Glomerular Nephritis

ADDIS

GLOMERULAR NEPHRITIS

DIAGNOSIS AND TREATMENT

THOMAS ADDIS, M.D., F.R.C.P., (Edin.)

NEW YORK THE MACMILLAN COMPANY 1950

腎臟病診斷與治療 31"×43" 1/22 定價 ¥ 47,000

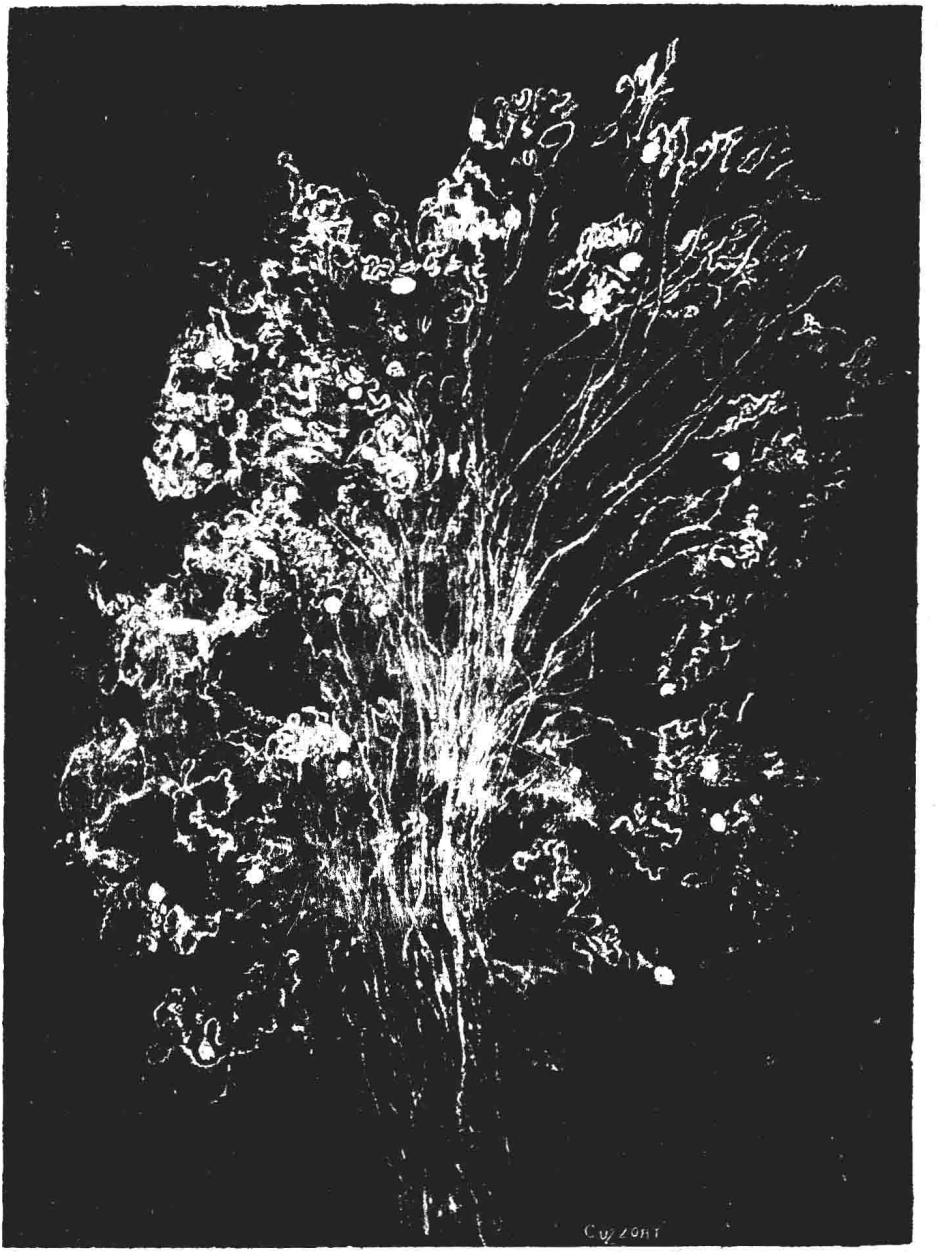
影　印
發　行
印　刷
裝　訂

專售科技西書
萬錩書局南京發行所
南京(2)太平路28號電話21936
本書新定價¥ 4.7000

一九五五年一月化學印　　(印數) 1—156

Printed in the United States of America

Set up and electrotyped. Published, March 1948.
Reprinted January 1949. Reprinted June 1949.
Reprinted March 1950.



Microdissection of a lobule from a normal, adult kidney. About the periphery of the lobule lie the glomeruli, each surrounded by the periglomerular mass of its proximal convolution and associated distal convolution. From this cluster two tubules pass to and from the central medullary ray, the terminal portion of the proximal convolution entering, the ascending limb of Henle's loop departing. In the sheaf of straight tubules that compose the ray, the junction of the collecting tubules can be occasionally seen (x 15).

