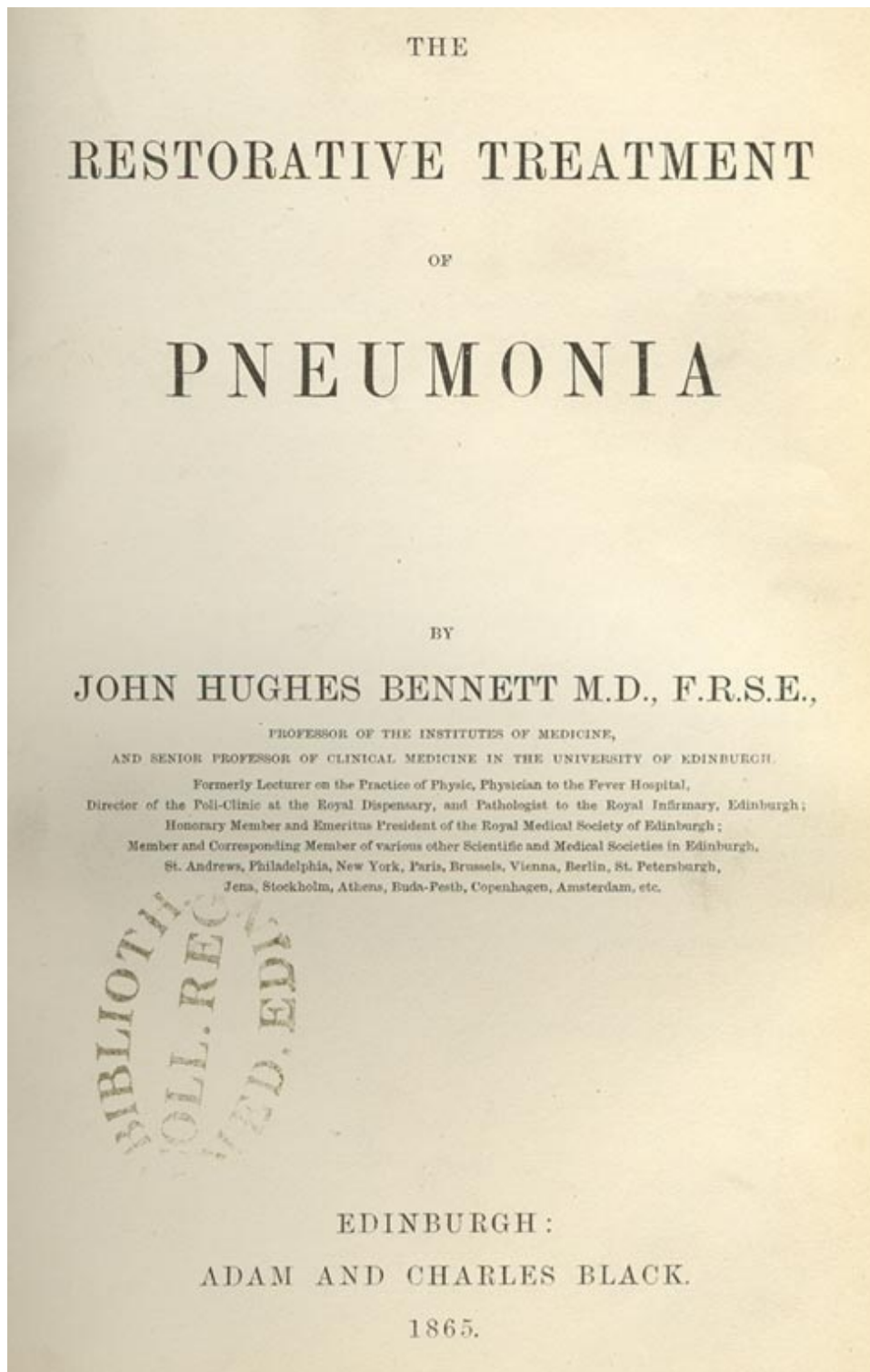


Bennett JH (1865). The restorative treatment of pneumonia. Edinburgh, AC Black.

