

bottle. The protection afforded by the reduced dose was probably less than that produced by the larger dose, but we had to bear in mind the fact that a severe reaction following inoculation always tends to make the operation unpopular.

In deciding upon the most suitable dose of vaccine to use, two points have to be kept in mind:—

- (a) The degree of immunity produced by a given dose;
- (b) The amount of discomfort (fever, pain and swelling) produced by that dose.

Speaking generally the larger the dose of vaccine given, the greater is the immunity produced; but the size of the dose is limited by the severity of the reaction which follows inoculation, for the larger the dose, the more severe the reaction. In selecting a suitable dose of vaccine, therefore, care has to be taken to give as large a dose as can be borne without great inconvenience. For this reason when fresh or immature vaccine is used a smaller dose has to be administered than when mature vaccine is used."

After describing the work of Capt. Taylor, I.M.S., on rats it is remarked:—

"With these data before us we are in a position to gauge approximately the **reproductive powers of rats**. A single pair produces, say, 6 rats every two months; these young rats are able to produce young ones in turn in approximately 4 months. Males and females are produced in about equal numbers. On an average, say, one litter of every three produced is destroyed by the parents before they reach maturity. In these circumstances in less than one year the single pair may have multiplied to 50 pairs. If this rate of multiplication were continued for a year and a quarter, the number of rats would be very great indeed. As a matter of fact observations in the field show that such rapid multiplication does not actually take place: it is probable that overcrowding, destruction by natural enemies, and disease keep down the numbers. There is another very important factor which has to be taken into consideration in making such calculations. It should be remembered that not every pair of rats is fertile. The number of rats we experimented with was too small to yield any reliable estimate on this score, but by a somewhat complicated calculation we reckon that somewhere about one pair in every four may be considered to be infertile in the sense that reproduction in these does not go on at the estimated rate of a litter every two months. Even making this allowance but taking no account of the effect of disease and destruction by enemies, it does not seem possible to give a lower estimate than that a single pair of rats may multiply to 40 pairs in the course of a year.

Experience in the field does not by any means confirm this estimate. Difficulties are met with in field work which prevent us making an accurate estimate of the rat infestation of any place. The experience, however, suffices to put us on our guard in making calculation regarding the reproductive power of rats from purely laboratory observations. Sufficient has been learned by these experiments to convince us that rats multiply very rapidly and the important deduction can be drawn that any destructive measures against rats must be very thorough and very persistent."

There are numerous reports showing the great efficacy of **anti-plague inoculation**, and we quote in full the following experiences in H. H. the Nizam's Dominions.

"During the past year a very severe epidemic of plague broke out in Hyderabad (Deccan). Lieutenant-Colonel Drake Brockman, I.M.S., came to the conclusion that a vigorous inoculation campaign would be the only means of checking the epidemic, as evacuation on any large scale was impracticable. Inoculation was done on an absolutely unprecedented scale, a striking example of what energy can accomplish. Up to date only a few records of results have come in, but these with

Lieutenant-Colonel Drake Brockman's permission will publish. He writes:—

"I took every precaution from the first to make it (*i.e.*, inoculation) a success, paying great attention to many little details in order that the records obtained would furnish us with reliable *data* upon which possibly useful deductions could be made. Enclosed I have briefly noted down for your information the details I refer to, and I think that you will agree with me that the whole thing has been thoroughly carried out. The inoculation campaign in this city was started in last September and practically ended in January, a period of about 4 months only, and during that period I was able to inoculate over 75,000 human beings, and that too without entertaining any really extra establishment but done with the agencies of my dispensaries and their medical subordinates in charge. I think, humanly speaking, that these records could not have been more carefully kept, for the whole campaign was from first to last under my personal direction and supervision, and I never allowed any person to perform the inoculation who had not been most thoroughly taught the whole technique of the operation before being allowed to perform it; moreover, I went sometimes 20 miles a day in my motor and constantly visited the places where inoculations were being carried on, and beyond one arm much inflamed which was in the case of a European who foolishly played hard tennis directly after being done, I cannot honestly remember any untoward result of the operation out of the whole lot—men, women and children. It was a sight worth seeing in the mornings, the crowds of these people hanging about the roads outside these dispensaries awaiting their turn for inoculation."

