ALCOHOL AS A CAUSE OF RENAL DISEASE.

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IN my work on *Albuminuria*, I ventured to bring forward some facts which seemed to show almost conclusively that alcohol is not so prolific a source of renal disease as has been thought. The degree, indeed, to which disease of this nature has been attributed to drink is so extravagant as to carry its own refutation. It was said by a distinguished northern physician—and the saying has been often approvingly quoted—that in Edinburgh three-fourths, or even four-fifths, of the cases of granular degeneration could be traced to intemperance. And Dr. Roberts, who follows on the same side, though less numerical, is still, as I think, extravagant in saying, “The abuse of spirituous liquors ranks high—probably higher than any other single circumstance—as a determining cause of Bright’s disease.”

The tracing of disease to its cause is not always easy. It does not follow that, if a man who drinks whiskey become diseased, therefore the whiskey has caused the disease. A wide-spread habit must continually coincide with a wide-spread disease, unless the habit and the disease be incompatible. The *post hoc, propter hoc* delusion, or, in Hippocratic phrase, *the fallacy of experience*, is never more dangerous than in such a case as this. The habit has justly an ill name, and is apt to be wrongfully blamed. Because much drinking injures health, we are prone to attribute to it any disorder to which it has been antecedent. Hence, it is necessary, with more than ordinary strictness, to balance and compare. We must measure the drunken against the sober, all circumstances but drink being the same. To find the effect of alcohol upon the kidneys, we must look not only at the frequency of renal disease among British drunkards, but also have regard to the amount of the disorder which in this climate is inevitable, or due to causes other than drink.

Thus cautious, and trusting chiefly to *post mortem* evidence, as excluding clinical oversight and diagnostic error, I proceeded to examine the question. I found that persons who had died with delirium tremens had scarcely more renal disease than others, similar in sex, age, and station, but not known to have been intemperate, who were accidentally killed. Referring to the experience of Dr. Peters, of New York, and Dr. Ogston, of Aberdeen, both of whom had especially investigated the morbid anatomy of drunkards, I found that they had observed in these persons an amount of renal disease which, though looking considerable when taken by itself, was not very much greater than is usually found in as many *post mortem* examinations of the temperate. With these direct observations, I next appealed to the Registrar-General’s reports, and showed that in general terms there was no relation between drunkenness (as measured by deaths from delirium tremens and alcoholism) and renal disease. Many died of renal disease in places where few died of drink; and conversely, it often happened that where many died of drink, few died of renal disease. