That the fund should be fixed is an essential part of an insurance system. Salaried officials have to bear a similar risk of exceptional work. It is often assumed in discussions of this matter that contract payment is incompatible with private practice; so far is this from being the case that in many of our smaller colonial settlements all the private practice among the white population, even of the best class, is run on the contract system with complete success and general satisfaction. That it inevitably involves scamping and shirking is a proposition which is contradicted by the experience of other walks of life, and I decline to believe it is true in the case of the medical profession.

But from the point of view of mere logic, if the Insurance Act is accepted in principle, as it has been, then, seeing that the fund for the payment of the doctors is limited, the service is essentially a contract, whether the distribution of the fund is to be on the basis of persons on the doctors' lists or attendances given. Any other system restores the very evils from which the old individual system suffered—the risk of bad debts and the unlimited risk to patient coming just when he is least able to bear it, and introduces the element of the money anxiety into the relation between the doctor and his patient, which may hamper the doctor in his work and the patient in his recovery. So bound up is the flat-rate system of fee with the unity of interest of patient and doctor from the preventive side of medical practice that I cannot help thinking that one day it will be extended to better class private practice in proportion to the means of the patient and on the foundation that every family would be compelled to choose a regular medical attendant. It may be looking a long way ahead and sound impossible to-day, but it would carry with it immense advantages to all concerned.

CONCLUDING SURVEY.

I have ventured to carry your minds back over the familiar ground of this question from the very beginning, because it is only by viewing things as a whole that we see them in their right proportions and are able to subordinate that which is subsidiary to that which is fundamental and essential, and because it is here that our difficulties still lie.

It is only by standing back, so that one may get, as it were, a bird's-eye view of the situation in its broad outlines, that we can trace the direction in which the different roads are leading, and that we are reminded of the tracts of country we have left behind.

Also we are now able to reconsider the question at a time when we are beginning to test our anticipations and our fears in the light of actual experience, and this is much to be desired at a moment when some further decision must shortly be come to as to the attitude to be adopted by the Association.

May I say how such a survey strikes me? In the first place we are apt to underestimate the great influence we had in moulding the Act to our views in its broad aspect in the early stages, and also the successes we achieved as a result of our representations to the Government last autumn.

The next broad fact which we cannot ignore is that the Act is now in work and that a majority of our own members in this country are working under it and are apparently resolved to continue to work under it. How can we as an Association, in the face of this situation, maintain, as we did before medical benefit began to be administered, that the Act is to be condemned as derogatory and unworkable? Remember it is one thing for individual members or groups of members to continue to hold this view and to act upon it in their individual capacity, should they consider it their duty, by refusing to take service under its conditions, and another and very different thing to persist in condemning by resolution of the Association the action of those who, after experience of the work and in enormous numbers, apparently take a contrary view.

It is for those who continue working the Act to tell us their difficulties and in what respects we can help them to obtain improvements, when we ought to give them every assistance in our power.

It is a matter for those who are still dissatisfied and are standing outside and resisting to consider, on the one hand, whether they can bring the medical service of the

Act to a standstill, and if so, whether that would lead to a satisfactory solution, or whether the outcome might not be infinitely more disastrous to private practice than the present position; and on the other hand, if that is not their intention or desire, whether they are likely by their present policy to induce the Government to concede their demands, or whether they would not do better to drop their hostility to the measure and by friendly negotiation and co-operation with the men on the panel seek to make the Act as fair and efficient as possible.

But whatever view they may take on these matters, I suggest that the time has come when the Association should officially dissociate itself from any such hostile action.

The present position is that the Act is on its trial; it is still being improved, and it is too early to pronounce judgement upon it; it is not yet time to curse it nor to bless it altogether.

Let us, then, withdraw our condemnations, which were based mainly on anticipations; pronounce no blessings, but wait and see. If we call a truce now we shall save ourselves from the risk of humiliation and the loss of prestige in the public eye, which would be the result of ineffectual action.

And as regards our own members and the profession generally, we shall gradually heal our dissensions, we shall close our ranks, renew our vitality, and clear the ground for the Association to exert in an effective and whole-hearted manner that influence on public opinion and future developments to which it is entitled by its representative character and its splendid work and achievements in the past.

SOUTHERN BRANCH.

MEDICINE AND EUGENICS.

By JOHN COWPER, M.B., C.M.EDIN.,

Shanklin, Isle of Wight.

