BRITISH MEDICAL JOURNAL

LONDON SATURDAY JUNE 3 1944

SOME PROBLEMS OF INFECTIVE HEPATITIS

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We might begin by asking ourselves what justification there is for regarding infective hepatitis as an infection at all, or for using this name in place of synonyms such as simple jaundice, catarrhal jaundice, or epidemic jaundice. Certainly none of Koch's postulates has been fulfilled, and the history of nutritional deficiencies such as ergotism and scurvy shows that epidemicity is no criterion of infection. There is, however, the well-authenticated fact that people often go down with infective hepatitis approximately a month after contact with a case—often quite a brief contact; and the transmission experiments of Voegt (1942) and Cameron (1943) are hard to explain except by some sort of virus. Nevertheless, although infective hepatitis seems to be an infection, it has not been possible to transmit it to any experimental animal in spite of numerous attempts and modifications. There are no characteristic histological lesions such as inclusion bodies, and tissue culture has given no assistance. We are unable to recognize the virus except by human transmission, and there is no laboratory test for the pre-icteric or subclinical forms of the disease. Compare the position with Weil's disease, in which there is a visible organism, an animal host, and a sensitive serological test, and in which, as a result, the frequency of non-icteric and subclinical infection has been revealed, and it will be seen under what a disadvantage we labour.

It is important to consider this question whether "catarrhal jaundice" occurring sporadically or epidemically is an infection, because jaundice sometimes occurs after the injection of human serum or plasma, or during the course of treatment of syphilis with organic compounds of arsenic. The symptoms, biochemical alterations, and morbid anatomy of this homologous serum jaundice and of post-arsenical jaundice are very similar to those of infective hepatitis. In all three illnesses there is an acute hepatitis and the microscopical picture of the liver is identical. (In parenthesis it may be added that jaundice proved to be due to catarrh of the duodenum or bile ducts is excessively rare.) Nevertheless, it is doubtful whether homologous serum jaundice and post-arsenical jaundice are infectious or contagious, or transmissible in any way except by deliberate inoculation.

The incubation period of infective hepatitis is approximately 30 days, while that of homologous serum jaundice and post-arsenical jaundice is usually 60 to 90 days. It is possible that homologous serum jaundice and post-arsenical jaundice are one and the same disease, small quantities of serum being passed from one patient to another in the syringes used for the injection of the arsenic. The incidence of post-arsenical jaundice can be greatly reduced by scrupulous precautions to prevent transfer of material from patient to patient (Salaman,

1944). It seems improbable that the virus of infective hepatitis is the factor which is transmitted in inoculation jaundice. A virus should not require a longer period of incubation with injection than with natural infection. Moreover, homologous serum jaundice does not protect the individual from contracting infective hepatitis; in fact, it appears to increase his susceptibility to this disease (Gordon, personal communication, 1944). Although second attacks of infective hepatitis do occur, they are uncommon enough to suggest that an attack of infective hepatitis itself is usually followed by the development of immunity. Finally, there is no evidence that patients with homologous serum jaundice or post-arsenical jaundice ever infect their neighbours. This is not for want of opportunity, because both diseases have occurred on an unprecedented scale during this war. On the other hand, homologous serum jaundice can be transmitted to further subjects by inoculation, both during the incubation period and for at least a week after the onset of the jaundice (Oliphant, Gilliam, and Larson, 1943). These observations suggest that infective hepatitis is a different disease from homologous serum jaundice or post-arsenical jaundice, but they are not conclusive.

Obviously infective hepatitis is not a disease one would choose to study if one was out for quick results. Unfortunately it has proved one of the three most important diseases in the Mediterranean theatre of operations, the others being malaria and venereal disease. Disturbing features of infective hepatitis are the relatively long stay in hospital, the absence of any method of prophylaxis, and the steady rise in incidence in the four successive autumns of the war. On occasion it has interfered with military operations, and it has disproportionately affected key personnel such as officers and air crews. Coming at a time of year when malaria and battle casualties are also likely to be frequent, it imposes a heavy strain on medical facilities. The military authorities may well wonder what the incidence is likely to be next season, and public-health administrators are troubled by the risk of epidemics among the civilian population.

