it with the point of a pencil, it responded to stimulation. Liquids and food also never returned through the nostrils. That it was involved, though to a slight degree, was manifested by his inability to sneeze or blow his nose, acts which do not depend in any way on the power of closing the lips. The lower facial muscles and the tongue responded feebly to faradism, and there was no reaction of degeneration. The mind seemed to be unaffected, though at times he was very desponding, and when amused he laughed immoderately.

On testing the general muscular power, it was evident that, in addition to the symptoms of progressive bulbar paralysis, for the relief of which he sought advice, his hands, upper extremities, and trunk were the seat of progressive muscular atrophy. The left hand was affected to a greater extent than the right, and exhibited the features of the "claw-hand." The dorsal interossei of each hand were atrophied, and, as well as the second and third left palmar, did not respond to faradism. The thenar and hypothenar eminences were also flattened; the abductor pollicis did not respond to electricity, while the opponens responded very feebly. The power of grasping was greatly impaired, the dynamometer registering 12k. to the right, and 5k. to the left, hand. The forearms were soft and flabby; the deltoids, and markedly the right one, were atrophied. The patient was unable to raise his right arm to the horizontal level, and without the aid of the left hand to place the right on the vertex. Fibrillary movements were seen in almost all the muscles, but were well marked in the deltoid, pectoralis major, triceps humeri, and flexor digitorum. Sensation was unaffected. With the exception of the deep cervical muscles which raise and turn the head, all of which were more or less involved as to function, and probably also as to size and structure, the muscles of the trunk and lower extremities were unaffected. There was no loss of power or co-ordination while walking, and the bladder and the rectum were healthy.

The patient remained in hospital for two months, and in some respects was much better than when admitted. He was able to use his right arm much better, and to raise it to the level of the shoulder. He was also less dejected, and was very anxious to try and work. The treatment consisted of nourishing diet; faradism and galvanism to the affected muscles; and chloride of barium, in half-grain doses, three times a day, combined with tincture of hyoscyamus.

REMARKS.—It is not my intention, nor, indeed, do I consider it profitable, to discuss the question of the origin of progressive muscular atrophy. The weight of evidence, at the hands of Charcot, Duchenne, and Joffroy abroad, and Lockhart Clarke and Ross at home, has convinced me that the "neuropathic theory" of Charcot is that which best explains the *rationale* of the disease; and that a similar primary central lesion, in the motor nuclei on the floor of the fourth ventricle, produces the paralysis and atrophy of the tongue, lips, pharynx, and larynx in progressive bulbar paralysis. The association of the two diseases is well exemplified in the foregoing cases. In both, the tongue was first affected. In Case I, eight months elapsed before the patient was aware of the presence of paralysis of the lower extremities; in Case II, four months. I say advisedly "before the patient was aware of the paralysis;" for it is more than probable that the progressive muscular atrophy had commenced its work long before the patient was made aware of it. It is a painless disease, most insidious in its approach, and fitful in its advances, involving separately several portions of a muscle, and not the whole; and it may not be until a very great portion of the muscle is invaded, or a group of muscles becomes atrophied, that the paralysis is recognised by the sufferer. This holds good for the muscles of coarser action, even more than for those of fine and delicate action. It is soon discovered in the fingers of a telegraphic or scrivener's clerk, or in those of a pianist, but very much later in the muscles of the trunk, arms, and lower extremities. I have no doubt in my own mind that, had they been looked for, fibrillary movements would have been discovered in the affected muscles of the trunk and extremities of these patients a very considerable time antecedent to the presence and date of their paralysis. As bearing on this point, I would refer to the case of the combined diseases under consideration which Charcot has detailed, in which he discerned the presence of these evidences of advancing amyotrophic paralysis in the pectorals, deltoids, and latissimus dorsi muscles, before the patient was aware that he was suffering from anything but the affection of speech.

We may therefore, I think, fairly assume that, if not synchronous in their occurrence, the interval which elapsed between the primary engagement of the nerve-centres, which gave rise to progressive bulbar paralysis and progressive muscular atrophy, was short.

