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In the August number of the *Indian Medical Gazette*, Major Smith has replied to my criticisms in the May number of his paper which appeared in the previous September number (1905). I have carefully read and re-read his paper, and I cannot find that Major Smith has brought forward any statistical or other evidence which can be considered to afford support to the position he has taken up in this controversy.

I will take up some of his points *seriatim*. The italics are my own. Major Smith takes exception to my understanding him to imply that he holds that vitreous "when partially lost is generated *de novo*," and adds "any one who reads my paper or the passage can see that it does not imply anything of the kind." What Major Smith actually wrote was "The vitreous seems to repair as well as any other tissue, and why should it not? The place of escaped vitreous is either taken by aqueous humour, *or is it*

renewed. . . . ." I leave it to your readers to decide whether these last four words do or do not imply a belief on Major Smith's part in the possibility of a regeneration of the vitreous. It is the only meaning I can put on them as they stand. Moreover, in the rest of the paragraph he lays himself out to defend the same position. Closing this defence he writes, *apropos* of Parsons: "It is a pity that the writers of books on general and special surgery were in so many instances not men of experience. If they were, we would not be entertained to such rubbish." To any one who has read Parsons' book, I can safely leave the judgment on this matter. It is scholarly, erudite and scientific, and requires no defence from me or any one else. It is difficult to believe that Major Smith has ever read the book he criticises so crudely.

Major Smith replies as follows to my demand for statistical evidence in proof of his contention above stated:—"After recovery of an eye from which there has been a small escape of vitreous: (1) there are no appearances of scars or bands in the vitreous, (2) normal tension is maintained, and (3) the vision of the eye does not suffer." Major Smith is evading the point. I wrote "if Major Smith can tell us that he has kept a number of cases of vitreous escape under ophthalmoscopic observation for months or years and that he has not observed the formation of bands in the vitreous detachment of the retina or other evil consequences, his evidence will be epoch-making." Major Smith's comment on this is "I have something else to do than to make out pedagogic statistics, which at best would be considered to have a human element in them." He thus acknowledges that to follow up his cases

is a task far beyond his power. No one contends that the evil consequences of vitreous escape are immediate; they take months or years to come on. Such is the experience of European surgeons who are able to follow up nearly every case they operate on, and who are therefore more *practical* men than we can possibly be. An operator in this country may logically say: "my operative practice is too large for me to follow up my cases. I don't know what becomes of them. My impression from what I see during the days or weeks they are under my eye, is that, they will continue to do well, when they go home," or he may make great efforts to follow up his cases (the vitreous escape cases in this instance) and may confront European experience with figures which, whilst they admittedly only cover a part of his practice, are yet sufficiently numerous and sufficiently consistent to carry conviction with them. Major Smith admits that he cannot follow up his cases, and at the same time asks us to believe that the complications which we fear and which the experience of others who do follow up their cases has led us to fear, do not exist. This seems to be illogical, and it will certainly fail to carry conviction either in Europe or India.

Major Smith says, "my failures do not go elsewhere." He is probably the only surgeon in the world who can say that. In view of his admission that he is unable to follow every case, one wonders how he knows it.

Again he says, "statistics are at best a very poor substitute for what can be actually seen." I would reply that it is universally recognised by scientific men that general impressions are very apt to lead to erroneous conclusions; that science is measurement; and that it is only by

the most laborious study of accurately compiled statistics that just conclusions can be arrived at. Every one admits the human element in statistics; but if such an element creeps in even where we have done our best to exclude it, how much more dangerous must it be in influencing the conclusions of those who rely on the leadings of a so-called practical experience unchecked by scientific methods.

Major Smith comments on my remarks about the diagnostic value of upward displacement of the pupil. I did not say "upward displacement of the pupil". What I did speak of was "the characteristic upward displacement of the pupil" which "indicates that the hyaloid membrane was ruptured at the time of the operation." From his remarks I gather that this sign has escaped Major Smith's attention. Major Herbert has described it at length on p. 44 of his book; Pope recognised and described it on p. 205 of the *Indian Medical Gazette* for 1901; and quite independently of either of them, I have long been familiar with it. Major Smith says "But in no case do you get it (*i.e.*, upward displacement of the pupil) without adhesion of the iris to the scar of the corneal incision, which is its sole cause." He could have made no more cardinal mistake; for the very essence of this sign is that it is independent of the incision and that "the upper half of the iris has disappeared, retracted behind the scleral margin" (Herbert). Or to borrow Pope's words the upper part of the iris disappears above the sections. Only the most superficial observer could confuse these cases with those in which the iris is caught in the wound, for the plane of the iris is so obviously posterior to that of the portion of sclera behind which it has partly disappeared.

Our opponents may urge, and with some show of reason, that since we so greatly dread the consequences of vitreous escape, we ought to produce our own statistics as a justification for our fears, and to prove therefrom that our apprehensions of the ill-consequences arising from vitreous escapes are well founded. I would answer this objection in a two-fold manner (1) positively and (2) negatively. To take them in this order; (1) We know that the European ophthalmologist has a very decided dread of the after-consequences of vitreous escape. Major Smith values European opinion very lightly; in this we differ from him; unless we have a very strong case to go on, we feel that it would be injudicious of us to go against what we believe to be a widespread and deeply rooted ophthalmological opinion. (2) Negatively we have to acknowledge that up to date our own statistics give us too little to go on for the simple reason that with our present methods the percentage of vitreous escapes is so very low that those are comparatively few of such cases to return and be seen again. It is not even as if one could count on one's failures invariably coming back to the original operator. I see other peoples' failures from all parts of India, and I do not doubt that others see mine. It is only natural that this should happen, and I do not think that *any other* surgeon will advance the claim Major Smith has advanced, *viz.*, that his failures do not go elsewhere. Seeing then that our vitreous escapes are few in number; that *probably* only a percentage of them culminate in blindness (we will presumably all concede this to Major Smith); and that of these only a percentage are likely to come back to the surgeon who did the original operation, it is obvious that our difficulties in the way of settling the

question are great. At the same time it is henceforth our obvious duty to lose no opportunity of carefully investigating the cause of failure in each poor or blind eye we meet with after extraction.

When I spoke of the "blind eyes I see from old operations in which the characteristic upward displacement of the pupil indicates that the hyaloid membrane was ruptured at the time of operation," I was dealing mainly with my experience of the failures of other operators, and I did not pretend that I was quoting exclusively from my own statistics. The circumstances of the case made it impossible that I should be so doing. The value of the evidence lay in its supporting, so far as it went the opinion of European workers. I leave it to your readers to say whether Major Smith's remark that "any evidence seems sufficient to persuade Major Elliot when he has a case to support" was deserved or not. I submit that it was uncalled for and incorrect, and it is, moreover, peculiar in that it emanates from one who has resented the criticism of another on himself as being "over the border-land of fair controversy."

Major Smith makes the following comments— "Major Elliot does not agree with me on iritis following cataract extraction. He does not call a case in which there is plastic exudation from the lower surface of the iris attaching it to the capsule a case of iritis." To thus take this sentence out of its context gives a very twisted idea of its meaning. I am sure that any surgeon who will read my original statement at the foot of page 163 of this journal (May 1906), will agree with what I have there written. Such plastic exudation as I refer to is no more to be considered inflammatory than that which seals together the lips of an aseptic wound healing by

first intention. In fact, the same conditions are present in both cases, *viz.*, the apposition of aseptic raw surfaces. One of course admits that the modern pathologist considers the changes which occur in a wound healing by first intention to be of the same nature as those which are met with in inflammation. There is, however, this great difference that whilst in the one case these changes are kept within well defined bounds, and subserve the needs of the healing process alone, in the other they overleap those bounds and produce morbid phenomena. To confuse these two conditions clinically would be worse than pedantry, as it would obscure important issues. The determining factor is the presence of sepsis. In its absence, we are bound to deny the title of "inflammation" to the process clinically, whatever may be said of the strict nature of the healing process from a purely academic and pathological standpoint. On the other hand, I have never denied the advantages to be gained from a method which is proved to eliminate iritis, always provided that such immunity is not too dearly bought in other ways.

Major Smith does not agree with me that removal of fragments of lens capsule (I object to his word "tampering" as it is unscientific in that it begs the question) after expulsion of the lens is less liable to cause escape of the vitreous, than expressing the lens in its capsule. He says "it is much more liable in my experience to be associated with escape and copious escape of vitreous." These remarks are unsupported by any statistics. In my last paper, I quoted the vitreous loss of 200 cases. In 170 cases, the capsule was left untouched with a percentage of vitreous loss of a little under 3 per cent. In 30 cases where capsule was removed the loss rose

to 5 or 16·6 per cent. What now are the figures for vitreous escape in Major Smith's operation? Do they support his contention?

Pope, after giving Major Smith's method a "good trial," gave it up, on account of the high percentage of vitreous escapes. (*I. M. G.*, June 1901.)

Major Birdwood had 47 per cent. in his earlier cases, and 35 per cent. in his later cases, of vitreous escape, whilst performing Major Smith's operation. (*I. M. G.*, June 1906.)

