globin, the oxygen-carrier of the blood, and prevents it from giving off oxygen after it has taken it up. When the blood is exposed to the vapour of the drug as it passes through the lungs, the oxygen becomes

FIG. I.

FIG. II.

FIG. III.

locked up in it, and cannot be given off to the tissues for their use, so that however much oxygen the blood in the arteries may contain, it acts very much as if it were venous. In consequence of this, I have found that, when the vapour of the nitrite is passed into the lungs of rabbits, they are seized with suffocative convulsions. The greater part of the blood which passed through the lungs having been acted on by the vapour, it affects the brain just as it would have done if the trachea had been closed, and no air allowed to come near it during its passage through the pulmonary vessels, and therefore convulsions occur as soon as it reaches the brain. Professor H. C. Wood of Philadelphia, who has lately written a valuable paper on the action of the nitrite, found no convulsions after injecting it subcutaneously. By giving it in this way, the blood was not acted on during its passage through the lungs, and asphyxia was prevented. This, I think, shows that if nitrite of amyl is to be used at all as a remedy in cholera, it must be given internally or by subcutaneous injection, and not by inhalation. This mode of administration seems to me to be indicated also in spasmodic asthma, when the arterialisation of the blood is also impaired. In angina pectoris it will act when given internally, but a larger dose is required than when inhaled; and it remains to be seen whether it can be given in cholera in sufficient doses to relax the capillaries without, at the same time, injuriously impairing the respiratory changes in the blood and tissues.

MOLLUSCUM CONTAGIOSUM.

By BALMANNO SQUIRE, M.B.

IN reference to Dr. Liveing's note on his case of molluscum contagiosum, I may mention that some years ago a series of three cases of this disease occurring in children of one family, and affecting all three within a period of a few weeks, brought the contagiousness of the eruption so clearly before me that it occurred to me to seek a cause for it. I accordingly examined the sebaceous pellet which forms the pulp of the little tumours, with a view to discovering, if possible, the presence of a parasite; although, so far as I am aware, a parasitic nature has never been imputed to this disease.

It will be anticipated that I met at the outset with a very formidable obstacle in the well-known strong refraction of light which is characteristic of parasitic germs, and which renders it difficult to distinguish them from minute oil-globules; and that this peculiarity of fungus-spores would make it by no means easy to search for them in a sebaceous mass of which minute oil-globules are a prevailing constituent. In the diseases of the skin, the parasitic nature of which has been hitherto ascertained (favus, tinea tonsurans, and chloasma), this difficulty does not present itself. It is, therefore, easy in examining the structures (the hair and the epidermis) that are invaded by the parasites proper to these diseases, to determine the presence or absence of the fungi that are known to infest these tissues.

The difficulty which presented itself I, however, contrived to get rid of—as I think, very effectually—by digesting the sebaceous mass for many

hours in ether, a process by which the mass was very considerably shrivelled. I then discovered under the microscope what I believe to have been most decided parasitic spores, to which I will not venture to give a botanical name. These spores, if my recollection serves me, were somewhat larger than those of the parasite of favus (*Microsporon Audouini*). I distinguished them clearly from oil-globules by a feature which I had found to be very characteristic of the large spores of the *Microsporon Audouini*, namely, that (unlike oil-globules) under a high power (an eighth) their apparently rounded outline, as seen under a quarter-inch power, discloses itself as a faceted (decidedly angular) outline. By this I do not mean that they have sharp angles, but rounded (as if slightly worn) angles. I did not find any mycelium; but, as regards this, I may add that I have never been able to make out any unexceptionable evidence of mycelium in hairs infested, even abundantly, with the spores of the trichophyton tonsurans, although it is plain enough in the *Microsporon Audouini*, and also in the *Microsporon furfur*. I ought to mention, that a distinct oval nucleus, similar to what is presented under a high power by the spores of the *Microsporon Audouini*, was plainly perceptible also in the fungus that I am imputing to the molluscum contagiosum.

There is another matter which arises from what I have put forth, and which ought not to be omitted: molluscum contagiosum is a disease which presents very anomalous and peculiar features. In no other disease of the skin do sebaceous collections increase with the surprising rapidity that is characteristic of them in this disease. In acne, in non-contagious molluscum, in sebaceous wens, the growth of the sebaceous collection itself is remarkably tardy. But the presence of a parasite will afford the requisite explanation of the difference. If, as I believe, the parasite which I have described be the special vegetable parasite of the sebaceous glands, just as the parasite present in chloasma is the parasite proper of the epidermis, and again as the parasites of favus and of tinea tonsurans are the special parasites of the hairs,—it becomes then no more difficult to understand a rapid formation of sebaceous matter provoked under the irritation of the sebaceous glands set up by the presence of a parasite, than it is to comprehend the rapid formation of scurfy epidermis produced by the irritation of the surface of the cutis by the parasite present in chloasma.

I did not publish my investigation at the time when I made it, being then busy with clinical photography; but I think the research, over which I took some pains, is worth recording, if only to serve as a hint for some more able investigator.

THE USE OF BATHS IN SMALL-POX.

By CHRISTIAN BAEUMLER, M.D., Assistant-Physician to the German Hospital.

LOOKING upon hospitals as our guides in therapeutical matters, I am sure that other members of our profession besides myself must have felt some disappointment at the uncertainty of opinion regarding the above subject, which the medical evidence in the Hampstead Hospital inquiry has brought to light. The statements which were made were not characterised by that confidence which is founded on an exact knowledge whether and when a remedy is suitable or even necessary, or when it is contraindicated.

At the present day, no more doubt or uncertainty ought to exist on so important a point as this; and if it would be unreasonable to expect from any hospital accurate clinical observations on such a question at a time when an epidemic is rapidly spreading or at its height, and when necessarily all the resources of the hospital are strained to the utmost, yet the opportunity to settle this question ought not to be allowed to pass while the epidemic is declining and the hospital staffs are less overworked; for I am afraid we are still very far from the much-desired end of stamping out small-pox altogether, nor is the mortality from it so reduced as to let us lay down our hands in complacency and as to make unnecessary any further endeavours to find out means for reducing it still more.

The beneficial results which follow the use of cooling baths in other fevers can only encourage an attempt in the same direction with small-pox. Any one who has seen how the restlessness of a patient suffering from the initiatory fever of scarlatina, or in any other fever, is quieted by, and how sleep follows upon, a cool bath, cannot help thinking that the same effect would be produced in small-pox whenever the restlessness and delirium are accompanied by high pyrexia, and that, consequently, mechanical restraint would much more rarely become necessary. That the pyrexia had something to do with the delirium of those patients that had to be restrained at Hampstead, I conclude from the fact that this restraint has, in almost all the cases, only been found