may therefore become so slow in the particular part as to allow of the formation of a small coagulum of fibrin or a thrombosis; there would then be complete obstruction, and consequently the greatest possible pressure would be brought to bear on the arterial wall, and result very probably in rupture. In this way not only may cerebral apoplexy be accounted for, but pulmonary apoplexy and apoplexies in the kidneys and spleen, or hæmorrhagic infarctions, occurring in cases where no endocardiac or valvular mischief has existed. The same explanation may be offered with respect to oedema of the integument and dropsy, as is given for oedema of the lungs. A characteristic of the oedema in Bright's disease is that it changes its position, now attacking one organ, now another. How is this? And why does the morbidly altered blood cause contraction of the minute arteries; is it by direct contact with the vessel, or is this the result of the action of the blood on the ganglionic cells of the nervous system? These are interesting questions still requiring explanation.

THE PATHOLOGY OF BRIGHT'S DISEASE.

By GEORGE JOHNSON, M.D., F.R.S.,

Physician to King's College Hospital; Professor of Medicine in King's College; etc.

SIR WM. GULL and Dr. Sutton, in the short note which appears in the last number of the JOURNAL (p. 654), state that the vessels from the pia mater of the sheep in which they recognised the hyaline thickening of the tunica adventitia had been acted on by acetic acid. In this statement they have inadvertently erred. The vessels in question had been immersed only in a mixture of equal parts of glycerine and diluted naphtha (1 part naphtha to 9 of water). I have specimens in which glycerine alone, and others in which equal parts of water and glycerine, may be seen to have produced precisely the same hyaline thickening of the tunica adventitia in perfectly normal arteries. In each of these cases, the glycerine is, without doubt, the agent which effects the hyaline change in the fibrous tunic. It will be remembered that the specimens of Sir Wm. Gull and Dr. Sutton are mounted in a mixture of glycerine and camphor-water.

I have referred to the action of dilute acetic acid, only to show the extreme rapidity with which it produces a hyaline swelling of the adventitious tunic, not distinguishable from that which is more gradually effected by glycerine. At the *conferenza* of the Royal Medical and Chirurgical Society, I showed a specimen thus acted on by acetic acid with glycerine, and another by a mixture of glycerine and naphtha-water; the two specimens having precisely the same appearance. The fact that this appearance of the tunica adventitia can be produced by various fluids acting upon quite normal arteries, is scarcely consistent with the theory of "arterio-capillary fibrosis."

Without a detailed history of each case and of each specimen, I cannot undertake to say why it has happened that Sir Wm. Gull and Dr. Sutton have found that the vessels from certain subjects presented none of these changes, although prepared in the same way. The imbibition of fluid by the fibrous tunic, upon which the appearances in question depend, is doubtless influenced by various physical conditions not easily determined. It was stated in the paper that the changes which the authors describe are often very partial; the trunk of a vessel may be affected while the branches are free. So when the vessels from a healthy animal have been acted on by glycerine, some arteries may be seen quite unchanged in the midst of others conspicuously hyaline.

That Sir Wm. Gull and Dr. Sutton have met with these hyaline changes in healthy arteries, is a reasonable inference from their statement that they have found them unassociated with renal disease. Upon this observation they chiefly base their doctrine that a general arterio-capillary degeneration precedes the local disease in the kidney. But it is obvious that, if my interpretation of the "hyaline fibroid" appearance is correct, this affords no evidence of a pathological change in the blood-vessels preceding the renal degeneration.

Dr. Moxon reports, in the last number of the JOURNAL (p. 637), a case of extreme granular degeneration of the kidneys without hypertrophy of the heart; and he maintains that the occurrence of such a case is inconsistent with the view that the cardiac hypertrophy, which is so commonly associated with the contracted kidneys, is a result of the blood-changes and the consequent impediment to the circulation induced by the renal disease. I confess that I fail to see the force of this argument. By a similar process of reasoning, if a case of Bright's disease with a large white kidney terminate fatally, as such cases sometimes do, without the occurrence of dropsy at any period of the history, we might conclude that the dropsy which is frequently associated with this form of disease is not a result of the blood-changes induced by

degeneration of the kidney, but a consequence of some other unknown cause usually, but not constantly, associated with the renal disease.

It is an acknowledged fact that, while the hypertrophy of the left ventricle is found, as a rule, in all cases of contracted kidney, it is less constantly, though not seldom, associated with the large white kidney. Why is this? In the first place, it is a fact that the hypertrophy of the left ventricle is always associated with hypertrophy of all the systemic arterioles. With the contracted kidney, there is, as a rule, a free secretion of water with retention of urinary solids; and this uræmic condition is, in various ways, the cause of death. On the other hand, in cases of large white kidney, there is a scanty secretion of water by the kidneys, and, in consequence, a state of hydræmia and dropsy. In the first class of cases, the urinary solids accumulating in the blood probably excite the contraction of the systemic arterioles, and thus indirectly provoke the hypertrophy of the left ventricle. In the second class of cases, while there is some retention of urinary solids, there is a disproportionate accumulation of water, by which the contaminated blood is diluted, and thus rendered less irritating to the minute arteries. Hence it happens that the arterial resistance is less, and the cardiac hypertrophy less constant and pronounced in cases of large white kidney with consequent hydræmia, than in cases of contracted kidney with resulting uræmia.

THE ANATOMY OF BRIGHT'S DISEASE.

By PAUL W. SWAIN, F.R.C.S., Stoke, Devonport.

I WAS much interested in the paper and discussion on Sir W. Gull's views regarding the morbid changes produced by Bright's disease. I have had the good fortune to inspect carefully the specimens of hypertrophied capillaries shown by Dr. G. Johnson; and, as I have been in the habit of using one of Powell and Leland's best microscopes since the year 1844, I may lay claim to some experience in judging of the truth and value of microscopic demonstrations. I therefore trust that a few observations may not be unacceptable to many who may not have had the same opportunities of studying minute pathological preparations.

The action of the arterioles and capillaries causes the intermittent stream of blood, as delivered from the heart into the large pulsating arteries, to become a steady uniform current through the minute channels, as may be seen in the web of the frog's foot. The power thus possessed by the small vessels is derived from their muscular elastic coat. This power may be well illustrated by a reference to the India-rubber instrument used in producing the æther-spray. The action of the hand is analogous to the contractions and relaxations of the heart; and the action of the second elastic bulb resembles the effect of the capillaries in causing the current of the spray to be uniform and continuous. Of course, the elasticity of the air must be taken into account in the production of the spray; but the analogy is sufficiently complete for illustration.

Now, I do not mean for a moment to set up ingenuity of theory against absolute and undeniable facts; but I do say that, if we accept Sir William Gull's idea of a fibro-hyalin deposit, substituting an inert obstructive material for an active conservative force, we demolish what appears to me one of the most beautiful illustrations of design which, to my understanding, appears to rest on evidence as sound in logic as it is in fact. The morbid changes and their causes may be briefly summarised.

The kidney is no longer able to filter the blood and withdraw from it the nitrogenous and deleterious matters. When it reaches the capillaries thus deteriorated, it irritates the minute nerve-fibres spread over their internal lining; reflex action causes the muscular envelope to contract, thus reducing the calibre of the tube; the heart insists on forcing on the circulation, and thus at each end of the chain increased muscular action is induced, gradually but certainly causing muscular hypertrophy. To this capillary resistance Dr. George Johnson has very appropriately applied the term "stopcock" action; and there can be no doubt that the whole process is one of those wonderful efforts of Nature to make the best of a bad matter, and thus to prolong the life of the individual by setting up what is actually an abnormal proceeding. In the present subject of discussion, the question has been confined to instances selected from Bright's disease, in which it is probable that the starting cause of the muscular hypertrophy would be in the capillaries. But there are conditions of hypertrophy of the left ventricle which may be called endocardiac, and which are known to begin in the heart itself. Dr. Houghton's examinations of the enormous heart of the greyhound Master Magrath, and his reference to the size of the heart of Eclipse, led him to suggest that hypertrophy of this organ might not be always absolutely a morbid condition, but be produced, like the blacksmith's arm, by increased action, and actually having for its aim the adjusting the organ