Captain J. C. S. Oxley had 30 per cent. of vitreous escape in 40 cases of Smith's operation.

Herbert (whose exact percentage of vitreous escapes in the complete operation I cannot exactly make out) writes: "It is thus evident that the lowest attainable percentage of vitreous accident in ordinary extraction is very distinctly lower than in the 'complete' operation." (*I. M. G.*, Feb. 1906.)

It is to be borne in mind that my figures represent a percentage within a percentage. They do not show my vitreous loss on the 200 cases, which, even with this element thrown in, was only 5 per cent. but on the 30 cases out of that 200, in which it was found necessary or advisable to remove floating fragments of capsule after extraction. It is, however, the series of 30 cases which comes within the arena of this argument. Even so, a comparison of my figures with those of skilled surgeons, such as Major Birdwood or Captain Oxley, appears to support my contention, which was, that, "Though this manœuvre (the removal of obvious floating pieces of capsule after extraction of the lens in selected cases) has thus doubled my vitreous loss; it is to be taken into account that it is a much

less dangerous proceeding than the expression of a lens in its capsule, etc."

Major Smith attacks my views on the causation of keratitis, and maintains that my figures support his contention and not my own. I leave that point to be settled by the judgment of your readers; and pass on to criticise his statement. "These things (keratitis, etc.), practically speaking, do not occur after extraction in the capsule." Is this statement correct? Turn on two pages in the August number and read Maynard's evidence on the subject. "Keratitis.—There was haziness of the cornea in 19 cases, varying in degree, and coming on a few hours after operation, etc., etc." The percentage figure here reads at nearly 11 per cent. My percentage of keratitis quoted in the paper Major Smith is discussing was 12 in one series and 14 in another. Maynard's figures and my own are strictly comparable, since we both lay stress on the value of careful note-taking, and do our best to follow up our patients, a practice which Major Smith contemptuously labels "pedagogic" (!!!) note-taking. This, I think, safely disposes of Major Smith's assertion that "these things do not occur after extraction in the capsule."

Major Smith says: "*Major Elliot does not consider after-cataract an evil of any importance*, nor an invariable consequence of leaving capsule behind." He then quotes Berry and Sim against me, and goes on to say that he agrees with the opinion of those writers, and thinks he is right in saying that "the men whom Major Elliot calls the 'masters in Europe' are of one mind on this point. Later he says "*Major Elliot regards iritis, keratitis and after-cataract as things of not much importance.*"

First let me quote some passages from my paper which Major Smith is answering. Therein I wrote: "*I do not mean for a moment to deny that iritis after cataract is a great misfortune,* but I take exception to the sweeping nature of Major Smith's statement." Again I wrote: "*As to after-cataract, I admit that it is an evil,* but not, to my mind at least, as serious an evil as Major Smith contends." And again "it has long been recognised that *the inclusion of fragments of capsule in the margins of the wound is a prolific source of deep-seated inflammation during the after-course of a cataract operation.*" These sentences show the whole tone of my paper, nor is there in it one sentence to suggest that I make light of iritis, keratitis, etc. I submit for the judgment of your readers that Major Smith has gravely mis-stated the case. I consider that Major Smith has over-rated certain dangers and under-rated others, and I have said so; but there is no justification in the whole of my paper for the mis-statements I have quoted above. They convey a wholly erroneous impression to any one who has not read it in the original.

It is true that I do not fear the consequences of needling an after-cataract, and that I do not believe an after-cataract to be the invariable sequel of the operation I perform. I stand by all I have written on this subject. My experience of needling in after-cataract has been a very happy one, thanks to my taking the same precautions to obtain asepsis in this little operation as I take for a major operation such as an extraction. In a letter to this journal published in the June number and written before he had seen my article now under discussion,

Major Herbert gave his experience in dealing with after-cataract, which coincides very closely with what I had written. In the same letter he discusses the question of "the invariable after-cataract." He deals too with the part played by the posterior capsule in these cases. Any one who will take the trouble to read Herbert's article and mine side by side will be struck with the closeness of our agreement, and with our wide divergence from Major Smith's views; yet we wrote in absolute independence of each other.

That these views which Herbert and I hold are not universally accepted, I am well aware, but when Major Smith says in discussing Berry and Sim's views, that he thinks "The masters are at one on this point," he is clearly mistaken. The views of "nos maitres" have been recently very carefully collated by the indefatigable Darier, and published by him in *La Clinique Ophthalmologique*. It would be hard to find any single point on which they are agreed, but this would certainly not appear to be one of them.

