for both sugar and albumen in every specimen of urine. This double qualitative testing, which occupies less than two minutes, is done as follows. To about a drachm of urine in a test-tube is added an equal volume of a saturated solution of picric acid. If albumen be present, the liquid is rendered more or less turbid, and this turbidity is increased by boiling. Then half a drachm of liquor potassæ is added to the boiling mixture, the albumen, if present, is redissolved, and the boiling is continued for about thirty seconds. If sugar be present in the proportion of a grain to the ounce, the liquid will become darker than the quarter-grain standard, and two grains or more to the ounce will make the mixture black. The presence of an excess of sugar having thus been ascertained, the actual amount may be speedily and accurately determined by the quantitative method of analysis before described.

AN ADDRESS ON COLLECTIVE INVESTIGATION OF DISEASE.

*Delivered at a Meeting of the Northern District of the Metropolitan
Counties Branch.*

By DYCE DUCKWORTH, M.D.,
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GENTLEMEN,—Addresses on the subject of Collective Investigation of Disease have become a part of the machinery employed by the Committee of the Association which I represent here to-night. Most of those who now hear me have, I suppose, read the masterly addresses delivered this year by Sir William Gull, Sir James Paget, Dr. Wilks, and others, and may even have been stirred by their voices. I assume that you all know the design of our Committee, and that I have nothing to tell you in respect of that, or of the method in which it is sought to garner the fruit of the experience of many capable and hard-working members of the Association. I can, therefore, only ask you to take my remarks as a text for the discussion which your district Branch has, as I think wisely, determined to hold on this subject to-night. I have been told that there are members of the Association not in sympathy with this great movement. I cannot believe that they are numerous. It is conceivable that objection may be taken to parts of the scheme, and, as a matter of fact, I know that objections have reached the Committee in respect of this. I may add, at once, that such objections are in all cases carefully considered. It were too much to expect that a vast piece of machinery, such as we have set going, should work smoothly all at once. And that it is truly vast, cannot be gainsaid, for I hold it to be very remarkable that so many inquiries should have been issued, and responded to, within the last eighteen months, and that so much addition to general medical knowledge should have been furnished by this Committee as was embodied in the first volume of its *Record* in July last. So far, the work is certainly encouraging, and there is clear prospect of greater things to come. For my part, I think the Association never before so vindicated its existence as when it determined to employ its unrivalled resources for this good purpose.

I take it that, as a profession, we are bound not only to do all we can for the sick committed to our care, to seek constantly for truth and for increase of knowledge that shall help us, but also to make known our experience for the common welfare of humanity.

Now, gentlemen, it cannot be denied that, day by day, the grave closes over members of our body, who are taken to their well-earned rest, who leave us not only the poorer for their personal work, but who carry with them stores of knowledge and experience, honestly and hardly gained, which never went further than to do good service to the sick they tended.

Of course, much of this is inevitable, much of the art of medicine (and, gentlemen, it is well to emphasise it in these days, our profession is an art, and we are artists, if we are worth anything, as ministers to the sick), I say, much of the art of medicine is personal to the possessor of it, and is, in the nature of things, incommuni-

able by word or letter. Albeit, there is much, very much, that may be unburdened from the well-stored mind, and that ought so to be passed on to the stock of common knowledge. This is our special work. Systematically, categorically, and with exactness, to gather in the fruits of careful and observant practitioners; to do this in full recognition of the many calls and claims upon the time and energies of men who are mostly busy, mostly pressed as bread-winners, and without, in many cases, the unquestionable stimulus that urges on some to seek position and fame as just recompense for their efforts.

This has been nobly achieved, in many instances, long before this Committee was formed. Some of the hardest-worked men have done much in this direction, and it would be easy to adduce examples.

Those who have preceded me in offering addresses of this kind, have indicated well how private practitioners must naturally bear the largest share in building up the particular knowledge we seek. You have the special privilege of retaining your patients, for the most part, under long-continued observation, both in health and disease; and pray remember that, much of the best knowledge needed in treating a case of disease, is derived from accurate acquaintance with the healthy or ordinary life-history of the patient. The family-practitioner is at a great advantage here, as compared with a consultant, or a hospital-physician, who is often heavily handicapped in this respect.

The great doctrines of diathesis and heredity, which are happily coming back to prominence, enlarged and rendered more certain and trustworthy by the light of advancing knowledge, are available as material guides for better therapeutics; so that we now, more than formerly, treat our patients rather than their diseases. With family- and life-histories before you, you have great aids to diagnosis and prognosis.

As you are aware, several inquiries are now in progress. The question of the contagiousness of pulmonary phthisis is not yet fully answered. This is, perhaps, the largest and most difficult problem issued by our Committee, and, perhaps, none has excited more wide-spread interest, and, at the same time, more obloquy.