Fibrillary movements of the tongue and lips are even more certain symptoms of the advancing bulbar disease than atrophy, for atrophy may not be an early sign. Paralysis may precede it by a very considerable interval, and even to the last the tongue may appear smooth

and of normal volume. This was made known by the case of Charcot referred to, in which, while it appeared unaffected by atrophy, the tongue was very far advanced in fatty degeneration. It seems to me that in all probability the condition of this organ in Stephen S. (Case I) was very much the same as in Charcot's case, for though the tongue appeared slightly atrophied, as compared with the organs of James C., (Case II) its functions as to speech and deglutition were very much more impaired; in fact, no words could be attempted by Stephen S., although he had the aid of the lips, an advantage which the other patient had not, and yet the latter was able to pronounce some monosyllables, although his tongue seemed more furrowed, hummocky, and shortened by atrophy.

These cases illustrate, alike in bulbar paralysis and in progressive muscular atrophy, the very unequal manner in which the same disease may attack different individuals, and a similar want of order by which it makes its advances to a fatal termination.

Case I, in eight months, was fast approaching that consummation; Case II, on the other hand, seemed to be in a stationary or even improved condition at the end of ten months, so far as the disease of the muscles of the extremities was concerned, while, with respect to those of speech and deglutition the disease seemed to be in abeyance.

The bulbar lesion in Case I had chiefly and most seriously involved the nuclei of the hypoglossal nerves, so as to interfere with their function in their distribution to the muscles attached to the hyoid bone, and it but slightly touched the facial; while in Case II the facial nerve seemed much involved, as well as those filaments of the ninth which move the tip and anterior part of the tongue. In neither were taste or sensation affected.

Further comment on these cases is unnecessary, beyond noting that they throw no light on the predisposing or exciting causes of progressive bulbar paralysis; and, although syphilis is accredited with producing the disease by some observers, it is not a factor in these cases. In truth, nothing is known as to its etiology, beyond the facts that it occurs twice as often in men as in women; and after 40 years of age. Heredity is acknowledged to be a strong predisponent of progressive muscular atrophy; yet, though the two diseases are but parts of the one lesion, it has not been proved to be a recognised factor in progressive bulbar paralysis.

ADDRESS ON COLLECTIVE INVESTIGATION.

By SIDNEY COUPLAND, M.D., F.R.C.P.,

Physician to the Middlesex Hospital.

ALLOW me, in the first place, to thank you, on behalf of the Collective Investigation Committee, for the invitation which has brought me here to speak of that Committee's work, before this important Branch. I very much regret that the secretary of the Committee, Dr. Herringham, should have been unable to have come himself, and that the duty should have devolved upon me; for my knowledge of the practical working of the scheme is very incomplete as compared with his, so that the most that I am able to tell is but a small portion of what might be said upon the subject. As a member of the Committee, and therefore desirous to see its work prosper, my words shall not be lacking in earnestness, although they may be deficient in scope.

In setting on foot this undertaking, the British Medical Association is making a great experiment, and it would be too much to expect that it should achieve full success at a bound. Particularly is this true of a plan that requires the co-operation of so large a number, many of whom may naturally ask whether the results to be obtained are worthy the efforts made to obtain them. Few, however, can question the fact that the idea of collective investigation is a worthy one. To bind together all the practitioners of this country for the common object of revealing the obscurities and mysteries of disease, is surely, in the abstract, a great conception. The British Medical Association, by its constitution, possesses within itself every means for realising such an idea in practice. Indeed, the scheme itself, when perfected, should act powerfully in maintaining that organic unity so much to be desired in a large association. It is interesting to notice that in Germany, where the lead of our Association has been followed in a movement for collective investigation, the initiative had to be taken by a Berlin medical society, and centres of inquiry had to be established in the chief towns. They have had to make their organisation; with us, the organisation is ready to hand. It is reasonable, then, to hope that, in our country, the movement will take deeper root than in Germany, and that, owing to our wider organisation, the inquiries may be of more importance and utility.

