

performed in London with the hope that other operators may be induced to record their experience.

TABLE II.—*Hysterectomy for Fibroids complicating Pregnancy and Labour.*

Operator.	Month of pregnancy.	Result.	Reference.
Cripps, H.	Fourth.	Recovered.	Museum of St. Bartholomew's Hospital.
Clark	Fourth.	Recovered.	Middlesex Hospital Reports, 1897, p. 218.
Bland-Sutton	Third.	Recovered.	Clinical Journal, vol. xi., p. 305.
Bland-Sutton	Second.	Recovered.	Ibid., vol. xvi., p. 404.
Gow	Eighth.	Recovered.	Transactions of the Obstetrical Society of London, vol. xxxix., p. 7.
Galabin	Sixth.	Recovered.	Ibid., vol. xlii., p. 258.
Horrocks	Fifth.	Recovered.	Ibid., „ „ p. 242.
Routh	Ninth.	Recovered.	Ibid., „ „ p. 244.
Spencer	Ninth.	Recovered.	Ibid., vol. xxxvii.
Duncan	Second.	Died.	Middlesex Hospital Reports, 1897, p. 182.
Duncan	Fourth.	Recovered.	THE LANCET, March 3rd, 1900, p. 613.

It cannot to my mind be too strongly set forth that in cases where a fibroid has obstructed labour and has been successfully "pushed up," as the phrase goes, if any dangerous symptoms supervene during the puerperium then coeliotomy, followed by myomectomy (or hysterectomy if the need be), should be carried out without delay. That myomectomy and enucleation are successful during the puerperium I have already testified in this lecture (Table I.).

I am well aware that in a very large number of instances fibroids and pregnancy coexist and no harm ensues, for though the tumour occupies the pelvis in the early stages it is so much "part and parcel" of the uterus that, as the enlarging organ rises out of the pelvis, it carries the fibroids with it. In the case of an incarcerated ovarian tumour it is different, for the more the fundus of the uterus ascends the more the ovarian pedicle elongates and the more perfect the incarceration becomes.

From a very broad survey of this question I have arrived at the following deduction: ovarian tumours have given more trouble to pregnant and parturient women than fibroids, but fibroids have been far more lethal as they so frequently destroy puerperal women from sepsis. The whole subject is an instructive study of the baleful effects which environment often imposes on so-called innocent tumours.

REMARKS ON AGGLUTINATION BY PLAGUE BLOOD.

By E. KLEIN, M.D., F.R.S.

VARIOUS observers have stated that the blood of persons convalescent from plague possesses the power of agglutinating plague bacilli of culture. To mention a few only of these observers, the late Dr. Leumann in several reports of the Bombay Plague Laboratory found that the blood not only of persons convalescent from an attack of plague, but also of those who had been injected with the Haffkine plague prophylactic, possesses marked agglutinating action; Zabolotny,¹ as also other observers, speaks of a similar agglutinating power of the blood of plague convalescents; and Dr. A. K. Chalmers, at the meeting of the Incorporated Society of Medical Officers of Health on Nov. 10th, 1900, mentioned that in Glasgow this agglutinating action of the blood of plague convalescents was also ascertained.

Now, without in the slightest degree intending to cast any doubt whatever on the correctness of these observations and statements, it may be permissible to point out that

unless the method according to which the experiments were made is mentioned in detail the nature of these observations is not easy of comprehension. All those who have worked on specific agglutinations and all those who are constantly using this most valuable diagnostic method—to wit, in typhoid fever—are agreed on this, that all agglutination experiments must start with a good emulsion of the microbes on which the agglutinating action of blood has to be tested. Unless a good emulsion is used in which the bacilli are isolated and evenly distributed no conclusion can be drawn; indeed, no reliable experiment at all can be started. If, for instance, a distribution of the bacilli is employed in which already at starting the bacilli are aggregated in larger and smaller clumps, the experiment of testing the agglutinating action of blood, serum, or any other fluid on such an emulsion can be of little value. It is well known that of the typhoid bacilli, cholera vibrios, and other microbes a good emulsion—i.e., a uniform distribution of the microbes either as a broth culture, or as a bouillon, or salt emulsion made from the growth on agar or gelatin—can easily be prepared, and therefore no difficulty is experienced or apprehended in starting without further precautions the required agglutination experiments. But this, unfortunately, is not the case with the plague bacilli. The plague bacilli in a broth culture form naturally at the sides and the bottom of the broth granular and floccular aggregations—masses of chains of bacilli intimately bound together—the main part of the broth remaining clear. However much such a broth culture is shaken there are always present, when a droplet is examined under the microscope, a sufficient number of clumps to vitiate all further proceeding in the agglutination test. On agar, glycerine-agar, or serum the plague bacilli form a characteristic sticky viscid growth, due to the fact that the bacilli produce on these media a gelatinous, sticky, interstitial substance, by which the bacilli remain coherent. If a particle of such a growth is placed in bouillon no amount of shaking can produce a workable emulsion, microscopic specimens show numerous clumps and coherent masses, and therefore no reliable test can be made with such a distribution. This will be sufficient to show that to perform the agglutination test with plague blood on plague culture the initial desideratum of having a good emulsion of plague bacilli meets with great difficulties. These were the difficulties I met with in trying to repeat the observations recorded by the above-named authors, for, unfortunately, the method of preparing the required emulsion was not stated by them. A positive agglutination test of plague bacillus might not only be of theoretical value, but, as must be obvious, would possess great practical importance in those cases of convalescent persons in whom the bacteriological or other evidence did not directly demonstrate their illness to have been plague.