It is all very well to scoff, as some have done, at the idea of contagion in connection with phthisis, but the question still waits for an answer; and facts alone, if they can be had, and not opinions, will furnish the reply. I may say here that the residuum of evidence adduced so far by the inquiry, goes to prove that any fears on this head, fears which, if they exist, are either justifiable or unwarrantable, are all but vain. We have learned that only under conditions of extreme contiguity and of most vicious hygiene, can this malady prove communicable. It was surely worth the effort to learn this; we had no knowledge at all on the subject previously, and, without doubt, ere the inquiry is closed, we shall be wiser still.

The endeavour to gain more exact particulars about the workings and manifestations of syphilitic virus is one set on foot, and such an inquiry at once commends itself, and cannot fail to be helpful to us. The same may be surely said of the inquiries as to diphtheria, chorea, acute rheumatism, pneumonia, and acute gout; and I may remind you that the subcommittee on pneumonia desires soon to issue its report, and therefore begs for a return of cases as soon as possible.

Objection has been raised to the number of inquiries set going almost simultaneously, and I think there is some reason in this, though I cannot but suspect that the objections come, in some instances, from Gallios amongst us, who consign all the documents of the Committee, without qualms of conscience, to their waste paper-baskets. It is, I believe, the experience of our subcommittees and secretaries, that the men from whom a report is expected on one subject, are equally to be depended on for efforts in respect of all; and this only confirms what is well-known, that the men who have least time to lose, contrive to do more work and have more true leisure than less active men. As I am seizing as many points upon which objection has been raised as I can, I may here refer to those which have reached me in respect of the card issued upon acute gout. As an instigator of that inquiry, and having taken a share in drawing up the questions upon it, I was surprised to hear that some of these were thought incapable of reply. In respect of the temperature-curve of acute gout, it seemed not a hard matter for a practitioner to secure this. It has been said that no one attends a case of acute gout more than once a day, and that, therefore, no two temperatures can be secured in twenty-four hours. Surely, there can be no insuperable difficulty here. The public are rapidly learning to use the clinical thermometer—too rapidly, as some may think—and a duly set instrument may safely be left with

a patient for his use. Patients with acute gout, in every rank of life, are commonly possessed of more than the average intellect, and can therefore afford particular aid in this and kindred investigations. It is certain that, in hospital-practice, we cannot pursue this inquiry, for cases either do not present themselves early enough, or are not deemed fit objects for a bed. Meantime, we remain in ignorance of the particular type of gouty pyrexia, an ignorance that could be removed within one month if fitting use were made of opportunities.

Another objection has been made to our definition of the disease. To avoid endless errors, which are only too easy to fall into, the subcommittee agreed to limit the gout-inquiry to cases in which a great toe was alone, or at all events primarily, involved. This is classical podagra. It will hardly be believed that one member of the Association made objection to this, because he had happened to see more cases of acute gout in the knee than in the great toe.

Other inquiries are about to be issued. The subject of puerperal pyrexia is a pressing one, and we want better knowledge of this important matter. A card has been prepared on cholera; and if, as is not unlikely, this pest invades us next autumn, it will be well to have our inquiries systematised, and available in good time.

Complaint is sometimes made that more time would be required for response to these inquiries than can be fairly demanded from busy men. We well know how ways can be found for willing minds, and I might here adduce as a monument of industry, erected, almost imperceptibly, at odd moments in a busy life, that valuable work of Dr. Neale's, the *Medical Digest*, to which I am ready to acknowledge my frequent indebtedness.

The art of collective investigation can be best cultivated by acquiring a habit of recording, even in mere jottings, the points and prominent features of cases. A note of one line in length at the time is worth a page written from memory a few days afterwards. The value of committal to paper, too, is very great as an incentive to accurate, and corrective of superficial, observation. And all this demands an interest in our work, and calls upon us to show how much we really love our art. To judge by some men's conduct, it would seem that they did not love their daily work for its own sake, and I will illustrate what I mean by some personal experience.

To take a class of diseases about which we want information, and which we too often fail to reach in hospital practice;—I allude to abdominal tumours. Perhaps there is no more difficult subject of diagnosis than this. Years of experience (and, as my great predecessor at St. Bartholomew's, Latham, remarked once, "great experience is not all experience") are required to render one's opinion valuable in this direction. What do we find but too frequently? That, as soon as we have paid careful attention to a case of this kind, and shared our difficulties with our colleagues, the patient becomes alarmed at the concern his malady creates, and forthwith quits the hospital. In too many instances we never hear more of the case. It will certainly fall into the hands of some practitioner; but the occasion is rare when it excites sufficient interest, or becomes the subject of a necropsy; and still more rare, if this should be made, that the hospital-physician ever hears the result. And thus great *lacunæ* in our knowledge come to exist, for want of timely interest and enterprise. It would be but kind, and it would certainly be wise, for the practitioner to communicate the fact of his having such a case to those who preceded him in charge of it; and in many instances the hospital would provide for an inspection, and thus secure a fragment of knowledge for common benefit. All this entails some trouble; but I hold it to be a duty, and no work is worth doing that gives no trouble. Indeed, nothing in the way of our duty can possibly be a trouble, properly so called.