to meet the demands required for speed of pace in animals used for racing. It has been stated that certain gymnastic exercises of the arm in drilling soldiers lead to hypertrophy of the heart. Now, it would be interesting to inquire if in such cases as the above the capillaries become more muscular, and that in proportion to the size of the heart. If this question could be answered in the affirmative, it would at once settle the value of Sir W. Gull's theory of inert "fibro-hyalin" matter; though even now, I believe that Dr. George Johnson's explanation of it, as a microscopic illusion, will be found to be correct. The beauty of Dr. Johnson's preparations is deeply impressed on my memory, as I had two distinct opportunities of examining them, once assisted by himself, and I can vouch for the accuracy of the engravings given with a paper referred to by Dr. Johnson, April 16th, 1870, in the BRITISH MEDICAL JOURNAL.

ON THE TREATMENT OF SMALL-POX BY VACCINATION AND INJECTION OF LYMPH.

By ROBERT GRIEVE, M.D., Hampstead Hospital.

MR. FURLEY, in the JOURNAL of the 8th instant, while describing some instances of small-pox treated by vaccination and the injection of lymph, takes occasion to criticise the cases published in the JOURNAL, in which vaccination in small-pox was tried at the Hampstead Hospital. There are points in his paper upon which I wish to make a few remarks. He differs from the opinion of Mr. Marson, and also that held by me, that the influence of vaccinia over variola when concurrent is *nil*, unless the former have been induced within the first five days of the period of incubation of the latter. I may state that my belief on this point is formed after observation of a very considerable number of cases of these diseases when co-existent. I have a difficulty in reconciling Mr. Furley's language on this point with what he has stated in another journal, where he says that he has found vaccination in small-pox in adults *almost inoperative*. It is well known to all accustomed to the treatment of small-pox, that vaccinated children very rarely, almost never, have that disease severely. Will Mr. Furley inform us upon how many unvaccinated children he has tried the cure by vaccination, and with what results? Mr. Furley asks: "Is it Dr. Grieve's experience for one-third of his cases to abort?" In vaccinated subjects, it is my experience that three out of four abort—that is to say, run a highly modified course, and that this proportion is even much larger amongst those who have four or more good cicatrices of vaccination, as it is less in those imperfectly protected. In unvaccinated subjects, the disease very rarely indeed aborts; but of the six, here five were vaccinated, so that under ordinary circumstances three ought to have aborted. Although this has been my experience of small-pox, I do not agree with Mr. Furley that necessarily it is a peculiarly happy one, for I think it will be found to be the experience of many others. I may remind Mr. Furley that three out of the six died, our general mortality is not fifty per cent., and, as he says, there are no cases without a lesson; what lesson does he draw from this? He falls into an evident error as to the period of the disease at which vaccination was tried; he says they were not vaccinated until the fifth and sixth days, evidently thinking it was at that stage of the eruption; while it was the second and third days of the eruption, or the fifth and sixth days of the disease. I saw nothing in the progress of these cases to cause me to alter the opinion that vaccination after the appearance of the small-pox rash is utterly useless. In order to give a full trial to the treatment, seven more cases were operated upon by the latest of Mr. Furley's methods—namely, the hypodermic injection of lymph in quantity. I take the opportunity of expressing the belief formed upon the results in these cases, that not only is its curative action absolutely nothing, but that in a certain number of instances most serious consequences have resulted from its use. Two only of those cases are as yet completed, both by death. One was from *variola hæmorrhagica* uninfluenced by the operation; the other had severe small-pox with bronchitis in the earlier stages; afterwards, erysipelatos inflammation of the arm operated upon set in, resulting in supuration and sloughing of the cellular tissue. Diarrhoea came on, and convulsions shortly before his death. Another man has also had severe inflammation and supuration of the arm, with serious constitutional symptoms; and in three others, abscesses either have formed or are forming in various parts of the body. These results are not such as to encourage me to continue the practice, but rather have determined me to discourage it by all means in my power.