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Dedicated to the patients, laboratory workers, dietitians, medical students, and doctors who have worked in the Nephritis Clinic of the Out-patient Department of Stanford University Medical School in San Francisco. Here they will find a record of their observations and of the conversations and controversies in which they have taken part over many years.

Affectus qui passio est, desinit esse passio, simulatque eius, claram et distinctam formamus ideam.

Spinoza, *Ethics,* part V, prop. 3.

INTRODUCTION

This book is written because we have come to the conclusion that the present-day treatment of patients with renal disease is inadequate and sometimes dangerous. There is no universally accepted plan of treatment, but none of the current proposals takes cognizance of the therapeutic efficacy of rest. In the past the giving of rest to the diseased kidney was explicitly recognized as the theoretical justification for various dietary prohibitions and as the reason for the administration of drugs that induced sweating and purging. But in those days we did not know how the kidney worked, and so we did not know how to give it rest. Today, though we know something about the work of the kidney, we find the principle of rest implicitly, if not explicitly, denied. Recognizing the errors in the dietetic treatment of the past, and influenced by accumulating evidence as to the deleterious effects of protein undernutrition, the leaders of investigation in this field treat patients with renal disease, particularly those with edema, in such a way as to impose an unnecessary amount of work on the kidneys. On theoretical, experimental, and clinical grounds we have slowly reached the conclusion that this is a dangerous error. This whole book may be taken as a reasoned exposition of that view.

In general, then, our subject is the theory of the application of this principle of rest in the case of any patient in whom any disease has led to a substantial reduction in the number of functioning nephrons. But whenever we try to apply the theory in practice we find we cannot use it effectively for any individual patient until we know him and his situation and have learned from direct, clinical observation of his blood and urine as much as we can about the nature and the extent of his renal lesion. This information modifies and controls the endeavor to contrive those conditions, consistent with adequate nutrition, under which the minimum amount of renal osmotic work will be imposed. Without this knowledge the application of the principle is only formal and schematic and, in detail, very likely ill-advised. So the general theory can be converted from an abstraction into a reality only insofar as it is fitted in detail to the specific conditions that exist in particular diseases and in particular patients suffering from these diseases. This task we cannot undertake. There is only one disease of the kidneys in which we have had an experience sufficiently extensive, detailed, and prolonged to

warrant us in using it as a means through which the actual and practical application of the theory of rest from osmotic work may be illustrated. This disease is glomerular nephritis. The title of the book is, therefore, *Glomerular Nephritis: Diagnosis and Treatment.*

The methods we use in our clinical work can be used in any doctor's office. We believe this, because, like most doctors who are not specialists, we have to make our own clinical observations. We have no chemists working for us, and, in any case, most of our patients are not able to pay for repeated determinations of urea, creatinine, and protein in their blood serum or for frequent estimates of the rate of protein excretion in their urine. When we came to see that rational treatment required the reiteration of these and other measurements over long periods of time, we were thus confronted with the necessity of doing the work ourselves. But we had no time to take histories, make physical examinations, and treat our patients, and also to determine the quantity of these materials by means of the standard laboratory methods. We were forced to contrive a series of extremely rapid methods, so simple and mechanical in their operation that the possibility of large error is excluded, and so direct that the results are presented as pictures whose significance can be read at a glance.

It may seem that methods of admittedly only approximate accuracy are to be justified, if at all, as merely makeshift contrivances, to be used when no laboratory facilities are available or when the economic status of the patient makes it impossible to obtain the services of those who can give us more precise results. This perfectly natural view is based, however, on a misapprehension as to the purpose of these methods. These are clinical, not laboratory, methods. They are designed to make the examination of the blood and urine a part of the physical examination of the patient. The results they give sometimes raise questions that can be answered only by the use of laboratory methods for which the doctor has no time. But they cannot be replaced by any laboratory method, no matter how precise and elaborate. They give us, while we are still working with the patient, information that is useful for any patient and indispensable for those with renal disease.

We are well aware that this enthusiasm for laboratory methods so quick and simple that they merge with inspection, palpation, percussion, and auscultation will amuse many of our most experienced readers. This is what they have always wanted, often tried for, and always failed to get. Their skepticism is founded on hope repeatedly denied. They know very well that a good doctor will not leave his patient for any physical or chemical manipulation, no matter how easy it may be. We know that too, and we are not supposing that the doctor will ever measure and

centrifuge blood and urine. All that is required is that he will give them a glance after they have been manipulated, and see them as part of the patient—as much a part of him as his build and manner, the appearance of his retina, or the size and shape of the shadow of his heart. Even so, there will still be doubt because someone has to do the work. If the doctor cannot do it, who will? The problem has not been solved; it has only been transformed into a problem of organization that may be equally insoluble.