Delivered before the South-Western Branch.

This question of utility is the main thing to be considered in every new venture; and, as regards this movement, much has been said in its favour. The leaders of our profession have spoken in no uncertain terms upon it. It has the advantage of appealing to a general sentiment; not that its scientific value should be overrated; but this will not be small, if the scheme can be worked as thoroughly as possible. At any rate, no one can gainsay the advantage to be derived by the co-operation upon one line of research of a vast body of skilled observers throughout the country. There are questions in medicine to which the experience of one man affords no answer; which will indeed never be answered until we can collate and analyse the experience of all. By means of the collective inquiry into such matters, we may expect to advance considerably our knowledge of unsettled points in etiology, as well as in therapeutics, and learn much of the cause and varieties of disease under varying conditions.

But collective inquiry is of value in other directions than these. It gives to every practitioner the means of recording facts that are daily before him, and recording them in a way that is sure to be of use. Is it not a source of regret that hitherto, in spite of the existence of so large a body of men engaged in one pursuit, men of culture and intellect, sagacious and skilful, so little should be produced? Even with the growing literature, and increase in journalistic activity of these days, the fact remains. We all know men, whose opinions are of the highest value, who have never put pen to paper, and who will not leave behind them, except by oral precept, one grain from their vast storehouse of accumulated thoughts and experiences. Partly, no doubt, this is due to the engrossing nature of their life-pursuit; the daily round of unceasing duty in ministering to the wants of the sick leaves them little leisure to devote to the records of the facts they observe. The world is the poorer then, and our science the loser, in a very real sense, when such men die, for "their works follow them." Any scheme which should prevent this great waste of knowledge is to be welcomed, and to a certain extent collective investigation does supply this need.

But the individual worker, in contributing to the common stock of knowledge, is himself benefited. For the scheme we advocate fosters and directs his powers of clinical observation; and fixes his attention upon matters, a knowledge of which may prove to be of instant service in his treatment of the case, as they will be ultimately of value when considered as parts in the work contributed to by all.

I need not, in this room, dwell longer upon this part of the subject, for we may all concede that collective investigation is good in itself. The main purpose of my presence here was not to advocate the cause so much as to explain the manner in which the work is conducted at present in our Association.

It was, of course, essential that there should be some central body to receive suggestions, to initiate and to control inquiries, and this was supplied by the formation of the Collective Investigation Committee, whose chairman, Professor Humphry, and secretary, Dr. Mahomed, have laboured so indefatigably to promote the work. The Committee not only decides the subjects suitable for investigation, but undertakes the reports upon the returns received; and last year issued the first number of the *Collective Investigation Record*, for the publication of these reports. The second number of the *Record* will appear this summer, and by its contents you will be enabled to form a fair notion how the work is proceeding.

There have now been instituted, since the Committee was formed two years ago, no fewer than eleven inquiries, embracing subjects of widely varying interest and importance. With few exceptions, the plan of conducting the inquiry is the same in all, viz., by means of schedules containing each point requiring notice, the preparation of these being placed in the hands of men specially qualified to deal with them, who are also instructed to draw up explanatory memoranda. Their work is submitted to the judgment and criticism of the Committee before it is finally issued to the profession at large. It is to Dr. Mahomed that we owe the device of printing the schedules upon cards of convenient size; and, as a proof of the value of this plan, I may mention that it has also been adopted in the German investigation, to which I have alluded. Some of these cards—notably those upon diphtheria, hæmoglobinuria, and syphilis—are in themselves an epitome of known facts concerning the diseases, and at first sight present rather a formidable appearance, calculated to damp the ardour of the observer. Although it has been contended, and with certain justice, that this over-elaboration tends to defeat the object aimed at, and to stifle under too great a mass of learning the free exercise of observation, I cannot but think that we are liable to take too exaggerated a view of this matter. For it must surely add vastly to the interest one always feels in investigating any case, to have before one, in concise form, all the points which the case may present to be unravelled;