I am unable to follow Major Smith's remarks about the method of diagnosis of an after-cataract membrane. I presume his reference to the use of X-rays is meant as a form of humour. I think it is out of place in a scientific discussion.

As to the rest, does Major Smith mean us to understand that in order to diagnose such a membrane he uses a candle, and arrives at a diagnosis of the presence of a membrane by the difficulty he has in seeing a fundus, as compared with the difficulty in seeing that of a normal eye. It is almost incredible, and yet it is the only meaning I can extract from his words. *Of course,* it is always more difficult to

examine an eye from which a lens has been extracted. To do so, one always chooses a strong source of light. This is explainable on ordinary simple optical principles. It is due (1) to the absence of the normal lens; and (2) to the substitution for it of a smaller artificial lens at a greater distance from the subject's illuminated retina. The absence of the normal lens leads to a want of concentration of the beam of light entering the patient's eye, which is hence less intensely illuminated at the requisite spot. The substitution of a smaller lens farther away from the illuminated retina which is acting as a secondary source of light, must obviously reduce the amount of light available for the observer's use; the more so as in view of the absence of the normal lens, this light is divergent and not parallel in direction at the present time, and with modern methods of establishing an accurate diagnosis at one's disposal, no one thinks of *guessing* as to the presence or absence of an after-cataract by the amount of interference it presents to the passage of light through itself to the fundus. The usual, I would almost say the invariable, method adopted by ophthalmologists to effect the diagnosis of such a membrane, is to place a powerful + lens in the aperture of the ophthalmoscope, and employing a bright source of light *to obtain a clear view of the after-cataract itself*. An electric loupe will do the same thing still better, but few surgeons possess one, whilst all have an ophthalmoscope. Membranes which are invisible with an ordinary ophthalmoscope, are easily seen if a well-lit $5\frac{1}{2}$ volt lamp is used. Any one who likes can try this for himself, and place the question beyond the boundaries of mere assertion. I am accustomed to show it to my students, and I have

had the opportunity of demonstrating it to a few medical men as well. I am willing to do so any day I am asked.

I must apologize to many of your readers for entering on such elementary matters, but Major Smith's unlooked-for assertions have obliged me to do so.

I cannot understand Major Smith's expression, "the men whom Major Elliot calls, 'the Masters.'" Any one who is acquainted with the continental literature of ophthalmology must know that the term "the Masters" did not emanate from me or from any other Englishman, and that it is of no mushroom growth. It is a widely used and honoured term for men whose work and opinions command respect amongst all scientific medical men. It is extraordinary that one should be obliged to make such a disclaimer in such a paper as the *Indian Medical Gazette*.

Major Smith says of atropine: "It is one of the most dangerous drugs in the pharmacopœia, though so necessary in the old operation from the frequency of inflammatory complications of the iris . . . . The amount of congestion it produces is objectionable, and a not inconsiderable danger of post-operative glaucoma, one of the most formidable of complications, is associated with its use."

I am not aware of any evidence on which such statements can be based. I have searched carefully through the leading authorities' views on the subject of the pharmacological action of the atropine salts, without finding anything to support the view that these drugs cause congestion of the eye. Whitla says of atropine: "It has been advocated in various acute inflammations . . . . on the theory that it *contracts*

the small blood-vessels. (Whitla's Materia Medica and Therapeutics, 8th Edn.)

Sollmann says of its use in iritis that its " effects assumed *favourable changes in the circulation of the iris*". (Text-book of Pharmacology.)

Shoomaker speaks of an " increase of arterial tension " which he ascribes to " the contraction of the smaller vessels," and which he thinks " may be due to action of the drug on the muscular fibres of the walls." (Text-book.)

Again, speaking from the purely practical standpoint, it is my routine method to instil a solution of sulphate of atropium into the eyes of all cataract patients (provided no contra-indication exists) two days before the operation. The patients come up the following day in order that the lens may be described. In this way I see about 20 cases of cataract with the pupil under atropine every Friday morning. So far from these eyes being congested they are always clear and natural.

And again, whilst admitting that rest to the intrinsic muscles of the eye is a large factor in the action of the atropine salts, would the drug be universally resorted to by ophthalmologists in iritis if it at the same time produced congestion of the iris ? Certainly not.

Most writers on the subject are content to ascribe post-operative glaucoma to the consequences of the impaction of a tag of capsule, or possibly of iris in the lips of the wound. I fail to see the need to conjure up an assumed action of atropine to explain a phenomenon which is easily and satisfactorily explained on simpler grounds. Is it possible that Major Smith is confusing atropine-irritation of the conjunctiva with deep-seated congestion of the eye ? With

the former phenomenon, we are of course all well acquainted.