Gentlemen, I have expanded the remarks I purposed to make, and have taken up an undue amount of your time. I desired to give you a text for your discussion, and have avoided touching any topic which my predecessors have exhausted. Let me conclude by commending the efforts of the Committee to your best consideration and support. If you possess the first *Record* of its work, you will not fail to procure the second, just about to be issued; and, if you do not find your own share of contribution in either of these volumes, I am confident you will not allow the third one to appear without some token of your sympathy with this good work. My last words shall be those of one of the greatest and most painstaking physicians of our time, and they are very pertinent to the subject of collective investigation: "In the medical profession, all truly valuable and practical knowledge is only to be gained by appropriate sacrifice of time and labour; and, as a general rule, the one may very fairly be measured by the other." Thus wrote Thomas Addison. For those who are faint-hearted, or lack confidence in

their powers of observation, I would commend a daily reading of this inspiring epigram, which has before now served good purpose: "Maximum mediocris ingenii adjumentum diligentia."

RODENT ULCER, AND ITS RELATIONS TO EPITHELIOMA.

By G. H. HUME, M.D.

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THERE seems to be now almost an agreement among pathologists on the subject of rodent ulcer and its relation to epithelioma. But the question is not yet entirely settled, and the investigation of which this paper is a record was undertaken with the desire to aid in definitely determining the pathology of the former disease.

It is not necessary to enter at any length upon the history of this question. But it is convenient to have in mind the leading facts, and it is found that these group themselves into two periods. In the first period, in which rodent ulcer was first clinically distinguished by Jacob, no special cell-growth was detected as characterising the disease, and it was therefore classed among the non-cancerous ulcerations. The second period dates from the description by Thiersch in his *Atlas* of the existence of a special cell-growth. Since the publication of Thiersch's *Atlas*, there has been practically little added to the description he gives of the masses in which this cell-growth occurs. Later observers may have differed as to more minute details, but the existence of peculiar glandiform cell-masses has been accepted as the distinctive pathological feature of rodent ulcer, just as Jacob's description has remained the clinical picture of the disease. The point on which opinions have mainly differed has been as to the genesis of these cell-masses. Thiersch considered them to be of epithelial origin, and he therefore distinguished the cases in which they occur as flat epithelial cancer. He does not seem to have directly traced the cell-masses to any one of the skin-structures, but, from their general form and character, he was of opinion that they originated in the hair-follicles. This view, both as to the epithelial character of the masses and as to their origin, has been generally adopted, and among recent writers has been especially advocated by Drs. Tilbury and Colcott Fox (*Pathological Society's Transactions*, vol. xxx), who traced the growth to the external root-sheath of the follicles. This sheath being an extension of the Malpighian layer of the epidermis, from which the growth in epithelioma is known to spring, this view necessarily reduces the difference between the two diseases to a minimum. In the one disease, the new growth takes the form of prolongation from the rete Malpighii; in the other, the portion of the Malpighian layer attacked is that belonging to the follicles. But in opposition to this view, Dr. Thin has maintained the belief that rodent ulcer is in reality a cancerous adenoma of the sweat-glands (*Pathological Society's Transactions*, vol. xxx). He traces the origin of the cell-growth to the secreting epithelium of these glands, and not to any epidermic structure, and thus assigns to the disease a pathological connection altogether distinct from the epithelial cancers. Between these two views, there can be no doubt that the balance of evidence is decidedly in favour of the former; but on neither side has the endeavour to trace the actual development of the new growth from its origin been altogether satisfactory.

During the past five years, I have had opportunity of examining specimens from eight cases of rodent ulcer. Five of these cases were under my own care; the remaining three were under the care of my colleagues in the Newcastle Infirmary, and I was enabled in these instances also to see the patients previously to treatment. To the earlier of these eight cases, I do not here make more than a passing reference. Their examination familiarised me with the ordinary histology of rodent ulcer, but the portions of skin obtained were too completely involved in the disease to admit of other than a conjectural opinion as to its origin. I therefore determined, as opportunity occurred, to obtain portions of skin as far removed as possible from the edge of the ulcer. It seemed a reasonable assumption that the mode in which the disease is propagated into the surrounding healthy skin (apart from the direct growth of the glandiform masses) is a repetition of the process by which it at first started; and that therefore a methodical examination from the healthy skin towards the ulcer would yield important evidence as to the structure in which the morbid action begins. So far as could be judged from the few published cases, the development of the disease did not seem to be more easily read in the nodular or pre-ulcerating period, than after ulceration had occurred; and the