some of which, were our attention not specially directed to them, are likely to be overlooked. I hope, however, to hear from the members present an expression of opinion on this head, for I conceive that a weighty objection may be raised to those detailed investigations on the score of time; and that the Committee may have too much lost sight of the conditions under which the practitioner works, and, in aiming at an inquiry which shall be thorough, may have made too great a demand upon him. Practically, it has been found that the largest number of returns are received on those questions which are of limited scope; and until the profession is more generally conversant with the method, and more thoroughly convinced of its utility, both to individuals and to medical science, it is too much to expect that the returns will be numerous on the more detailed questions. Upon some of these questions—e.g., that of hæmoglobinuria—the comparative rarity of the disease is an obvious explanation of the paucity of returns. It seems to me that the great reason in favour of elaboration, is the confirmation it brings of care and precision in observation. As to the manner in which the system has hitherto worked, and the character of the returns received, I have no personal knowledge except upon the inquiry into pneumonia. Dr. Sturges and myself are now engaged in sifting and analysing the returns on this subject, with a view to their publication in the forthcoming *Record*. Without in any way forestalling the tenor of the report (which is yet unwritten), it may be of interest if I refer briefly to this inquiry, taking it as an example of the practical working of the whole scheme.

Acute pneumonia is a disease of such general prevalence, and such well marked clinical characters, as to be peculiarly well suited for collective inquiry. It might be thought that little gain to knowledge could come from such inquiry upon a disease so widely known; but there has long been much difference of opinion respecting its real nature. For many reasons, clinical and other, some of which are enumerated in the memorandum that accompanied the card issued by the Committee, the opinion—which, I believe, Dr. Austin Flint, of New York, was one of the first to formulate, although, very many years ago, one of your own members, Mr. Christian Budd, of North Tawton, arrived at a like conclusion—that acute pneumonia should rank rather with the general specific diseases than with the inflammatory local disorders, has been gaining ground. Nowhere has this view been more strenuously adopted than in Germany, where it may be said to have culminated last year in the alleged discovery of a specific micro-organism by Friedländer. The Collective Investigation Committee, in placing this subject on their list, hoped to gather evidence from the practitioners of this country which should throw light upon the conditions under which pneumonia prevails, the circumstances which specially tend to a fatal type of the disease, and generally on its course and issues. Many collateral points would also come out of this inquiry, among the most interesting being the expression of opinion as to the appropriate lines of treatment in an acute disease which mostly runs so self-limited a course; the relative incidence upon age and sex; the relative frequency with which one or other lung is attacked and the portions involved, with their bearings on the ultimate issue of the case; the association with other diseases; and the liability or not to second attacks. Not that such an inquiry should set at rest for ever these questions, as some think. There can be no finality, but there can be placed on a firmer basis the facts of the natural history of diseases, and the circumstances that influence them. The accumulation of well attested facts is a step towards a conclusion, and brings us nearer to the truth; but to say that in this way we can arrive at the whole truth, so that dogma may be introduced into medicine, is to overleap the bounds of common sense. If this should be the fruit of collective investigation, we had better cease to advocate it. The progress of medicine was long shackled by a slavish adherence to the doctrines of the "schools," and subservience to "authority." Equally detrimental to the growth of knowledge would be the outcome of collective inquiry, which should set a limit to knowledge, and create a dogma based on the experience of the many.

In what respects and to what extent this present investigation upon pneumonia has advanced our knowledge, remains to be seen. It will be found to have borne some fruit, and to have brought to light several interesting and valuable facts, particularly in reference to epidemic influences and the occasional occurrence of contagion; and it will also correct and enlarge our ideas upon many points.