Our experience, however, shows that in this new form there is no longer any material difficulty. Since no nurses are assigned to our clinic, we asked for help from patients and friends. They were neither biochemists nor dietitians, but ordinary people who liked to work in a group of ordinary people to help other ordinary people. By contributing their ideas as well as their work they have helped us all to develop the laboratory and dietetic methods we describe to their present level of simplicity and reliability. It is true, of course, that the conditions in our out-patient clinic are in some respects quite different from those that exist in most doctors' offices, and that in some places it may be difficult to get such help as we received. But nowadays the fact that a doctor needs assistance if he is to work with efficiency and celerity is recognized in practice, so that almost every doctor has an office nurse who works with him in all sorts of ways. What we propose is that the office nurse be asked to undertake this most important and interesting work which already goes far beyond patients with renal disease and, in principle, can be extended to cover the examination of every patient. This would be the simplest possible clinical team—the patient, the doctor, and the office nurse. This book may be regarded as an introductory handbook for just such a group.

A reader familiar with the world literature on nephritis may be disappointed to find that no attempt has been made to connect our observations with those made by clinical investigators elsewhere. We can only ask him to remember that we are not trying to harvest all that is of value in this field. We are trying to say something new about out-patient and office practice. This is an enterprise so difficult and dangerous that we cannot afford to go far beyond what we ourselves have seen and know. Thus we make no references to any clinical facts that cannot readily be demonstrated in any doctor's office. This is a severe limitation, for though office work is admirably adapted for obtaining a broad survey of the development of disease over long periods of time, we cannot hope, with our simple methods, to explore deeply the status of any one patient at any one time or expect to be able to analyze in detail the interplay of the factors responsible for his immediate symptoms. That is the task of

the clinical investigator working with the patient when he is in bed and in a hospital. Office work has a quality that may be described as extensive. It can never hope to be intensive. So two books should be written about the treatment of glomerular nephritis—one embodying the experience of the practical doctor, the other the experience of the hospital physician. Both have their place and their use, but neither alone can encompass all that should be said on this subject; neither can present the problem as it really exists, in depth as well as in breadth. In spite of this limitation in our experience it may be hoped that some of the deeper understanding that is coming to us through investigations on patients in the hospital does pervade the text as an unacknowledged background for much that is said. And insofar as there is anything novel in what we say, a good deal of the confidence with which our views are presented derives from the feeling that they are only an inevitable development of what has already been said by others. We hope that it will thus become apparent to the scholarly reader that at every step we have been guided and supported by the work of our colleagues.

This, however, is not a textbook. It contains no compilation of methods used in the past in the treatment of glomerular nephritis, nor any detailed criticism of these methods beyond the implicit criticism that runs through the whole argument. We address ourselves to those who feel that in this matter we have not reached the textbook stage and that more investigation is what is needed now. The work done by our out-patient group is necessarily quite inadequate and calls for critical revaluation, both clinical and experimental. It is because it is so provisional and incomplete that we have had to present it in detail. Only thus could we hope that it might be extended and corrected.

We have asked to be heard because we have been subjected to conditions that are like those existing for the practicing physician. It may not seem quite reasonable if we make the same request again on the ground that some of us have spent more time in experimental work on rats than in clinical work with patients. It is true that we were obliged to go far back into general biology and that the relation between our experimental and clinical endeavor was not always obvious. Nevertheless, it was invariably the patients who raised the questions we tried to answer, and it was through combining clinical observations with experimental results that our opinions have been formed, and that the method of treatment we describe gradually attained some clarity of definition.

We include many experiments that have not been published. Ordinarily these would have been described in a series of papers and would have been sent to the technical journals with the names of those who executed the work, but all of us felt that there was some advantage in

having them appear in what we think is their proper clinical context. They were carried out by Mrs. Evalyn Barrett, Mr. D. D. Lee, Mr. W. Lew, Mr. L. J. Poo, Mr. W. D. Yuen, and the member of the group whose name appears on the title page of this book because he is responsible for the more speculative clinical sections, and because he has been able to remain a member of the group from its beginning, when it did not have its present composition, and when it was engaged in work that provided an indispensable foundation for this extension into the field of therapeutics.

We have said that the growth of efficiency in medical service depends on the development of well-co-ordinated teams or groups. The truth of this statement has grown sharper and clearer in the course of writing this book, which itself is nothing else than an account of the work of one such group, and an attempt to derive some immediately useful conclusions from what that group has done during the past twenty-five years. It would be pleasant if all those who, at one time or another, have been members of this group could meet again to consider the meaning of what we have done together. We should certainly not accept this book as it is now written but would fall at once to the task of tearing it into shreds. It would be thus that we should re-establish for ourselves the proper atmosphere of the group, which lives always at war with itself, no claim being ever advanced that does not meet its counterclaim, no thesis presented that does not promptly elicit its antithesis. It will always be so, because the group as a whole is not interested in anything that belongs to the past or the present, except to break it up and build, from any fragments of truth and usefulness that may remain, a new bridge into the future. The group still carries on, forever immune to the attempt of any individual within it to crystallize its meaning or bound its activities within the confines of a dogma. The facts it has brought to light will remain long after this particular formulation of their significance has been superseded and is forgotten.

T. ADDIS.

San Francisco, California

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