Clinical Features

Infective hepatitis is characterized by a pre-icteric and an icteric stage. During the epidemics abroad a surprisingly high proportion of the patients have been admitted to hospital during the pre-icteric stage. The symptoms in this stage are quite unlike the picture of "catarrhal jaundice" which many of us had formed. The onset of the illness may be sudden, with high fever. Respiratory catarrh occurs in a small proportion of cases, though it is not certain whether this is part of the symptomatology of infective hepatitis or is due to a coexistent respiratory disorder. In other cases adenopathy is a feature, and the blood picture in the early stage may resemble that of glandular fever. More commonly, anorexia and alimentary symptoms are in the foreground. The dissimilarity between our mental image of infective hepatitis and its real appearance may have two effects: it may cause the observer abroad to

wonder whether the disease he is seeing is the same as occurs at home; or he may be tempted to try to classify infective hepatitis into different types. I shall not anticipate unpublished work further than to say that there is little doubt that it is all one and the same disease.

A major difficulty in the pre-icteric stage is the differentiation from clinical malaria—that is to say, an attack of malaria in which the parasites are found only with difficulty and after a prolonged search. There is sometimes a lull between the pre-icteric and icteric stages of infective hepatitis, and the patient is then apt to be discharged from hospital with the diagnosis of clinical malaria, only to return a few days later with jaundice. In general, anorexia suggests infective hepatitis and splenomegaly suggests malaria, but it is impossible to make the differentiation with certainty at the bedside, and a simple laboratory test for infective hepatitis would be an immense help. A further difficulty lies in the fact that a patient may suffer simultaneously from malaria and infective hepatitis, as is only to be expected from their frequency and their occurrence at the same time of year. Finally, jaundice may occur as a complication of malaria itself when the blood or the liver is damaged by the plasmodium. For all these reasons it is wise to play for safety and to treat the patient for malaria when the diagnosis is in doubt. The importance of early treatment of malaria is great. Many pre-icteric cases of infective hepatitis have been treated with quinine and mepacrine and have taken no harm thereby. Pamaquin, however, is probably undesirable if there is any suspicion of disease of the liver.

Little need be said about the icteric stage, for when jaundice appears both patient and doctor are usually out of the wood. It is of interest to mention that arthritis, skin rashes, urticaria, and conjunctivitis have all been noted in infective hepatitis, and we therefore cannot regard these symptoms as peculiar to homologous serum jaundice, though they are probably more common in that condition. An uncommon but even more interesting symptom is neuritis, either localized or generalized. It must be remembered that both mononeuritis and polyneuritis are not infrequent in the Mediterranean theatre of war, and that there are few illnesses, medical or surgical, with which infective hepatitis has not at times been associated. We may therefore merely be dealing with the coexistence of two diseases. Nevertheless, similar nervous complications have been seen in this country, and we cannot avoid the suspicion that the virus of infective hepatitis may at times attack the nervous system.

Recurrences of jaundice are not infrequent, appearing in about 2% of cases. In most cases the relapse occurs within four weeks: of 21 relapsing cases, 2 relapsed a second time. The average stay in hospital in British troops is about 15 days, with another four weeks at a convalescent home. Only a very small percentage of cases have been ill enough to evacuate to the United Kingdom. The mortality among cases admitted to hospital is less than 1 per 1,000, and it is probable that a number of subclinical cases never reach hospital.

Duration and Treatment

There is no specific treatment of infective hepatitis. There is good reason to believe that recovery is accelerated by rest in bed and retarded by premature activity, and there is a strong conviction that patients take little harm from the amount of fat in the wartime diet, if they eat according to appetite. It may not be possible to provide special diets in the field, and the strict prescription of a fat-free diet may mean actual under-nourishment of the patient.