With reference to the statistics he supplied, he writes in another communication:—

"I am also sending you herewith an abstract of figures from these forms which up to date we have had returned to us as verified, which you can rely upon absolutely:—

	No. of Subordinates and other families.	Population.	Attacks.	Deaths.
Return received from 33 various departments up to 18th April 1912.	Inoculated	3,071	47	21
	Uninoculated.	2,988	204	204

He remarks:—

'All living under precisely similar conditions.'

Correspondence.

THE SMITH OPERATION.

To the Editor of "THE INDIAN MEDICAL GAZETTE."

SIR,—In your November number you publish a "special article" entitled "The Vexed Question of the Smith Operation" and say "we here reprint two articles on this subject which have recently appeared and to continue our strict impartiality on the subject we give views of both sides." You proceed to republish a letter of an intensely hostile and personal nature which appeared in the Ophthalmic Record of June 1911. Do you call this recent? This letter is of no scientific importance. You do not republish my reply to this letter from the same Journal of April 1912. Is this what you call strict impartiality? This letter was part of the Kilkelly Smith controversy, what in your special article is made to represent the other side of the question is the republication of an article which has no bearing on the above mentioned letter nor on the Kilkelly Smith controversy. You then finish by saying that "Those interested in the subject will doubtless have read the very interesting review by Major Elliot, I.M.S., in the Ophthalmoscope of September 1911 on

Dr. Derrick Vail's little book. There again you do not tell your readers that the "little book" was not a little book, but the reprint of an article published in the *Lancet* clinic of Cincinnati, not purchasable in the market, but only to be had from the writer as a favour. It is unusual, I think, to review anything which your readers cannot purchase. You do not refer your readers to Dr. Vail's and my replies to that review which appeared in the *Ophthalmoscope* in the current year.

Major Elliot's review and replies are composed for the most part of intensely hostile and personal criticism of me apart from the merits and demerits of the operation with due deference. I would invite your attention to the fact that what appears on this subject is divisible into two issues—on the one hand myself personally whether friendly or hostile—on the other hand, the scientific aspects of this operation. These two issues are mixed up and confused by the opponents of this operation of what interest to science are my personal doings! (They might be of interest, if the writers were honoring me by writing a posthumous biography).

I think it would be in the interests of the *Indian Medical Gazette* if the articles and letters copied from other journals were confined to the scientific and practical side of intracapsular extraction.

Yours truly,
H. SMITH,
LIEUT.-COL., I.M.S.

AMRITSAR.

[The proof was long in hand and at time of its printing the reply of Lt.-Col. Smith had not been seen. Moreover we also published a still longer article laudatory of Lt.-Col. Smith's work. In this matter we still say that we have only one desire, *viz.*, to lay before our readers an impartial account of the views of ophthalmic surgeons on this interesting and important subject.—ED.]

BLUE PATCHES ON NEW-BORN INFANTS.

To the Editor of "THE INDIAN MEDICAL GAZETTE."

SIR,—Recently a correspondent published a note on the above subject and considered these patches, if I remember rightly (I can't put my hand on the copy of the article at the time of writing) as an interesting point in anthropology.

I should like to point out these patches were noted by the Census Commissioner of the Punjab eighteen years ago and I have taken some interest in them. These blue patches are to be seen in the skin of Mongolian infants. Every Chinese, every Roman, Japanese and Malay child is born usually with a dark blue patch of irregular shape in the lower sacral region. The patch may be equally divided on both sides, or it may not be laterally disposed. It may be only the size of a shilling or at other times it may be as large as the hand. In addition sometimes there are also other patches on the trunks and limbs but never on the face. They have been in rarer cases, I understand, so numerous to cover nearly half the surface of the body. These patches may be likened to a bruise as by a fall. They usually disappear in the first year of life, but sometimes last for several years.

Up to a year ago and in little over a year the midwife working under me in Lahore city had 174 cases with such blue patches. Most of the children born in the Lahore city have these patches, and the children of Hindus and Mahomedans are equally affected. There is generally one big patch about the region of the sacrum, though there are sometimes several smaller ones on the lower part of the back. Though she ascribes them to the placenta, in my opinion they cannot be produced by it as, of course, we should expect them also in Europeans.