For some time past we have constantly had before us a subject closely affecting the relation of our profession to that of the general public. It came from without, having a political origin, and we had to face it, though not a few of us got rather weary of its changing aspects.

This year another subject, of a very different kind and origin, and of vastly greater ultimate significance, has been prominently brought before the notice of the people of this country, though it affects other countries as well as our own. I allude to the first Eugenic Congress, very appropriately held in London, the city in which Sir Francis Galton established his laboratory for the study of heredity as affecting the members of civilized communities.

Just as sanitary and hygienic reform was developed and pushed forward by the medical men so will this newer science rely to a large extent upon the same profession for the establishment in the minds of the community generally the grasping of the fundamental principles underlying it.

I do not assert that medical men alone have been the pioneers in the study of sanitation, hygiene, and heredity, but if you deduct the men who have had a medical education, whether they practised their profession or not, very little indeed would be left. Moreover, before any new studies bearing upon, and affecting, the lives of men and women can become effective the practical bearing has to be brought home to the minds of the people themselves, and in this respect I do not think that the profession to which we belong has had its due share of credit for the sanitary and hygienic reforms which it has been instrumental in bringing about, often not only without thanks or acknowledgement, but in the face of opposition and difficulty.

I am well aware that even amongst experts and medical men many of the questions in heredity are still theoretical, and some highly speculative, in spite of all the work that has been done in recent years. But besides these theoretical and speculative questions, certain well ascertained facts have emerged, as can easily be gathered from the treatises of such writers as Professor Thomson and Dr. Archdall Reid.

Darwin long ago pointed out that the factors determining the modification of organic beings are heredity and environment. The sanitary and hygienic reformer has given his attention to environment, and at the present day it must be admitted that all practical attempts to improve man's condition has been likewise aimed in the same direction. Such great strides have been made in the practice of our art, that we are apt to overlook the fact that all our efforts in the treatment of man's diseases and maldevelopments have been directed to his environment (sanitary and hygienic surroundings, baths, foods, drugs, massage, electricity, and surgery are all environmental).

Now, biologists, from Darwin downwards, have all placed a far higher value upon heredity than upon environment, and that view is even more pronounced in the writings of biologists of the present day. Nurture can do, and has done, much. Nature can do more. The stock-master has realized and acted upon this fact. Certainly, he has made use of environment, but it has been as nothing in the production of our domestic breeds of animals compared with heredity—namely, the selection of favourable variations. The breeders of animals have, of course, unlimited control over their stock; the generations are much shorter, and animals have been selected almost entirely for physical characters. Even the dog has suffered much in this respect.

In the case of man, the modification of environment is comparatively easy; the guiding of heredity is intrinsically difficult and surrounded by conventions. In the case of disease, however, heredity has played an important part. No one can peruse Dr. Reid's book on *Alcoholism* without becoming firmly convinced of that fact, and the same applies to zymotic diseases. Natural selection is here the agent at work. So much has been done to ameliorate man's environment that there is great danger of our attributing too much to its influence.

In certain ways the processes are antagonistic. Up till recent times the human organism has been struggling to attain to a natural immunity to microbic disease, and attempting to maintain itself against exterminating influences by a process of the natural elimination of the most susceptible members. Modifying the environment is beginning at the other end; it is, indeed, a complete reversal of method, and the more we ameliorate the environment the more we render the average member of a community susceptible to any given disease. What we gain at one end we lose at the other. If, however, we can completely control any given disease (such as cholera and typhus), then susceptibility matters nothing so long as we can maintain that condition. Can we hope to gain a complete control over the tubercle bacillus? Recent statistical researches by Professor Karl Pearson seem to show that much of the amelioration which we medical men have been in the habit of attributing to recent methods of dealing with the disease is in reality due to a natural process of elimination, and he supports this view by stating that the most marked fall in the tuberculous death-rate occurred before the introduction of the sanatorium treatment.

This is a point of importance, for the greater the number of persons that are tided over to a marriageable age the smaller will be the average natural resistance to the disease. This is a factor that faces us in other branches, as, for instance, obstetrics. The higher the skill attained in the obstetric art the more difficult will childbirth become. Again, when the child is launched into the world, the manufacturing chemist, the skilled nurse and the doctor are all, rightly enough, combining to make the environment as favourable as possible. To realize this, one has only to compare our present infantile mortality with that of a few decades ago, or to compare the East of Europe with the West in that respect. To prevent Nature (heredity) stepping in and claiming her own, we must be prepared to go on continuously improving the environment of the infant, and the same argument applies to our falling death-rate. Such like factors have been pointed to as portentous evils, leading to race degeneration, but it does not appear to me that the process of attaining a natural immunity (tubercle, etc.) would thereby be prevented. Its attainment would be delayed. I may say here, in parenthesis, that the sanatorium isolation of the poor affected by tuberculosis is an inestimable benefit conferred upon the relatives of the tuberculous, and it appears to me that

we ought to develop this system as they have done in America.