It will probably be extremely difficult to prove the value of any special treatment, as the death rate is so low and the length of illness so variable. In a series of cases admitted to a military general hospital the urine was free from bile within seven days after admission in 15%, within ten days in 55%, and within fifteen days in 75%. The coefficient of variation is therefore high even when an objective criterion of this kind is used. It is probably even higher when merely the duration of stay in hospital is measured. Stocks (1944) has analysed the records of 808 patients admitted from the Services to E.M.S. hospitals on account of jaundice. Although the commonest length of stay in hospital (including convalescent home or depot) was 14 days, the range of variation was very wide. A rough calculation from his figures shows that the

mean duration of illness was 40 days, with a standard deviation of 27 days.

To obtain convincing results from therapeutic trials it is necessary to treat a series of alternate cases and controls and to submit the figures to statistical analysis. A calculation* based on Stocks's figures indicates that a treatment which shortened the mean duration of the illness from 40 to 30 days would require a series of 117 cases for the results to be barely significant and 263 cases for them to be highly significant. If, however, the treatment shortened the illness by one-half—i.e., from 40 to 20 days—the corresponding number of cases would be only 29 and 66. Carefully controlled studies of this kind are rarely possible under conditions of warfare, when staff is short and patients are liable to be transferred at short notice. On the other hand, it is hard to get enough cases to carry out preliminary trials of this order at home.

At present we do not know what is the amount of time lost from this illness in our Forces. It is possible that it may be unduly high among troops in an operational area, who may be transferred from one hospital to another owing to pressure on beds and may eventually land up far from their own unit. It should not be difficult to discover the facts by an appropriate statistical study, and, if the time lost proved to be excessive, consideration might be given to the formation of special units for the treatment of jaundice close behind the line, such as have been formed for the treatment of venereal disease. Such units would also enable us to get more accurate information than we have at present about the effect of treatment and rehabilitation on the length of stay in hospital and convalescent home and on the rate of relapse. There is evidence that the enemy keeps men with jaundice in the line as long as they are not completely incapacitated.

Predisposing Factors

There must be reasons why jaundice is more common in war than in peace and why epidemics occur in the autumn. These, however, can only be predisposing factors, for there have been many epidemics of jaundice in peacetime and at all seasons of the year. The agents commonly invoked are cold, strain, dietary changes, and intestinal infection. So far as cold is concerned, "chill on the liver" is a familiar if vague synonym for infective hepatitis, and I have commented on the coexistence of respiratory catarrh in some cases of infective hepatitis. There is a high incidence of jaundice in British air crews, who may be exposed to the cold of high altitudes, and in British officers, who, unlike the men, undress at night and sleep in pyjamas. Nevertheless the last epidemic began in the hot weather and declined sharply after the first frosts of December. Neither American officers nor American flying personnel show an incidence above the normal, and in our own Air Force steps had been taken to prevent chilling when air crews had to fly at unaccustomed heights over Europe. Men of one armoured division, which had a high incidence of jaundice, were sleeping on camp beds in comfortable barns. Strain also appears an unlikely factor, for this particular division had been resting for three months, and the disease has occurred quite commonly in the back areas during this last year. The high incidence in certain combat troops may be a matter of place rather than of strain.

There is no evidence of any dietary or drug predisposition. It has been suggested that officers, who have had a much higher incidence of jaundice than the ordinary ranks, eat less carbohydrate than the men, but it would be ludicrous to pretend that the officers are suffering from carbohydrate deprivation. Suggestions of a deficiency of vitamins appear equally unfounded. Prophylactic mepacrine was administered on a large scale during the summer and autumn of 1943. Mepacrine is a potentially toxic substance, and there was some difficulty in securing tolerance for the drug when it was first administered, but there is nothing to suggest that it had anything to do with the incidence of infective hepatitis. Large epidemics have occurred among troops who were not taking antimalarial drugs, as in Malta and the Western Desert, and there has been a high incidence among British air crews, who take quinine instead

* I am indebted to Major Arthur Jordan, R.A.M.C., for suggesting this calculation.

of mepacrine because it is believed to have less effect on altitude tolerance. There is no evidence that alcohol, arsenic, or sulphonamides play any part, and it is stated that there has been no corresponding seasonal variation in the incidence of post-arsenical jaundice. As for diarrhoea and dysentery, they have been unusually mild in the present war, and they are not more frequent than normal in the history of cases of infective hepatitis.

