

wards, as well as for those not quite so large, which are so peculiar in form (as occasionally, but very rarely, happens) that the lithotrite fails to grasp or retain them? I think there is no doubt about the answer—viz., that it is the suprapubic and not the lateral position.” And again: “Finally, I think I am quite justified in believing that unless the operator has had a large experience of lithotripsy (and there are not many of whom this can be affirmed), the high operation would generally be a safer proceeding than crushing for a calculus which is hard and much above an ounce in weight.” In regard to the danger of impotence from peritoneal section, to which I alluded in my paper above mentioned, I am permitted to quote the following extract from a private letter received from my friend, Dr. A. H. Haemstadt of Pottsville, Pa.—a lithotomist of large experience: “Your eleventh accident—impotence—is well put; only one of eighteen whom I cut as boys, grown to manhood and married, has had issue as far as I can learn.”

One of the strongest arguments in favour of the suprapubic operation is that it avoids all interference with the genital organs, thus preventing the so frequent reflex disturbance of the nervous system accompanying the introduction of instruments through the urethra—a practical point to which, I believe, I was the first to publicly invite attention.

I am, Sir, yours truly,

W. G. TREMAINE, M.D.,

Major and Surgeon, U.S. Army; Surgeon-in-Chief,
Buffalo Sisters' Hospital, &c.

Buffalo, N.Y., Feb. 15th, 1886.

PASTEUR'S EXPERIMENT.

To the Editor of THE LANCET.

SIR,—In your leader of March 20th, and in the special article in THE LANCET of last week, which are devoted to a description of the method by which M. Pasteur secures and uses the protective virus, the attention of the medical profession is intelligently drawn to a subject of great interest and importance. For the moment M. Pasteur is concerned with hydrophobia alone, but the issues presented to the mind of anyone who has become practically acquainted with the inception and progression of disease are many and far-reaching. It may be said of all disease that the period of active pathological change is preceded by a period of passive fermentation, which prepares the whole animal structure for the consequent and more perceptible morbid process. At present, however, this period of latency in the majority of diseases cannot be exactly determined, and, indeed, the conditions and circumstances of disease, regarded as they are at present under the somewhat uncertain light of our knowledge, are strangely various and bewildering. Perhaps, if the jaded general practitioner of medicine, careful and troubled like Martha about many things, were to express his mind freely concerning the mysterious vagaries of his many-faced adversary, Virgil's unkind description of the gentler sex—

“Varium et mutabile semper femina”—

would, with a trifling alteration, suit his purpose best. There are, however, certain diseases in which the period of incubation can be exactly determined, and in other cases by careful and long-continued observation the scientific physician has arrived at conclusions which are found to be erroneous only in exceptional cases. M. Pasteur proposes to deal with a disease during its incubation period, and has selected a disease which is “only conveyed by direct contagion, so that the date of the introduction of the poison into the system can be fixed with precision.” (THE LANCET, March 20th). At present M. Pasteur proposes merely to inoculate persons who have been bitten by mad dogs, but, as you say in the leader from which I have already made a quotation, “in process of time it may become the fashion to inoculate with the mitigated virus in order to give protection against the possible bites of mad dogs, but at present there seems little likelihood of this practice being established, nor, on the whole, is it desirable.” The practice would not, in my opinion, be desirable, because hydrophobia is a disease of the lower animals, and is only occasionally transferred from them to man; but if M. Pasteur succeed in his experiment, it might become desirable that all dogs should be protected by inoculation. By his brilliant discovery Jenner drove out from the midst of the people a terrible and deeply-rooted disease. In like manner M. Pasteur may

succeed in removing from the animal world the disease against which his scientific labours have been for some time directed. If success should attend his efforts—and at present there is little reason for anticipating failure—a great advance will have been made in the preventive treatment of disease. I may be too sanguine in expressing my belief that the experiment which is proceeding at Paris is a swift approach towards the clear noonday of scientific medicine of which Jenner's discovery is the brilliant dawn. There is certainly ample reason to be found for the belief in the success which has attended the theory of vaccination in practice, and in the patient and questioning industry of the great French pathologist and chemist. He is indeed a worthy successor of his countryman, Lavoisier, who, dragged down with many others into the ruthless whirlpool of the first Revolution, asked for a fortnight more of life in which to finish an experiment.

In speaking of M. Pasteur and his experiment, one is led naturally enough to consider the manner in which it has been received by those who are opposed to science on purely sentimental grounds, and by those who, with varying degrees of responsibility, take part in the government of the country. In a recent and very admirable work Sir Henry Sumner Maine arrives at the somewhat sad conclusion that science will suffer under a government by the people. I have mentioned Lavoisier and his pathetic request, but, as Carlyle says, “the Republic had no use for experiments,” and so the fortnight more of life was denied to the chemist. We have not yet descended to the depth of moral and social dissolution to which the French people were dragged by Robespierre, Marat, and company, but we are hurrying on at a startling pace, and only the other day, at the call of a shrill and shallow sentiment which would send all science to the winds, the Contagious Diseases Acts were removed from the Statute Book. “The day of checks and counterpoises is over,” exclaimed Mr. Labouchere some time ago in the *Fortnightly Review*, and the present House of Commons is showing that there is some truth in the exclamation. “Government by the people for the people” (this is the formula I think) will mean a paralysis of scientific effort. True science can never be popular in the ordinary acceptance of the word. Unlike Radicalism, it does not promise well on the surface, and does not leap from a chaos of improbable and indefinite premisses to time-serving and popular conclusions. The best and most useful part of scientific work is hidden from the notice of the multitude. The days of earnest endeavour towards the practical attainment of a great scientific end are unheard of by the busy throng who thrive unthinkingly upon the discoveries that the votaries of science have made. At present we may congratulate ourselves that we are yet some distance from a government by the people, and rejoice in the fact that in spite of the absorbing Irish problem, a Governmental commission is to be appointed to investigate into the subject upon which M. Pasteur is experimenting. The names which you mention are of themselves sufficient to satisfy those interested that the work will be done in a thoroughly efficient fashion. Scientific investigation of the severest kind must always be applied to every discovery of moment, and in the instance of M. Pasteur's experiment the discovery is of the first importance. For my part, I believe that investigation will but prove the truth of the theory by making sure its practical success.

I am, Sir, yours truly,

Glasgow, March 30th, 1886.

ROBERT MACPHERSON.

HEALTH-RESORTS FOR PHTHISICAL PATIENTS.

To the Editor of THE LANCET.

SIR,—I read with much pleasure in your issue of March 13th a leading article on Health-resorts for Phthisis. My purpose in writing now is to endorse, with what weight an experience of twenty-two years' practice in Sydney may carry, the truth of your remarks, in so far as they relate to Australia. I have frequently expressed to my medical friends here, and in words almost identical with your own, the great importance of warning consumptive patients, when contemplating removal to New South Wales, against taking up their residence in Sydney or any other place upon the eastern seaboard. During nine months of the year the atmosphere is warm and moist, and the surroundings highly detrimental to the phthisical invalid. Tubercular deposit rapidly undergoes softening, and night sweats, loss