It must have struck most of us in inquiring into the hereditary history of patients, how little the majority of people know of the medical history of their ancestors and collateral relatives. Jonathan Hutchinson long ago suggested the advisability of the head of the family tabulating the diseases affecting his family. Unless some system is carried out, we cannot get sufficient data to be of any real value, except in rare cases. The importance of the problems of heredity is beginning to be generally recognized. For instance, is the assumption true that the slums of cities produce the kind of human inhabitant found there, or does the type gravitate there, having been cused from better places? The need for more data is urgent, and we as a profession ought to press that need home, for without carefully collected statistics on a large scale all sorts of unsound measures devised to ameliorate the social conditions of the population, both in health and disease, are sure to be resorted to.

It would be outside the scope of this paper to enumerate even most of the many theories of heredity. Not to go back farther than to the French naturalist, Lamarck, who wrote in the early part of the nineteenth century, whose theory was that of the "transmission of acquired characters." That all organisms, including man, do make acquirements during their lifetime is an admitted fact. Certain organs are strengthened by use, while others decrease and degenerate from absence of functional activity. Lamarck believed that these acquirements are inherited, and he supposed that after many generations of this process definite results would accrue.

Darwin, writing later, accepted this view, but he had to account for the phenomena of sexual and asexual reproduction, regeneration of lost parts, variability, inheritance of inborn and acquired characters, and reversion to a previous ancestor. He then formulated his theory of pangenesis, which supposed that the various cells of the body throw off "gemmules," which were distributed both in the germ cells and somatic cells of the individual. After this, a current of opinion set in the direction of independence of the germ cells, and that these cells were not made up of samples of the body; that is to say, the germ cells produce germ cells and body cells, but body cells do not produce germ cells, but only contain and carry them on.

Professor Weismann was the great exponent of this view, and his theory of the continuity of the germ plasm is known to all. One very important point at once arises from this theory, and that is that acquired characters cannot be transmitted if the theory be correct. Starting from the solid ground of chromosomes, he passes on with theoretical entities to develop a highly complicated and speculative system, until as a scientific doctrine it is in imminent danger of languishing by the very luxuriance of its theoretical growth. Whatever the ultimate fate of his theory, he has done more to stimulate inquiry into the subject than any other living investigator. Most of the younger biologists have accepted Weismann's doctrine of the continuity of the germ plasm and the non-transmission of acquired characters, but this view does not entirely hold the field, as may be gathered from the presidential address of Professor Francis Darwin, the botanist, at the British Association in 1908. That address was devoted to the elaboration of what is usually known as the mnemonic theory of heredity. This implies that every living cell, whether constituting a unicellular organism or a unit of a multicellular being, is possessed of a sort of organic memory. In the more highly organized forms of life the body plasm is coordinated and guided chiefly by means of a nervous system, and there we know with what facility difficult and complex processes become automatic (that is, what we are in the habit of calling unconscious) through repetition. In the germ plasm the ontogeny is regulated mainly or solely by the cell nuclei, or chromatin (Weismann's germinal substance); and Francis Darwin says, "May we not therefore consider it probable that the nucleus plays in the cell the part of a central nervous system?" Professor Hering had before this maintained "that ontogeny—the building up of the embryo—is actually and literally a habit." That is to say, one may liken each generation to the successive lessons in learning a

game or the playing of a musical instrument, or the details of an unknown road. The playing of a game or the musical instrument is learnt, and the road becomes so familiar, that complicated activities are performed without, as we say, thinking of them. Also there is the possible cumulative action of environment. Unhealthy conditions may exist for several generations before the effect becomes obvious, just as the soaking of water into a stratum of clay underlying sandstone goes on insidiously and unnoticed until the resulting landslip occurs suddenly, and is obvious at once.