Epidemiology

In discussing the epidemiology of jaundice there are two pitfalls to avoid. It is dangerous, when studying statistics from a large body of men such as a division, to assume that the experience of all its members has been uniform. If the statistics are analysed it will be found that no such uniformity exists, owing to the diverse movements and contacts of the individuals, and the losses and replacements of personnel. A unit which has been in a theatre of war for a year may contain less than half its original members; the remainder may be new-comers to the area. It is equally dangerous to assume that the epidemic of 1943 was a kind of influence or process which began in the middle of August and ended in January. Such influences may indeed exist, and it is true that the collected figures for a whole area or army form a smooth curve which extends over such a period. But when we break down the statistics into their component parts we find that the major epidemic, is really the sum of a number of minor epidemics, which were by no means contemporaneous. In the present stage of knowledge it would seem wise to base our opinions first on accurate study of these individual small epidemics.

Unpublished studies of this kind have now been made by a number of observers (Kirk, 1943; Spooner, 1943; Mallory, 1943). When a single unit is studied, such as a squadron of an air force or a body of troops living and working together, the pattern of the epidemic is not very different from outbreaks of jaundice in schools and similar communities at home: a herald case, then perhaps another at an interval of 1 to 3 weeks, usually 10 to 13 days; a similar interval, and then the epidemic, which rarely may be almost explosive in its intensity. The great bulk of the cases may occur within 14 days of each other, though usually they are spread over a longer period. I am not going to pretend that observers have agreed in their interpretation of these phenomena. The argument is whether they can be explained by the orthodox hypothesis of droplet infection or whether we must invoke some other mode of spread. To my mind the orthodox hypothesis is quite adequate. But my point at the moment is that this kind of analysis of concrete situations is the proper method of approach, and we should exercise great caution in drawing conclusions from crude data such as the general incidence of jaundice, flies, temperatures, etc.

Infective hepatitis may be localized in place as well as in time. The localization may sometimes be very precise, as in a series of cases occurring at approximately monthly intervals among people working in the operating theatre at a general hospital. Such an attenuated epidemic could very easily be explained by droplet infection, though it had also raised the possibility of infection via blood-contaminated hands. Commonly, however, localization is on a broader scale. In 1942 the Eighth Army bore the brunt of the attack of jaundice, and the incidence was heaviest in troops who had actually spent a week or more in the Alamein battle area. Though the incidence of the disease in Egypt rose considerably in Oct., 1943, it soon fell, and never reached the high levels of 1942 when the troops were in the Western Desert. The Americans believe that there has been less liability to jaundice in the Casablanca area than in Oran and Sicily.

There is no doubt, however, that the disease is endemic throughout the whole Mediterranean area. Cameron (1943) has already shown this for Palestine. Several better-class Egyptians with whom I talked spoke of their children having had jaundice. Dr. A. F. Zanaty told me that not only did he see the disease in private practice in Cairo but that there had been a small outbreak among Egyptians attending the King Fuad Hospital towards the end of 1943; the age of the patients was 15 to 30, and the disease began with slight fever and respiratory catarrh. Moreover, severe cirrhosis which is

not due to bilharzia is not very uncommon in Egyptian children about 7 to 10 years old; some of this may be the sequel to infective hepatitis. In North Africa infective hepatitis is not infrequent in the French and Italian colonists. There is no information about its occurrence in the Arab-Berber population, but all that can be concluded from this is that there has probably been no major epidemic among them.

