of the dura mater was normal; on removal, a thin, irregular shreddy layer of blood-clot was found between it and the arachnoid, and slightly adherent to both. It commenced over both occipital lobes, and extended downwards over the entire surface of the cerebellum; the whole of the base of the brain was also involved in a similar condition. There was more than usual fulness of the cortical veins, and the brain as a whole was unusually firm. No further macroscopic changes of any note were evident. The basal vessels appeared to be fairly normal.

Thorax.—Lungs showed considerable emphysema; heart presented a white opacity in pericardium covering anterior surface; no hypertrophy or dilatation, and very slight atheroma of the aorta.

Abdomen.—Liver presented signs of old extensive perihepatitis. Kidneys showed slight fibro-fatty changes. Large intestine: on opening up from the rectum to the cæcum it was found to contain a quantity of scybala mixed with blood-clots, which were more or less adherent to the mucous membrane. The blood ceased abruptly at the level of the hepatic flexure of the colon, where for an area of about three square inches the mucous membrane presented a purplish discoloration due to engorgement of the veins and extravasation of blood into the coats of the bowel, but there was no apparent breach of the surface. This was evidently the site from which the hæmorrhage took place. There was no ulceration in any part of the intestinal tract, nor were there any hæmorrhoids.

This case appears to be worthy of record, owing to the associated hæmorrhagic conditions. Although I have been unable to find any reference to cases presenting similar features, I believe one was recorded some few years back in which there was an associatied vesical hæmorrhage. Points to which attention might be drawn in the above case are the low blood-pressure, the fairly healthy condition of the kidneys, and the absence of any marked arterio-sclerosis. The hepatic condition found *post mortem* may be suggestive of alcohol as a factor in the causation, but this was denied in the previous history of the case.

Occasional Notes.

Insanity and Toxæmia.

The Scottish Division entered upon an important discussion at their spring meeting. It has been evident for some time

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that the trend of opinion in the advanced school of pathology has been towards a theory of toxic causation of insanity. Indeed, it is inevitable that the extension of such a theory, pushed to its furthest limits, should permeate all departments of medicine and find enthusiastic supporters in our own specialty. Toxic causes, established in such diseases as diphtheria and tuberculosis, have eluded observation in cancerous affections, although the search has been skilful and prolonged. A poisoning of the system, which we now call toxæmia, has been recognised in the domain of psychiatry for many years. Schroeder van der Kolk was so assured of his opinion that he based his treatment of insanity upon the theory of a common causation in the overloaded, disordered condition of the great intestine; and, even at the dawn of medical science, the toxic effects of black bile were denominated melancholia, and described at interminable length. At any rate, these theoretical ideas were useful in drawing attention to the physical basis of mind, and in offering indications for the treatment of its disorders. Of late years, however, there has been a remarkable advance in the strictly scientific knowledge of the physiology and pathology of living organisms. The science of biology has been rapidly evolved, and it is a real struggle to keep pace with the more important conclusions formulated by the great army of workers. In our own particular sphere of interest, we could not but expect the moment when the toxæmic stalking-horse should be advanced to occupy territory hitherto held strongly by the old guard of a less materialistic psychology. Therefore the battle-field at Glasgow could occasion no surprise—it was as inevitable as the great Boer war. Not that the Old Guard were inactive in defending the positions in which they have been so long entrenched, or that they have entirely lost their scalps in the fray. It would rather seem as if they were ready to establish a zone of neutral territory—ground common to both, which may yet be extended by diligent sapping and mining on the part of the aggressors. So the day ended.

We have no doubt that the report of the discussion, as printed on page 434, will be carefully read and considered.

Dr. Clouston, desirous of arriving at adequate results, left no point of moment unmarked—except that perhaps, in the arena so familiar to every true-born Scot, he devoted none

of his gladiatorial skill to the abiding question of *free-will*. He will not have it that mind is a mere secretion of brain; but is he not responsible for the story of the man who found salvation after the administration of a brisk purgative? The fact remains that we are in constant touch with the unknowable, and that we may safely content ourselves with invincible ignorance of the unsolvable.

Is there so great a practical difference between Dr. Clouston and Dr. Ford Robertson as mere words would indicate? Myxœdema was, quite recently, an incurable disease. By extension of knowledge, it is now a commonplace of medicine to treat it to recovery. We may unite in hoping that further research will afford us similar results in the management of other forms of mental disorder. There is no difference between the parties in reference to this practical side of the discussion. So far as we understand the difference, it refers to a question of territory—how far can the toxic theory be pushed? Dr. Ford Robertson gives it a wide application in saying that he would claim that all forms of insanity occurring in normally developed personalities are toxic in origin, the toxæmia being generally established before the mental cataclysm occurred, the nutrition of the cell having been altered by a breakdown in the first line of defence in the organism—in the gastro-intestinal tract and the bone marrow especially. That, of course, brings insanity into a line with tuberculosis—there is no hereditary consumption, only an hereditary weakness of defence against the tubercle bacillus. We can thus think more precisely. If these toxins can be demonstrated, more especially if the antitoxins can be beneficially applied, Dr. Ford Robertson's theory will be established; and it seems to us that it is, if not a more hopeful attitude, at least a more satisfactory method of approaching the subject. But much remains to be proved; has anyone accumulated a sufficient body of evidence to show that the first line of defence has broken down before mental disorder is apparent? Dr. Bruce indicates several observations on that point, and we hope that trustworthy records will be exhumed from case-books in order to establish the facts one way or another. All this does not induce us to repudiate the empirical treatment at present generally adopted. Our whole efforts are directed towards physical and mental hygiene. No one could suppose that

Dr. Ford Robertson in dealing with a case of simple melancholia would advise the continuance of mental worry and overstrain, or an environment of morbid psychical influences. We would place no great stress on Dr. Clouston's reference to the characteristics of periods of age—just as little as on the religious manifestations of the insane. They are naturally tinged by the colour of the surroundings, just as they come up to date with delusions regarding the Röntgen rays.

It would appear to us that the weakness of Dr. Ford Robertson's position lies in his absolute denial of any causative force in manifestations of functional activity, and that the strength of Dr. Clouston's position lies in the opposing idea. The latter finds his chain of evidence in primary morbid weakness, a stimulus, a nutritional disturbance, and then, after all, a toxin. The toxins of fatigue have been already recognised, and the various forms of trades' paralysis have been described over and over again. Will Dr. Ford Robertson object to our assigning a blacksmith's paralysis to his daily occupation as *a* cause in the complex of causes? It would be an imperfect description of the case which would omit such a relevant fact, as it would be impracticable in treatment to ignore it. We know that, in such a case, nutritional changes precede the disease; and on that analogy Dr. Clouston might maintain his position. In the affairs of daily life, however, we have not to determine whether the egg or the hen occurred first, and until the toxæmists accumulate more evidence we do well to withhold a final deliverance.

The Cathcart Case.

After a period of more than ten years justice and mercy have met in this case, some features of which are very interesting and may one day form the basis of a paper on medico-legal relationships.

The early history of Mrs. Cathcart's illness was dealt with at considerable length in the *Journal of Mental Science* for October, 1891.

Many of our readers doubtless recollect that in the July of