Lastly, we may mention the Mendelian theory of heredity. Though historically Mendel published his views in 1866, they received no attention till 1900. Mendel's theory is important, for it is the antithesis of what was generally held, and what one may best call the theory of "blending"—namely, that a fair and dark parent or a short and tall one will have intermediate offspring, and that this will be a more or less continuous and uniform process. Mendel has established that in a large number of instances this is certainly not the case. He found that certain characters which vary are inherited discontinuously, and that there is not blending, but segregation. His now famous experiments were carried out on varieties of the edible pea, which grew in his monastery garden. He observed that the peas varied in certain characters; he noted these individual characters, ignoring the rest, and experimented upon one character at a time.

In both the edible and the sweet pea there is a tall and a dwarf variety. The tallest plants of the dwarf variety are shorter than the shortest individuals of the tall variety. Mendel found that in crossing the short and tall varieties all the progeny were tall—quite as tall as the tall variety. He then self-fertilized these tall hybrids, and they then produced some short and some tall, but none intermediate. Here is definite segregation.

Mendel looked upon the tallness and shortness as alternative characters, and as tallness alone appeared in the first cross, he spoke of it as "dominant," and the shortness as "recessive." A dominant character has some factor which is not present in its recessive alternative. By continuing this process the tall and short individuals can be completely segregated, so that they breed true for an indefinite number of generations. This same process has been found to apply to many characters in animals in which it has been tried.

Mendel explains these facts by supposing that the tall plant produces germ cells or "gametes," bearing the character tallness; the short plant the reverse. The hybrid contains both conditions; the cells resulting from the union of these gametes contain a double set of determinants for the various characters, one determinant of each pair being derived from the male and the other from the female. An individual, the production of two gametes and having this double character, is called a "zygote." Further, when the zygote contains two like characters, it is called a homozygote (two tall), when two unlike characters it is called a heterozygote (a tall and a short). The gist of Mendel's theory, then, is the segregation of pairs of characters (called by Professor Bateson allelomorphs) in the production of homozygote and heterozygote offspring, which, when self-fertilized or by others of the like constitution, breed true, whatever their ancestry.

These results are not merely of scientific interest but of practical application, as is shown by the work of Professor Biffen of Cambridge on varieties of wheat. While English wheats produce heavy crops, they are liable to attacks of a fungus known as "rust," and the grain is deficient in gluten, which renders it inferior to the hard foreign varieties for baking purposes. It was held that the highly glutinous hard foreign wheat could not be grown in the English climate, as the hard varieties imported for trial soon deteriorated. Professor Biffen, however, obtained a hard variety which kept its qualities when grown in England, but its yield was too low to be grown for profit, and it was also subject to "rust." Among the many varieties experimented with he found one that was immune to attacks of rust, though in other respects it was practically useless. He further found that these wheats in their characters of immunity to "rust," hardness of grain, and power of production exhibited Mendelian factors on an experimental breeding analysis, with the

result that he produced a wheat combining hardness of grain, heavy yield, and complete immunity from "rust," and this wheat has been found to retain these qualities over the series of years it has been grown. Sufficient work has now been done to show that the same results can be obtained in the case of animals also. It is not my intention to add anything to the already interminable discussion on the important question of the "transmission of acquired characters." Much of the discussion has been over terms rather than facts, and some of the authorities are not yet agreed as to what constitutes an acquired character (vide *Nature*, February and March, 1912). This year a new edition of Weismann's book has been published, and in it Mendel's conception of segregation is accepted, and the author endeavours to reconcile his theory of heredity with the recent researches along Mendelian lines. The sexual cells are certainly segregated, but they are not shut up in hermetically sealed compartments until the moment of conjugation; in fact, they are nourished by the same blood and lymph as the rest of the body. The extremely dogmatic statements that are now current are liable to lead to the overlooking of a very important question, which may be of marked racial import. May not poisons, such as alcohol and toxins of various diseases, affect the germ cells previous to conjugation? Physiologists tell us that alcohol and various toxins do lower the vitality of cells. I can see no reason why toxic substances circulating in the blood and lymph should not affect the germ cells, and even produce pathological variations affecting the offspring built up from these cells. My late teacher, Professor Ewart, obtained results from experiments on pigeons suffering from malaria, which seem to indicate that the germ cells are liable to be influenced by fevers and other forms of disease that for the time being diminish the vitality of the parents. Further experiments may show that the germ cells are influenced in different ways by different diseases. In this case the birds infected were "recessive" while they were suffering from the malaria parasite, but were markedly "dominant" when well. In spite of all the material to gather from, there does not appear to be sufficient carefully collected data to decide whether alcoholism in parents produces degeneration in offspring. Possibly the data on this subject would need to be larger and more unequivocal than in others, for it is a curious psychological fact what a tendency there is to unconscious bias when considering the reaction of the body and spirit. The *odium alcoholicum* seems only to be second to the *odium theologicum*. Be that as it may, the environmental school appear to me to have left two points that might with great advantage have been dealt with. The evils of the over-indulgence in alcoholic drinks are admitted on all hands. That being so, the liquors containing the higher percentages are more detrimental, and a graduated duty in favour of the drinks containing the lower percentages would be of value. The other point is, why should not dipsomaniacs, with proper safeguards, be subject to compulsory certification, just as other maniacs are, not only for their own protection, but for that of their relatives?