I refrain from criticising Mr. Furley's cases, but I ask him to publish, not as he has done, a selected cure or two, but the history of every patient treated by him, with full particulars of their ages and

condition as to primary vaccination. In the absence of such details it is impossible to form any reliable opinion as to the success or non-success of his plan of treatment. I am afraid this is but another instance in which a lucky succession of cases of variola curta has led to the belief that at last has been found the long looked for specific for small-pox.

CASE OF IMPALEMENT ON A BRUSH-HANDLE, WITH PERFORATION OF THE DIAPHRAGM.

By JONATHAN HUTCHINSON, F.R.C.S.,
Senior Surgeon to the London Hospital.

ON the afternoon of Saturday, May 13th, 1871, I saw, in consultation with Dr. Brereton, of Old Ford, the subject of the following narrative.

Mrs. W., aged 27, had on the morning of the same day received a severe injury from a fall backwards on her own stairs. It was believed that she had put her foot on a bobbin which one of her children had left on one of the steps, and which caused her to fall suddenly and with great force backwards in a sitting posture. In her fall she felt something enter her body near the anus, and on recovering herself she found the lower part of the handle of an ordinary parlour hearth brush projecting close to the bowel. With great courage and resolution she extracted the whole of this without assistance, although she had, as she said, to change her hand twice on account of its length. Having extracted it she felt faint, and sat on the stair until her servant came and helped her upstairs to bed. She did not faint away, and she told me afterwards that she had at the time no very severe pain, but only considerable difficulty of breathing. Dr. Brereton saw her within half-an-hour of the occurrence. He found that her clothes had not been anywhere perforated by the brush-handle; there was a punctured wound close to her rectum, and no evidence of hæmorrhage going on. He estimated the quantity of blood which had been lost at about half a pint, for her clothes were in part saturated. The shaft of the broom was smeared with feces almost up to the part where it had broken off close to the broom-head. Thus it appeared certain that a length of at least sixteen inches had entered her body.

When I saw Mrs. W. in the afternoon, about eight hours after the accident, she was in bed, looking very pale, but not complaining of any great pain. She allowed me to examine her abdomen pretty freely, and, although tender in parts, it was remarkably little so. I found her right chest quite tympanitic, and was obliged to infer that the diaphragm and lung had probably been wounded. Although perfectly conscious, and with a countenance but little expressive of anxiety, she had no pulse at the wrist. The laceration by which the broom had entered was on the left side of the bowel, and would admit two fingers. There was no bleeding whatever, and there did not appear to be much to be done in the way of treatment. Death took place in the following night, about twenty hours after the accident.

At the *post mortem* examination we found that the track taken had been first through the skin, and then into the rectum an inch or two above the anus, and thence through its anterior wall just behind the uterus, into the abdominal cavity, after traversing which the implement had passed through the gall-bladder and liver, through the diaphragm, and had entered the lung. The pleural cavity contained a considerable quantity of air with some blood and serous effusion. The peritoneum was everywhere greasy with recent lymph, and contained probably about a pint of clotted blood mixed with bile and small portions of fecal matter.

The appended woodcut shows the form of the implement with which this fearful injury was inflicted.

I was puzzled at first to explain how it could have gained access to the body without damaging in the least the clothes. Dr. Brereton suggested, and probably correctly, that the brush was lying on the stairs with its handle pointing upwards, and that the woman's petticoats caught its tip and tilted it under them just before she sat down on it. As an illustration of a very unusual kind of accident, the case is of some interest. Nor is it wholly without medico-legal value as an instance of what it is possible for a fearfully wounded person to accomplish. This poor lady actually drew out of her body a weapon eighteen inches long, the point of which was in her lung, and which had to be extracted through her diaphragm, liver, gall-bladder, and the whole