It was known to me that gelatin surface cultures of plague bacilli do not possess the viscosity and stickiness characteristic of agar or serum cultures; they appear of a drier character, and therefore I tried to obtain a workable emulsion from such a gelatin culture. Consequently a particle of the growth was distributed in bouillon and by shaking this it was attempted to produce a workable emulsion. But this was of no avail; the bacilli, although in many places in an isolated condition or in small masses, remained sufficiently numerous in bigger clumps, and therefore made this distribution unfit and unsatisfactory for a reliable experiment.

It occurred to me, however, that by distributing the gelatin growth in physiological salt solution—a fluid otherwise known to be capable of dissolving interstitial (globulin) substances, it might be possible to obtain a good and workable emulsion. The experiment proved this contention to be correct. There was no difficulty in making a good emulsion by shaking up a particle of the gelatin growth in ordinary (0.75 per cent.) physiological salt solution. A drop of this emulsion examined under the microscope showed, indeed, the bacilli uniformly distributed and practically in isolated condition; at any rate, there were no obnoxious or vitiating clumps. And such a microscopic specimen shows (sealed up) even after being kept for 24 hours, the same isolated condition of the bacilli. The first and important difficulty of having a good emulsion of plague bacilli was thus overcome. It was now possible to proceed to test on such an emulsion the agglutinating action of plague blood or of any other substance.

I had already from a number of previous indications the

¹ Archives des Sciences Biologiques de St. Pétersbourg, vol. viii., N. 1, referred to in the Centralblatt für Bakteriologie und Parasitenk., Band xxviii., N. 25, p. 381.

suspicion that bouillon *per se* is not without effect in causing agglutination of plague bacilli, and the first experiments were therefore made with sterile alkaline bouillon. The result was sufficiently striking. If to a good salt emulsion of plague bacilli (taken from the gelatin surface) bouillon is added in the proportion of 1 of bouillon to 20 of emulsion the result of positive agglutination is evident already in from 12 to 15 minutes. Instead of the uniform distribution of more or less single bacilli there is now everywhere a general massing of the bacilli into larger and smaller clumps and a corresponding disappearance of the previous single bacilli. Even when the dilution is 1 in 40 the same clumping is noticeable, though somewhat later (in from 15 to 30 minutes). From this it is evident that for the performance of the agglutination experiment with plague bacilli the method of making an emulsion of the microbe in bouillon generally used for other bacilli—e.g., typhoid and cholera—is not applicable.

The next experiments were made with normal human blood and with normal mouse's blood. In all instances a good salt emulsion of plague bacilli from a gelatin culture was made and to this the blood was added in the proportion of 1 in 20 and 1 in 40. Not even after 24 hours was there any difference noticeable between the control (emulsion) specimen and the specimen made with emulsion *plus* blood.

The next experiments were made with blood of rats which had been injected first with Haffkine's prophylactic, then with small doses and finally with large doses of living plague culture. These latter injections produced slight bubo and general illness, but after a few days the animals recovered, whereas the control (normal) rats injected at the same time and with the same amount of the same living culture succumbed. The blood of two such rats which had recovered was used for the agglutination test, three and five weeks respectively after the last injection. The proportion of blood added to the salt emulsion of plague bacilli was 1 in 20 and 1 in 40 and the result was distinct agglutination in 10 minutes; by the end of 15 minutes or so all bacilli had clumped into smaller and larger clumps, leaving the intervening parts practically free of isolated bacilli. Such rats' blood produced not only sooner the agglutination but the clumps were larger than in the case of the bouillon experiment. From this, I think, it follows that the blood of the rat, convalescent from plague, has positive agglutinating action on an emulsion of plague bacilli. Whether tested by this method also the blood of a human being convalescent from a mild attack of plague or after infection with Haffkine prophylactic has, as might be expected, this agglutinating action, I am from my own experience unable to say.