My observations as to their origin are as follows:—These patches are due to the effect of pressure on the back of the child due to the method of native (and other oriental) women tying their skirts about the level of the umbilicus. There is usually a knot in front and this may at times change its position. This presses against the lower part back of the child *in utero* and is liable to make the part pressed unduly congested and pigmented as a result of the intermittent pressure destroying delicate vessels in the delicate skin of the child. When one presses the finger on a table or book at first there is anæmia, and when the pressure is relieved there is slight hyperæmia. In the case of the unborn infant the pressure is probably intermittently (so to speak) sufficient to injure the delicate vessels and probably lead to the extravasation of blood as results in a bruise. It is more commonly prevalent in normal pregnancies, because in normal pregnancies some part of the back is towards the front, and is more likely to be seen about the sacral region because the sacral region is first likely to come under this pressure. Likewise the squatting habits of women in the East may help to produce the other patches on the body according as the child does or does not change its position. In Europeans this patch is not seen simply because European women wear corsets which distribute the pressure of the skirts, and if not wearing corsets

they usually go in for a loose gown which is kept up from the shoulders. There is no 'squatting' with them but reclining on a sofa or couch. This appears to me to be the cause of these patches which are not pathologic and disappear usually in the first 18 months of the child's life.

At the beginning of this year I desired further information about these patches, and brought the matter before the Punjab Branch of the British Medical Association, but none of those present had any experience of them.

A. G. NEWELL, M.D., D.P.H.,
Health Officer, Lahore.

[See *I. M. G.*, August 1912, p. 306, for Dr. Fink's paper. In our Editorial Note (p. 324), we showed that Beal's claim that these marks are a sign of Mongolian blood could not be sustained. Dr. Newell's theory is of interest. Further correspondence is invited.—ED., *I. M. G.*]

FORMALIN AGAINST FLIES.

To the Editor of "THE INDIAN MEDICAL GAZETTE."

SIR,—In reply to D. P. H.'s query in *The Indian Medical Gazette* of December 1912, regarding the best way of using formalin in order to kill flies, I found the method of exposing it in shallow plates to be useless. An excellent way is to sprinkle the mixture of formalin, milk and water in tiny pools of from $\frac{1}{4}$ inch to about 1 inch in diameter on the floors, shelves, tables, etc., when the flies readily drink it. I used this method during the last rains in the jail kitchen here with great success.

RAJKOT,
13th December 1912.

Yours faithfully,
W. M. HOUSTON,
MAJOR, I.M.S.

RE BEDBUGS AND THEIR DESTRUCTION, 1912.

To the Editor of "THE INDIAN MEDICAL GAZETTE."

SIR,—I am glad to note that Sub-Asst.-Surgeon Satkari Ganguli after trying turpentine on bedbugs writes his experiences in the October 1912 issue of this your esteemed journal, and passes his glowing opinion on its effectiveness in destroying them. This is as you know what I claimed in my article of the 28th August 1911 in the October 1911 issue of the journal, which was the first article I wrote on the subject.

Please allow me to explain fully that I use the medicinal turpentine (spirit), and to prevent evaporation I make it into an emulsion of equal parts of soap suds. Without soap suds it has not the same effect, owing to the bedbugs having very smooth, and hard backs the turpentine alone runs off, and they sometimes can slip away and escape destruction. The suds fixes the turpentine to them in a way in which they can never escape with their life.

HOOLUNGOORIE,
MORIANI P. O.,
ASSAM,
3rd December 1912.

Yours, &c.,
TAFOZOOL HOOSAIN,
NATIVE DOCTOR.

THERAPEUTIC NOTICES.

"ECSOLENT COMPOUNDS."

WE find we did an unintentional injustice to the Ecsolent Compounds in our issue of November last and therefore quote in full the extract from the *British Medical Journal* of 21st January 1911 (p. 144):—

"In the *British Medical Journal* of February 2nd, 1910, p. 136, there appeared a short notice of three preparations supplied by the Ecsolent Compounds (Saracen Buildings, Snow Hill, E. C.) intended for the treatment of eczema and other skin affections. These comprise an ointment, a soap, and a dusting powder. The ointment is made with a basis of soft paraffin, and contains zinc oxide, starch, boric acid, and resorcin, with small quantities of a number of antiseptic and aromatic substances; the soap shows no free alkali, but is slightly superfatted, and both this and the powder contain some of the aromatic and antiseptic ingredients used in the ointment, while the basis of the powder is talc, with zinc oxide, starch, and boric acid. We expressed the opinion that "from the composition of these preparations they may be expected to prove useful for the purpose for which they are intended." Practical experience has confirmed this expectation, expressed nearly twelve months ago. We have had the opportunity of satisfying ourselves that the ointment is of great value in the treatment of itching eruptions, especially about the anus and genitals, and the powder has proved an excellent application in cases of troublesome intertrigo in the folds under the breasts, on the