nationalities proper to quote. For my part, I cited the chief relevant writers—Krönig, whom we have heard about; Bandelier and Roepke, the English translation of whose book is on every tuberculosis officer's shelf; Schröder and Blumenfeld, authors of the fullest work on phthisio-therapeutics in existence; and the American Pottenger, whom my third critic also cites. To *approfondir* a subject one must forget geography, and that mendacious goddess Politics.

The fourth one, I fear, with every wish for an amicable summing up, I must characterize as another confuser of *thorax paralyticus* with a unilateral phenomenon; as is surely plainly apparent. Even if he were not, two words are a smallish basis for a priority claim. We are not all as easily satisfied as M. Jourdain was when they told him "Bel Men" meant such a lot of things.

The "drooping shoulder" seems always to excite interest. Readers might therefore care to turn up *Tubercle* for July, 1920, where I have traced the analogy with muscular spasm and atrophy in osseo-articular tubercle, and in some cases of scrofula.—I am, etc.,

Barnsley, March 11th. W. C. RIVERS.

THE CURE OF HAEMORRHOIDS WITHOUT OPERATION.

SIR,—I fear I must ask you to allow me to trespass upon your space again in order to reply to Dr. Lyth's letter in the JOURNAL of March 12th. I do not know whether to take his opening remark, in which he accuses me of being a physician and not a surgeon, as a compliment or the reverse, but it is typical of the inaccuracy of observation which he shows all through his letter.

Dr. Lyth admits that my views as to the essential differences between external and internal haemorrhoids are assumed by most authors. But he goes on to quote from Walsham and Spencer's book on *Surgery* to prove that "most authors" are incorrect. This little students' book is hardly one to which to refer in a scientific discussion, and Dr. Lyth is forced to make a very "tall" assumption to support his theory that an internal pile may be converted into an external one. The quotation to which he refers is merely to the effect that some piles are "covered in part with skin and in part with mucous membrane." I cannot see why Dr. Lyth should assume from this that the authors meant to imply that internal piles may ever become completely covered with skin and shift their base from above the anal valves to