St. Bartholomew's Hospital.

WHY ARE BOTH LEGS OF THE SAME LENGTH?

By GEORGE E. WHERRY, M.C. CANTAB., F.R.C.S. ENG.

THE poet Pope must have been easily satisfied when he wrote the lines—

"Why has not man a microscopic eye?
For this plain reason—man is not a fly."

Here we are reminded of the tailor who thought that man had two legs in order that he might wear breeches. In this new century we are more persistent in our inquiries, less satisfied with our answers, and some of us even wish to interfere or try to mend. For these body-cobblers all simple mechanical influences become of the utmost importance as being to some extent under their control. Man cannot meddle with the mechanism of the sun, and we have therefore the more confidence in daylight; nevertheless we may watch the effects of great laws, as of symmetry or heredity, and help ourselves by the study.

The general question whether "a man by taking thought can add one cubit to his stature" is not now much to be considered, but rather whether a child has chances of gaining length of limbs apart from inherited potentiality. Though much would depend on heredity, something would depend on conditions during growth. The Eton boy is taller than the workhouse boy of the same age; and

Frederick William's tall guardsmen with selected tall wives may still stamp the present generation with a tall progeny. Grogan found a long-legged people living on the Nile sudd who may have developed from mudlarks to storks by the unusual conditions of their life for generations, and I am prepared to believe that individuals and peoples may be stunted in stature by weight-carrying and over-walking in youth.

As a fact, our lower limbs are not usually both of the same length, though they are so for all practical purposes. There are slight differences in length, usually an excess in the left, which does not influence the gait or in any way limit the usefulness of the legs in progression. That the lower limbs are longer than the upper is an advantage; but it is interesting to note that they are not born so, and the short helpless lower limbs of the infant are rather prehensile than ambulatory. With regard to the upper limbs of the adult these prehensile organs are shorter and smaller than the lower limbs, and the right arm is almost always longer than the left. The right humerus and radius taken together will be from one-third to three-fourths of an inch longer than the left. This is not so at birth, but becomes so during growth, and the increased development is probably associated with the use of the right arm and hand. In the gorilla and anthropoid apes, the adult skeletons which I have measured in the Cambridge museum show no such difference. It interests me to note that the right arm of man gains on the left not only in size and strength but also in length by excessive use. The gorilla, having no need to use one limb more than the other, is ambidextrous or ambisinistrous. A climbing monkey has been seen to swing from bar to bar across a space of 20 feet and in the transit to catch a bird in the air with wonderful precision, assisting itself with five prehensile organs—four limbs and a tail.

The length of the arms in the anthropoid apes is very striking and one would like to train a baby gorilla to stand and walk upon its legs expecting that it would grow longer in the legs than other gorillas. It may be if a child were trained to walk upon its hands, and the feet were used like those of the Brussels painter with prehensile toes, the arms might then grow as long and as strong as the legs. About legginess generally it may be stated that we are not born leggy like the foal or the kangaroo, but we gradually achieve legginess. At about 15 years of age the human boy shows what he can do in that direction and develops this strikingly human characteristic. What an extraordinary difference in this aspect from the foal, who has to follow the mare about the field, and is born with limbs well adapted for that purpose.

The kangaroo is an example of an animal with disproportionately long hind limbs. In the development of this creature after an intra-uterine life of less than six weeks the mother by means of its mouth then takes from the womb the tiny one-inch foetus and places it inside the pouch, where the foetus finds, and hangs on to, the teat of the mother's mammary gland; by the time the baby kangaroo leaves this second uterus, in about nine months, its hind limbs have acquired their adult proportions and become serviceable for hopping about. After this the infant kangaroo occasionally returns for food and shelter to the marsupial pouch always to its own teat though younger infants are in possession of other teats.

In organs which grow continuously through life it is necessary for healthy development that natural resistances should be continually offered to the growing organ. Without this opposition the individual suffers, as in the case of the rat when, a tooth being lost, the unopposed tooth tacks the jaws together or seriously hampers the bite by its increased length.

With regard to mechanical causes which influence growth it is difficult to avoid the speculation that the direction of a spiral often depends upon some outside resistance so applied as to determine a right or left helix, even if the spiral itself be not so made. That this direction is so determined is made probable by considering a carpenter's shaving. The reason of the spiral form of a right-handed screw which is usually seen in shavings is because the workman tends to drive his plane slightly to the left. He plays, as a cricketer would say "a little to the on"; if he made a perfect stroke fore and aft a simple coil would result; in "playing to the off" or standing on the wrong side of the bench he would make a left-handed twist in his shaving. The carpenter is usually all unconscious of the twist that he makes and