Although there is a local reservoir of infection, there have been some curious differences in the incidence of the disease in different elements of the Allied forces which have come to the Mediterranean area. The relative immunity of Indian and Maori troops has already been established. It has since been discovered that the relative incidence in white and coloured troops in the American forces is 10 to 1, though there is no difference in work or feeding habits. A contrast is provided by native Belgian Colonial troops brought to the M.E. Among these there were 216 cases of infective hepatitis, with 9 deaths. They are a primitive people who had previously mixed little with other races, and they have also suffered severely from mumps and chicken-pox. It is hard to imagine that a people could have an inborn susceptibility to three different infectious diseases, and these variations in racial susceptibility may well be acquired characteristics.

There is another variation in incidence which can hardly be explained by any inborn difference in susceptibility. This is the fact that officers in the British Army and flying personnel in the R.A.F. are four times as often affected as ordinary ranks and ground staff. The most popular explanation of this phenomenon is that it is due to a difference in feeding habits. The ordinary soldier lines up to receive his food in his individual mess tin, whereas officers and flying crews mess together and share crockery in common. It is therefore easier for infected crockery to circulate among officers and air-crews than among ordinary ranks.

A number of facts support this explanation. Officers and men were equally attacked last year in the New Zealand forces who were in the battle area, where officers may eat under the same conditions as the men. There is no difference in the incidence of infective hepatitis in officers, air-crews, enlisted men, and ground staff in the American forces. American officers more often eat with their men than do ours, using their own personal mess tin or can of food. Moreover, American dish-washing facilities have hitherto been better than ours, and officers' messes have followed the standard procedure, in which the utensils are dipped in water which is actually boiling, so that the risk of transferring infection by this route is minimized. In the British Army officers' cook-houses have not always been as strictly supervised as those of the men, and the level of hygiene has sometimes been inversely proportional to the rank of the mess.

Measures to improve the hygiene of washing-up should of course be encouraged. Too often the water is cold, greasy, and dirty, and the towels far from clean; soap is often in short supply, and however carefully the crockery is rinsed a film of grease remains on it. There is evidence that units who wash up with soapy water have less jaundice than those who do not, but I am not convinced that communal crockery is the essential cause of the high incidence of jaundice in British officers and air-crews. The Service man's knife, fork, spoon, and mug may well be contaminated during washing-up in the communal bucket, and the use of communal plates is very common, especially in more static units. Again, it is odd that warrant-officers and sergeants, who also have their own mess and communal crockery, do not have a high incidence like the officers. It is said, too, that officers are unduly prone even when they are living and eating in their own homes or billets, though I do not know of any figures to support this statement. Like so many dramatic events in epidemiology, the high officer incidence of hepatitis seems on analysis to be due not to a single decisive factor but to a concatenation of circumstances: not only to communal crockery but perhaps to a higher susceptibility of the officer class, a greater opportunity for contact with strangers, the relative youth of flying personnel, and other unrecognized factors. It is a pity that a clue which at first sight seemed so promising has led in the end to no definite conviction.

Even if we accept the hypothesis of infection by eating-utensils it should be noted that it does not imply an excremental spread of infection. Similar precautions have to be taken in sanatoria for tuberculosis to prevent the infection of crockery with tubercle bacilli in the sputum. This point is made because of a hypothesis that the disease may be spread by the excreta rather than by droplet infection. It is certainly surprising that in spite of the frequent admission of cases to hospital in the pre-icteric stage, which is often regarded as the most infectious period, several hospitals have had not a single case of cross-infection in the wards or of transmission to hospital staff. But this could be paralleled in pneumonia and cerebrospinal fever, which are certainly air-borne. The place localization and the seasonal incidence may be more suggestive of transmission by excreta, and it has been stated that on taking up position on a clean site there is no jaundice and on a dirty site there is jaundice, though little evidence is advanced for this statement.