Possibly none of these theories of heredity contain the whole truth, but nevertheless sufficient well ascertained facts and results have been obtained to compel us to consider our position and attitude towards them. Before sterilization or other drastic methods as already advocated by some enthusiasts can have any chance of application, we must know a great deal more of the hereditary history of the individual. We want vastly more carefully gathered statistics. It has been said that you can prove anything by statistics. That has arisen through the skilful handling of statistics by politicians for party purposes. A competent mathematician trained to deal with statistics can bring out the issue provided that the material collected is adequate and correct. Huxley's analogy of the mathematical process and the flour mill was an apt and sound one. A mill will grind both good and bad grain, and whether you get out sound flour or not depends upon what sort of corn you put in, perchance of wheat or some other grain. Take, as an example, the statistics dealt with by the Registrar-General as to the certified cause of death, and imagine his results with and without an autopsy. When all allowance has been made for what is merely apparent, it is the real increase in

nervous wreckage that confronts us. Where does it come from? In short, what is its heredity? I am not now speaking only of asylum cases, but one would have thought that the asylums would have produced available statistics. I find, however, that Dr. Clarke, of Banstead Asylum, when writing on this subject in the *Journal of Mental Science*, says: "We have no statistics of insanity that are of any practical use. In the tables published annually by the asylums and by the Commissioners, the defective histories swamp and entirely violate the good ones, and no table gives any information as to the normal relatives or offspring of the psychopathic." The treatment of criminals is said to be a failure, and it always will be a failure on the opposing lines adopted and suggested, for the reason that you will not alter the constitution of a man's nervous system by handing him over to the philanthropic humanist, who would treat him as a spoiled child, any more than by putting him in prison. A seizure of "grand mal" is not dealt with satisfactorily, either, by a strong man holding the victim down, or by an old lady administering soothing syrup with kind words. But do we lock up criminals to reform them? Surely we lock them up to protect society, as we excise a cancer to save the individual. Doubtless it is true that, in our administration, cases have been classed as criminals that should have been otherwise dealt with.

It appears to me that some form of segregation, however attained, is inevitable in the not very distant future, if we are to avoid a continuous increase in nervous degeneration. In making a diagnosis in our routine work we get the available facts and marshal them as best we can in order to draw what seems to us the soundest conclusion. But here we have criminals, lunatics, feeble-minded, epileptics, alcoholics, and a host of other neurotic degenerates, and yet we have hardly any facts to guide us as to their origin. It is really remarkable that we know so little not only of the heredity of these unfortunates, but also of our soundest and best stock. Surely it is the duty of a Government responsible for the destinies of a people to obtain information on this the most important factor determining the advance or degeneracy of their population. Powerful and thoughtful indictments have recently been made based on such statistics and facts as are available. Are we allowing to slide what, in his *Social Evolution*, Benjamin Kidd speaks of as "projected efficiency," and, as writers like the Whethams maintain, the sound stock to dwindle while the unsound are rapidly increasing?

Before we cry out in panic for a remedy we ought to demand the facts on which alone a diagnosis can be made. The influence of the medical profession in matters of this kind is great, and that is my reason for venturing to address you on what is only, on its broader aspects, a medical question.

A CASE OF PERFORATED COMMON BILE DUCT FOLLOWED BY SUBPHRENIC ABSCESS, OPERATION, AND RECOVERY.

By C. CAMPBELL-HORSFALL, M.B., CH.B. VICT.,
NEWQUAY.

THE patient in the following case, a married woman aged 45, had suffered for years from dyspepsia and spasmodic pains which were at times intense and radiated over the upper abdomen and chest. I saw her during two attacks, which were unmistakably due to gall stones, and it was during the last of these that occurred the catastrophe which is the basis of this note.