Title pages

OBJECTION IV. *Statistics are incapable of determining the value of treatment.*

On no subject does the contradictory character of medical reasoning become more apparent than on that of medical statistics; because, whilst every practitioner is constantly endeavouring to multiply those cases which seem to prove his treatment to be successful, he regards with aversion everything that reminds him of failure. Nothing is more common than to see all sorts of remedies recommended to our notice on the faith of a few apparently good cases, whereas nothing is so rare as to find careful records of treatment in a series of cases, including the failures. How common, also, is the tendency to ascribe recoveries to medical skill, while the deaths are referred to the inevitable progress of the malady. Although philosophical physicians have in all times pointed out the fallacy of these beliefs, they still hold almost universal sway over the medical profession. The descriptions of systematic writers on medicine have tended to foster this state of things, in which we find accounts of maladies neatly divided into stages, forms, and varieties, and a treatment recommended—said to be successful according to experience—much of which, however, when tested clinically, is soon recognised to be inconsistent with reality. There is only one method of extrication from the difficulties so created, and that is by numbering and analysing well-recorded cases. In every proposition regarding the treatment of disease, we cannot avoid the consideration involved in statistics. It is no argument to say that they may be defective. If so, they must be rendered exact; and cases must be carefully taken, rigorously collected, and critically analysed. In no other way can we guard ourselves against representations of sanguine persons, generalisations from imperfect data, and confident assertions and

assumptions based on the memory of success and the forgetfulness of failure.

The great objection always brought against medical statistics is the limited number of the facts from which conclusions are drawn. No one observer, it is argued, is capable of collecting a sufficient number of cases to enable him to derive exact information from them. Dr. Barclay, in a small work entitled "Medical Errors," has recently endeavoured to support this view by an algebraical formula, which leads him to the conclusion that, if variable circumstances exist to the number of 15 in any given disease, no less than 32,000 cases would be required before we could meet with two of them exactly alike. He goes on to point out that these variable circumstances, such as differences in time, place, age, etc., oppose an insurmountable obstacle to obtaining similarity in cases. But it may be asked, is such exactitude in every particular necessary? because, if so, it might just as reasonably be argued that we ought not only to avoid comparing cases which occur in different countries and cities, but also in different houses, or even beds. The line of argument adopted by Dr. Barclay might apply to the chances of meeting with exactly the same combination of numbers in throwing ten or fifteen dice, but is wholly inapplicable and out of place in reference to medical cases. It is well observed by Louis* that the leaf of a tree having been well described, can always be recognised. It is not necessary, in order to compare one tree with another, that every individual leaf on each be identical in size and form. So with diseases: the essential characters admit of being known and so compared with one another as to allow the formation of general laws, which every-day experience confirms.

But Dr. Barclay declares that an attempt lately made to

* See his admirable memoir on the numerical method, in the first volume of the *Mémoires de la Société Médicale d'Observation*, p. 38.

obtain a large number of cases of pneumonia by the aid of the British Medical Association can lead to little benefit; because, among other reasons, "acute pneumonia is just one of those diseases in which a certain number of individuals attacked will die, in spite of any treatment yet known, while a certain number will recover if entirely left to themselves." The assumptions contained in this one sentence afford an excellent example of loose reasoning, and of the necessity of that statistical knowledge which the author condemns. What entitles Dr. Barclay to affirm that a certain number of individuals attacked will die in spite of any treatment yet known? The statement is evidently a gratuitous assumption, and begs the very question at issue. Its correctness is opposed by the fact, demonstrated in these pages, that 105 consecutive cases of primary and uncomplicated pneumonia under a restorative treatment all recovered. Should he not, consistently with his own argument, instead of opposing the employment of statistics among the members of the British Medical Association, urge them to second my endeavours? For if, according to his calculations, 32,000 cases be required, and the members of that Association number 2500, only 13 cases from each, instead of the 129 I have myself furnished, would serve to solve the problem in his own way. I believe, however, that 100 well-recorded consecutive cases, and in some instances half that number, are amply sufficient to test the value of any therapeutical remedy whatever.