It was noted this year in North Africa that jaundice declined sharply after the first December frosts. This is reminiscent of poliomyelitis, in the spread of which flies are now believed to take part. The curve of the fly population figures in North Africa and Egypt has peaks in May and October and a trough in August, when it is too hot for the flies to breed. The curve of the incidence of dysentery, which is largely a fly-borne disease, has the same bimodal outline. The jaundice curve, on the other hand, is monophasic, with a single peak about October and November. This has been used as an argument against transmission by flies, but it is only fair to the hypothesis of transmission by flies to point out that gonococcal ophthalmia in Egypt, which is almost certainly transmitted by flies, has the same monophasic autumnal incidence (Wilson, 1936). It is only in autumn that the temperature and humidity are high enough to allow the gonococcus to survive during transfer, and the same might be true of the virus of infective hepatitis. Finally, it has been noted that jaundice occurs at a time when culicine mosquitoes are prevalent. There is no conclusive evidence that insects or excreta spread infective hepatitis, but there is enough to make experimental work in this connexion desirable, and indeed rather urgent.

Discussion

Some people are of the opinion that we already have more facts about the epidemiology of infective hepatitis than we can digest and that little more can be learned from field studies until techniques are invented for the detection of the virus or the discovery of latent or subclinical infection. Certainly such techniques would constitute an enormous advance, but it is doubtful whether we do yet possess all the data which could be secured about infective hepatitis as it is now occurring on an epidemic scale. There is still need for intensive study of the incubation period and mode of spread at the actual time and place of the outbreak. Only the man on the spot knows what people are actually doing; whether, for example, men listed as flying personnel are actually flying and, if so, how often, how long, on what types of machine, whether on operations or not, and so on. Again, cases of jaundice in different ranks may actually be occurring in the same place. Other factors, such as movements, replacements, and contacts of troops, incidence of jaundice in the civil population, topographical distribution in units, cook-house hygiene, sanitary discipline, flies, and exposure to cold or to strain, must obviously be studied at the time if information about them is to be accurate. There is also still much to be learned from the analysis of data already collected. We ought to be able to answer the following questions, but I do not think we can.

1. To what extent and for how long is the individual protected against infective hepatitis by a previous attack of infective hepatitis, serum jaundice, or post-arsenical jaundice? Lieut.-Col. H. L. Wallace found that 4% of a series of patients with infective hepatitis gave a previous history of jaundice. I have already mentioned that an attack of serum jaundice seems to make people more susceptible to infective hepatitis.

2. To what extent is the herd protected by a previous epidemic of jaundice or by experience of a batch of icterogenic yellow fever vaccine?

3. To what extent is such protection, if any, influenced by replacements and additions to the herd?

4. Does the seasonal incidence vary in different places and countries, and, if so, can it be correlated with temperature, humidity, flies, etc.?

5. Is there a similar seasonal variation in the incidence of post-arsenical jaundice?

But although we may agree that epidemiological study of infective hepatitis in the field is still worth while, we can hardly begin to exercise rational measures of control until we can answer questions like the following: (a) What are the vehicles of infection—nasal secretions, urine, faeces, food, eating-utensils, fomites, etc.? (b) What is the normal route of infection? (c) Can flies, mosquitoes, or other insects transmit the disease? (d) How can the virus be inactivated, with special reference to camp hygiene? (e) How early in its course and how late is the disease infectious? (f) What are the factors which affect susceptibility to infective hepatitis?