No one disputes that under certain conditions the instillation of atropine may lead to an increase of tension, but all alike ascribe this to crowding of the iris into the angle, and interference with free outflow. In cases in which the tension is doubtfully or decidedly increased, no one thinks of using atropine. Personally I then substitute hematropine, or else dispense with a mydriatic.

As to the danger of post-operative glaucoma of which Major Smith thus speaks. On what figures does he base his remarks? Out of in 1,100 cases which I published in the *Indian Medical Gazette* in 1897, I only met this complication three times. Again, out of 750 cases published in the *Lancet* in 1902-3 there was not a single case of the kind. Together these figures give a percentage about 0·16 per cent. I am aware that others have met with the complication more often, but even then it is a rare event.

I therefore consider that Major Smith has greatly exaggerated the danger in question, and I further am of opinion that the theoretical basis on which he founds his arguments about the action of atropine is opposed to the usually accepted views on the subject, and is unsound.

I am moreover at a loss to explain the apparent contradiction contained in the above two sentences I have quoted from him. In the first, he says that the use of atropine is "*so necessary* in the old operation *from the frequency of inflammatory complications of the iris*"; whilst in the second, he asserts that "the *amount of congestion it* (*i.e.* atropine) *produces is objectionable.*" The two statements appear to be in flat contradiction of each other.

Major Smith has repeatedly laid stress on the necessity for seeing this operation performed by himself or by one of his pupils before one can do it properly, and I have been asked by a surgeon in another presidency, whether I think this claim can be entertained. I have not the least hesitation in replying in the negative. I am confident that there are a very large number of men in India, who can soon learn to perform *any* operation which has been *clearly* and *intelligibly* explained to them.

Major Smith asks me "Who are the masters in Europe?" It is a strange question, and only serves to accentuate the difference in the position taken up by Major Smith from that which most of us are content to occupy.

Again, Major Smith chafes under the treatment accorded to him by the profession at Home. It is not possible that other motives besides blind prejudice against a successful Anglo-Indian surgeon actuated the B. M. Association meeting of 1903, whose attitude towards himself Major Smith complains of. Most men in our service who have gone Home, have found their experience coincide with my own, that the status voluntarily accorded to us by our confrères at Home has been altogether too flattering—embarrassingly so at times, in fact; and this both on the continent and in Great Britain. In no case can we hope to better their opinion of us by writing in the way Major Smith has done. It has been the most regrettable feature of this controversy. Whatever professional men at Home may think of one of us, or of us as a body, their self-respect would keep them from discussing us in the discourteous and ungenerous way in which Major Smith has discussed them. Indeed, on one occasion he has gone so far as to

impute deliberate deception to "a few of the leading men in Europe," when a much simpler explanation of the problem would have occurred to any trained ophthalmologist, an explanation which must have suggested itself to many of your readers at the time, and which is more probable, more generous and more scientific. I refer to the remarks on p. 327, column 2, para. 3 of the *Indian Medical Gazette*, September 1905.

In the same connection, I would enter a protest against the remarks Major Smith has made about Major Herbert in his last article (September 1906). Fortunately, Major Herbert's reputation both in Europe and in the East stands too high to be in the least affected by such an attack; but the very fact of its being made marks an epoch in the degeneration of this discussion.

Major Smith has on several occasions endeavoured to rank himself and his movement with Freyer and Keegan in the litholapaxy revolution. Considering that such an attitude on his part reflects severely on the many who have not followed his practice, it would surely have been better for him to wait till others than himself enrolled him in such distinguished company. Maynard has dealt with this point so caustically and clearly on page 319 of the *Indian Medical Gazette*, August 1906, that I need merely to refer your readers to the third new para. of that page in the second column.

Major Smith accuses me of "posing as the guardian of the junior surgeons in India," and says they are men as capable of judging as myself.

I entered this discussion with reluctance, for the very reason that I have unbounded confidence in the judgment of the rank and file of our service, and I felt sure that they would in

time come to a right line and hold it. I was deterred from that policy of isolation by being asked by a number of men in the service to answer Major Smith's claims. Since my last article appeared I have received letters from various brother officers, in Bombay and Bengal, as well as in Madras, which have been all too kind in their tone. I am aware of my own limitations far too well to attempt to dictate to such a body of men as our service is composed of. What I have endeavoured to do in both these papers, is to place some of my experience (and like Major Smith I now count my extractions by the thousand) at their disposal. That the tide of opinion will flow irresistibly in the right direction, whatever that may be, I am confident, and in that confidence in my brother officers I leave the disposal of this important question without a fear or a doubt.