Apart from the results of the inquiry, has it, it may be asked, fulfilled the anticipations of its practical working? That more than 1,100 cards should have been filled up, by nearly 600 practitioners, affords a sufficient answer to this. For it shows that it is a workable scheme, and that it only wants to be more widely appreciated for the remaining nineteen-twentieths of the members of this Association to take the subject up. Never before in the history of medicine has such

a result been obtained, and it seems to me to be full of promise for the future. I must, however, here confess to no little anxiety as to the machinery of collection and distribution of the cards of inquiries, and the expense of their maintenance. Already the post of Secretary to the Committee has become almost too heavy to be borne by a single individual, the mere tabulation of the returns involving the expenditure of time and patience which can never be adequately remunerated. It seems to me that, as the collective investigation scheme grows, many and important modifications will have to be made; and that the Association will have to consider the advisability of utilising its Branches for the purpose of collecting and tabulating the data furnished by members of the Branch. In doing this, still greater demands must necessarily be made on the finances of the Association; but, if the work is to be thoroughly done and successfully, the Association must not hesitate to expend money upon it.²

As to the other inquiries now in progress, I have but little to say. Such subjects as chorea, rheumatism, diphtheria, and albuminuria are each so full of interest, and so frequently under the view of every medical practitioner, that no doubt can arise as to the possibility of obtaining material upon them. There has, however, been started one investigation which appeals more widely than these; I mean that upon "old age," which was instituted on Professor Humphry's suggestion. The conditions of longevity have never been systematically studied; and we want the correction of ideas, gathered from solitary examples, by the results of a multitude of instances, to estimate them truly. And, in pursuance of this inquiry, we can enlist on our side the sympathies of the subjects themselves, who mostly take a pride in their length of years, and would be well pleased to give us answers to the questions we put them upon their life-history. Mr. Francis Galton's scheme of a Family Album, which was supervised by the Collective Investigation Committee, is more thorough and exact; but it will take long for it to be thoroughly known and established. This inquiry on "old age" promises, however, to be brought to an issue speedily; and from the manner in which it is taken up we may be able to infer how far Mr. Galton's expectations are likely to be verified.

Although, as I have indicated, I do not myself think that the actual labour expended in filling up many of these cards is much, and that the concise manner in which the points are drawn up minimises this labour, so that anyone really undertaking it will be surprised at the readiness with which it is done, I am free to admit that I have never had to do this personally. But I trust that no one will be deterred from assisting in these investigations on this score. Indeed, I have come here to urge you to co-operate, and to use your influence in persuading others to co-operate, in assisting in these researches. At the same time, if there be any points to which you may deem it desirable to draw attention, the Committee will be grateful for your assistance.

I should also remind you that two, if not three, of these researches have been conducted on a far more restricted basis than the rest. Notably was this the case in the inquiry into the communicability of phthisis last year. The subject was one which had been very prominently brought before the notice of the profession by certain pathological researches upon which I need not now dwell; and it was a question which admitted either a simple negative or affirmative answer—appealing merely to experience on a single point, and not requiring much detailed exposition. The result of that appeal was that, in a few weeks, 1,078 replies were received, of which number 261 gave an affirmative answer, more or less abundantly borne out by details of the circumstances; so that the evidence, on the whole, was considered to be in support of the view that, under certain conditions, phthisis is communicable from person to person. The inquiry may not have been conducted with all the precaution essential to a really scientific research; but the fact that nearly three-fourths of that large number of observers had no experience of contagion in phthisis, whereas the remainder had had more or less positive experience, is a result which may prove to be of value. In such a question as this, an affirmative, if capable of proof, outweighs many negatives.

Another inquiry has recently been conducted upon much the same lines, upon a subject which the collected experience of the many alone may be expected to throw light; viz., puerperal fever. It is to be

hoped that the subject, which will be reported upon in the *Record*, will still be maintained in the list, in order that the multiplication of records of the disease, and the conditions of its supervention, may lead to that differentiation into its components which puerperal fever must undergo. It is time that, as a simple entity, the disease disappeared from nosology; but the varied febrile conditions of the puerperal state require yet to be widely and systematically studied.