These questions cannot be answered without experiment. Laboratory workers will doubtless continue to try to infect animals in the hope that eventually an animal or a strain of jaundice will be found that will take. At the present moment, however, no immediate line of advance seems open, except transmission to human volunteers. This is not an encouraging outlook; for knowledge rarely increases rapidly when a disease can only be studied by human experiment, as the history of leprosy, trachoma, and pernicious anaemia shows. In the case of homologous serum jaundice we might well quail at the risks of human experiment; for of the 109 recipients of K 60, the notorious batch of pooled measles convalescent serum, 41 sickened—37 with jaundice—and 8 died (Memorandum by Ministry of Health, 1943). Fortunately this seems to have been a most unusual serum, and where homologous serum jaundice has developed after yellow fever vaccine the mortality has been of the order of 1 per 1,000. We have no reason to believe that when infective jaundice is deliberately induced the mortality will be higher than that of the spontaneous infection; and that again is probably less than 1 per 1,000. Nor need we believe that the incubation period will be longer than the usual 30 days, though Cameron's experiments suggest that it might be. Nevertheless, the number of subjects who may be encouraged to volunteer for experiments on the human transmission of infective hepatitis is clearly limited, particularly in time of total war, and a similar limitation must be placed on the circumstances under which it may be permissible to propagate the infection in the community. I am sorry it is not possible to be more optimistic about the prospects of research on infective hepatitis, and I repeat that the prospect might change overnight if we discovered a suitable animal. But at present it is a hard problem. If it had been easy it would have been solved years ago.

Conclusion

Much of the information in this article is derived from confidential papers and reports, and from conversations during a recent brief visit to North Africa. It is not an official pronouncement, and I may well have made mistakes in reporting what I have heard and seen. In one matter, however, I think I shall not be contradicted. The volume and the quality of the work of physicians serving with the Forces over-seas are of a very high order, and they are perhaps not sufficiently appreciated by those of us who have remained to practise medicine at home. A military general hospital in the field may admit more than three times as many patients a year as a teaching hospital in Great Britain. This has been a physicians' war so far, and many more men in the Forces have been treated for sickness than for injury. In the first half of the war civilian surgeons in this country treated more casualties from enemy action than their opposite numbers in the Services. On the other hand, physicians at home have had no experience to compare with the epidemics of venereal disease, malaria, and jaundice, and the outbreaks of diphtheria and poliomyelitis, with which their colleagues in the Services have so brilliantly contended.

Summary

Infective hepatitis is one of the three most important diseases in the Mediterranean theatre of operations and one of the major medical problems of the war.

No specific measures for prophylaxis or treatment are known, but the mortality is fortunately low.

Orthodox opinion is that the virus is spread by droplet infection, but a considerable body of opinion favours spread by excreta and flies.

In the absence of a susceptible animal, progress in knowledge and control of the disease cannot be expected to be rapid, but useful information might be derived from carefully planned human transmission studies.

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WITH THE EIGHTH ARMY IN THE FIELD*

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(Concluded from p. 714)

EVACUATION AND EVACUATION SURGERY

The post-operative transference of the patient to the base, with its strains, vicissitudes, and delays, is a factor which influences treatment to an extent requiring further consideration. Already some idea has been given of a few of the difficulties.

In these campaigns the surgeon often had only a very rough idea, occasionally he had none, of the nature of the further journey of his patient, in terms of time, distance, and conveyances. While he knew that patients would be scrutinized at every halting-point, he appreciated that the less they or their dressings required to be disturbed on the way the better.

Road, rail, air, and sea, all were used. Road evacuation undoubtedly does more harm to patients and their wounds than do the other forms. An ambulance speeding along the wide smooth roads of England gives an impression of comfort and rest that is misleading. The roads in the countries of these campaigns, apart from a few main highways in Italy, do not compare, and by the time an army—or rather two armies, the retreating and the pursuing—have passed over them with the juggernauts of modern war the best become pock-marked and irregularly crumbled at the edges. That is putting the matter at its best. In the desert tracks became furrowed, and, when the sand was soft, vehicles floundered like small craft in a choppy sea. In country cultivated but not populous, roads were narrow and unsurfaced. Wide vehicles forced one another into side ditches. As the cookery books say, add a little water.

If he lie inside recumbent on a stretcher it is an education for a surgeon to make an ambulance journey of some 50 miles over rough roads. Because of the swaying and jolting it is a nauseating affair, however careful the driver may be. What, then, of the less fortunate patients? There are some to whom a road journey of a mile or two, even less, would be fatal; to others the risk may be considerable; to none is it beneficial.