Dr. Barclay, after pointing out the necessity of extreme similarity in the cases which are to be compared, and fully admitting the propriety of not "jumbling together the different experiences and cases of different practitioners," refers to the able paper of Dr. Sibson formerly alluded to, who has collected statistics of pneumonia from various sources, and given them in a tabular form, divided into two columns, headed, "Bleeding and Non-Bleeding Plan" respectively. Of

this he remarks : " Although he (Dr. S.) draws various conclusions from a strict analysis of all that admit of it, he does not even sum up the figures which he gives as a whole." But what Dr. Sibson as an able statistician did not do, knowing well the absurdity of adding together cases which, although bled or not bled, differed widely otherwise with regard to their treatment, Dr. Barclay actually does, with the following result : " Of 1750 patients, treated by repeated or large bleedings, the mortality was 18.5 per cent. Of about 1000 treated by few and small bleedings, it was 13.5 per cent. Taking both these together, the cases in which blood-letting formed one part of the treatment, gave a death rate of 164 in the thousand ; while 10,000 cases, treated almost entirely without venesection, gave a death rate of 203 in the thousand." It is thus made to appear, from cases treated in the most opposite manner, that bleeding in pneumonia causes only 164 deaths, while non-bleeding increases the mortality to 203 deaths in the thousand. So that the arguments, calculations, and peculiar statistical views of the author of "Medical Errors" has conducted him to what must be recognised by every medical practitioner to be one of the most erroneous conclusions ever arrived at in modern times. I hold it to be unnecessary to expose more in detail the argument by which Dr. Barclay endeavours to show that counting cases is useless in medicine, as its fallacy must be obvious to the merest tyro in statistical inquiry.

What is really required is that cases should be carefully observed and recorded by hospital physicians on a uniform plan. I still venture to think that, with reference to treatment, the facts recorded in the schedule issued to the members of the British Association are amply sufficient, and that they are easily arrived at. They are exactly the same as those in which I have recorded my own cases (see p. 12, *et seq.*) If others would follow the same plan, it is clear compari-

sons might be instituted, and all the essential sources of error avoided.

I cannot help thinking that the slight trouble required, and the general want of interest which prevails on such topics amongst the members of an overtaxed profession, are the real causes which led to the failure of the attempt in 1863.* A few also were doubtful as to whether cases of pure pneumonia, uncomplicated with pleurisy, bronchitis, or other disease, were required. It seldom happens that a pneumonia exists independent of some bronchitic or pleuritic affection; but this, if slight, in no way affects the result. If, on the other hand, it be intense, so that the pneumonia be secondary in importance, this, if recorded, must be stated in the appropriate column, and all error will be avoided in collecting the cases.

I still hope that the sanction of large numbers may be given to the views contained in this pamphlet, so that a great practical subject in medicine may be finally settled. We might then hope that a similar investigation might afford trustworthy results in other diseases; and thus the practice of our art approximate more towards uniformity. Notwithstanding all that has been said about the fallacy of statistics and the trouble of drawing them up, I can conceive no other method of settling the vexed questions of medical practice than a careful record of cases on some general plan—obtaining exact and similar facts—and then counting the results.

So far, therefore, from believing that correct statistics are incapable of determining the value of treatment, I consider it to be demonstrated that they offer us the only means of recognising the superiority of one treatment as compared with another, and of establishing how far remedies diminish mortality and shorten the natural progress of disease.

* I may observe that only 15 members of the British Medical Association returned their schedules to me, containing 45 cases of Pneumonia.