As time goes on, we shall learn better how to frame those inquiries, and how to limit them. The question of collective investigation is not yet out of the experimental stage, and we must be prepared to modify and alter plans originally formed, as we find them to work in practice. As to the questions to be investigated, there is no limit to them. Indeed, the fear is lest we may be too much embarrassed at one time by having so many from which to make a choice; and, personally, I deprecate this multiplication of inquiries. In the selection of subjects suitable for inquiry, which will promise to yield good results, the Committee invite the guidance of all those concerned. Undoubtedly, the ultimate aim of such labour is to enlighten us on the great question of the treatment of disease. The more facts we have concerning the conditions under which diseases exist, and the more knowledge we possess upon their nature, the better shall we be able to cope with them in practice. A system of collective investigation which is directed to such an end should not fail of support; and I do not think that, as the knowledge of its objects is more widely diffused, there need be any fear of this. It requires only the active sympathy and personal participation in its work of every member of the Association to make it one of the most remarkable movements which the world has ever witnessed, as well as one of the most beneficial.

TOBACCO AMBLYOPIA.

By CHARLES SHEARS, L.R.C.P. Lond., M.R.C.S.,
House-Surgeon, Liverpool Eye and Ear Infirmary.

FORTY men, suffering from loss of vision, which was probably due to the abuse of tobacco, presented themselves for treatment at the Eye and Ear Infirmary during ten months. Having taken notes of these cases, the honorary medical officers have very kindly permitted me to publish them.

"Tobacco amaurosis" has been very fully discussed, both at home and abroad, during the last few years, more especially in this country by Mr. Jonathan Hutchinson and Mr. Nettleship. There still remain, however, many points about which there is great difference of opinion; and the hope that later observation by some gentlemen present may tend to set these disputed questions at rest, has encouraged me to bring the subject again forward.

The diagnosis of tobacco amblyopia was arrived at in the following conditions: 1, where there was rapid and usually great failure of sight, not remedied by spectacles, and not due to obvious external changes; 2, where no ophthalmoscopic changes were found, or, at the most, slight pallor or redness of the disc; 3, where the patient was an excessive smoker.

The after-progress of most of the cases which I have collected seems to confirm the correctness of the diagnosis which was made in the first instance.

Of the forty cases, fourteen recovered completely; fourteen improved greatly; and in twelve no improvement was noted. Of the twelve cases where no improvement was noted, three attended once or twice only, and have not returned, though written for; five attended once only, and could not be traced; one attended for the first time quite recently; in three no improvement took place after attending the hospital for a reasonable time. Of these three cases, two would not in any way diminish their smoking; and the third was such a confirmed drunkard that, quite recently, he was attacked by alcoholic paraplegia.

I have drawn up my cases in a tabular form suggested by Mr. Hutchinson, when writing on the subject of tobacco amaurosis.

1. *State of Optic Disc.*—When attention was first drawn to tobacco as a cause of impairment of vision, very frequent mention was made of "white atrophy" supervening; and, from what I have been able to gather from the literature of the subject, a grave prognosis was given. Now, on the other hand, if a case be recognised as "tobacco amblyopia," one feels the greatest relief in being able to tell the patient that, in all probability, he will entirely recover his sight if he have only strength of mind enough to give up smoking. In 1869, Mr. Soelberg Wells, in his book on *Diseases of the Eye*, states that atrophy ensues in some cases; and Mr. Hutchinson, about the same time, recorded a case where absolute blindness resulted. In 1876, Professor Förster stated that progress to complete atrophy is very rare. Professor Hirschberg, on

² I hope I shall not be considered as too Utopian in another suggestion that arises out of this. I find, for instance, in this inquiry upon pneumonia, that about one-third of the cases reported upon came from Lancashire and Yorkshire. The large and populous centres in these counties are in part explanatory of this; but, of course, we have been unable to come to any conclusion regarding the prevalence of pneumonia in different parts of the country. The returns of the Registrar-General, which deal only with mortality, afford a certain, but by no means a safe, estimate upon which to draw conclusions on this head. It has, therefore, occurred to me that, if the British Medical Association could undertake, through its members, a system of voluntary registration of disease, a plan which could be worked side by side with this of collective investigation, a service of inestimable value would be rendered.