Appreciation of the value of general rest for injuries comes, I think, a little slow to a young surgeon. He is apt, in a plaster-of-Paris age, to calculate overmuch in terms of local rest. A limb well splinted is not necessarily a patient well rested. Lack of general rest naturally also leads to local results by the lowering of resistance to infection.

What happened to patients and their wounds on and after such road journeys as have been described, when undertaken a day or two after operation? Some examples may be given. In cases of compound fracture due to road accidents in civilian

practice limbs are frequently put, after operation, in skin-tight plasters and the patient is retained in hospital. Splitting of the cast is often done as a routine for safety, and certainly is fairly often required. If similar patients in these campaigns were sent on a road journey of several days' duration, not only was it found that splitting of all such casings was essential, but that splitting was not enough and that a split padded plaster was required. Otherwise circulatory risks and disasters occurred. In abdominal cases the journey, if undertaken much before the tenth day, frequently produced an intractable ileus. Patients with chest wounds who were still showing some dyspnoea or cyanosis while at rest became acutely embarrassed. Shocked patients had their shock increased. Patients with early infection had their resistance undermined.

Thus the clinical needs clashed with the administrative necessities. Theoretically there could be a multitude of holding medical units close to the line. That is the ideal of the medical services. But wars are not to be fought with hospitals instead of artillery dumps, with ambulances instead of tanks, or with syringes instead of rifles. Compromise, then, was required. The serious cases had to be held. Half alive in the field is better than dead at the base. The proportion of casualties detained where they were operated upon varied between 3 and 5%. All else had to start down the chain. The chain itself had to be reasonably clear. Along it occurred what might be called evacuation surgery. This, in spite of the term "coffee-stalling," was a skilled business and had never to have a casual air about it. Vigilance and ability were required to pick out and retain and treat those whose condition had deteriorated on the way, to note any overtightness of plaster casings, to renew, when necessary, dressings and casings, to spot and treat secondary haemorrhages, and to be helpful in the many little, but important ways that made for the patients' comfort on their further travels.

Air Evacuation

Air evacuation presents attractive possibilities and has often provided a magic carpet to transfer patients to a base expeditiously in a few hours, on a journey that would otherwise necessitate days, even weeks. Many thousands of sick and wounded in these campaigns were so transferred.

Omitting rare occasions when the plane brings wounded or injured men to the surgeon, and dealing only with post-operative transfers, it must be observed that air evacuation has limitations that are not apparent at first sight. To the popular imagination the idea of the seriously wounded being operated upon quickly, and immediately transferred to the base by air, is a fascinating one. But these seriously ill patients, operated upon well forward, would necessarily have to make a road journey to an aerodrome and another road journey at the other end. Enough has been said about road transport to explain why such journeys should be avoided. Occasionally in the desert a landing strip could be made alongside a casualty clearing station. In closer country that ideal will not be possible until planes can land with a shorter run. But there was another drawback—the uncertainty of the planes. Except for a few special ambulance planes in the desert, casualties were always removed in freight planes which brought up fighting stores and were fitted with stretcher racks for the return journey. There was, due to war exigencies, always some uncertainty as to what planes might be coming and whither they would be returning. While this was a difficulty that was got over in part by special sections of field ambulances sited on the aerodrome, the latter was no place to keep the serious casualties hanging around. The saying that no one is fit for the air who is not fit for the road became more than a saying, it became a rule.

The highest values of post-operative air evacuation were two. The first was to transfer certain patients who, though fit for road evacuation, were liable to special dangers if the journey were protracted. For example, a patient with a fractured femur might, without air evacuation, be some weeks in a Tobruk plaster. In this the limb is not so much "set" as made comfortable for transport. Any malposition might therefore be well organized before more adequate traction methods could be applied. A second example is that of a patient with a penetrating brain wound, where the routine daily treatment

* Based upon experience as forward consulting surgeon, M.E.F. and A.F.H.O., in October–November, 1943, and January, 1943, to February, 1944.