The patient had been seized with intense pain in the chest and upper abdomen and signs of common duct obstruction, jaundice, bile-stained urine, pale stools, and a moderate degree of fever. She had consented to operation, and this was to take place as soon as the acute symptoms had subsided. On March 14th I had left her comparatively comfortable, but on reaching home I had an urgent summons to return at once. I found her lying in bed quite immobile, pale and cold, pulse 120, and temperature 100°. It seemed that shortly after I had left she had been seized with an agonizing pain causing her to jump out of bed, and then collapsing. There was no rigidity of the abdominal muscles, nor was there any marked tenderness.

Deep pressure in the region of the gall bladder produced a well marked diaphragmatic grunt.

The following day there was moderate distension of the abdomen; pulse 120, but temperature falling. From the anamnesis I concluded the common duct had ruptured, and so, with the assistance of Dr. Tonking of Camborne, I proceeded to explore her abdomen.

Operation.

Under ether anaesthesia I opened the abdomen through the upper right rectus. On opening the peritoneum bile-stained fluid and pus welled up into the opening. After shutting off the peritoneal cavity an exploration was made of the biliary region, and although one could trace the stream of bile and pus down to the region of the common duct, the actual tear could not be located; the gall bladder was shrunk. A rubber drain was carried down to the common duct and a cigarette drain placed in the right kidney pouch. An incision was next made over the pubes, and Douglas's pouch was found filled with bile-stained fluid; this was gently mopped away and a tube inserted. The visceral peritoneum did not show any marked inflammatory action.

Progress.

The pulse was much better after the operation, and the patient was put back to bed in the Fowler position and proctoclysis commenced. She quickly rallied from the operation. Next day she was fairly comfortable, but looked blue and the skin leaky. As there had been no vomiting and she was very thirsty cold water was allowed freely. The tubes were removed on the fifth day. The day following she complained of pain in her left side; the temperature went up to 103°, and pulse 112. A definite fluctuating mass could be made out on the left side.

The same evening under ether I evacuated a large collection of bile-stained material from this side and inserted a drain. There was a good deal of shock after this, but under salines and pituitary extract the pulse recovered and the patient seemed much easier. The temperature fell, but the pulse still remained rapid. In spite of this her condition was not satisfactory, respirations became more frequent, the upper part of the abdomen ceased to move, and on March 25th there was distinct evidence of another collection of matter in the upper abdomen.

Second Operation.

Under ether an incision was made through the left upper rectus, and at once one came upon a large accumulation of biliary matter, together with a quantity of mucus similar to the contents of the small intestine. The cavity could be traced up under the diaphragm towards the right side. A large drainage tube was put in and a counter-opening made in the left flank, with through-and-through drainage.

Result.

Almost at once there was a marked improvement in the patient's condition, and this continued. During this time she had an attack of pain and evidence of renewed blocking of the common duct, but whatever the cause it quickly subsided, and with the exception of some fluid in the left chest (which has now disappeared) her convalescence has been uneventful, and she is now in better health than she has been for many years past, and instead of denying herself foods is able to eat and digest anything.

This case is interesting, not only on account of the satisfactory result, but because the usual triad of symptoms—pain, vomiting, and muscular rigidity—accompanying an intra-abdominal catastrophe were conspicuous by their absence. There was no vomiting at any time, and previous to the first operation, beyond the distension there was nothing to indicate the focus of infection except the history of the case which was undoubtedly gall stone disease.

Rupture of the common bile duct is extremely rare. Perforations of the biliary system, though by no means frequent, form an important class in the production of abdominal catastrophes. McWilliams has collected 108 cases from the literature.

In a series of 3,180 operations on the biliary system, there were only 29 cases of perforation, or less than 1 per cent. In the series of 108 cases there were only 4 cases of perforation of the common duct.

1. *Kehr*.—Female, 51; symptoms four days; perforation of common duct; cholecystectomy. Died.
2. *Riedel*.—Male, 56; symptoms three days; perforation of common duct and gall bladder; cholecystotomy. Died.
3. *Routier*.—Female, 56; symptoms one day; perforation of common duct, free bile; cholecystotomy. Recovered.
4. *Neupert*.—Female, 42; Symptoms fourteen hours; perforation of common duct at juncture with cystic duct, free bile; cholecystectomy. Recovered.

To this number must be added my own case. The five cases show in a very graphic way the importance of early operation in these cases. In the first two the symptoms had been in progress for four and three days respectively, and both died. The other three were operated on within thirty-six hours after perforation, and all recovered.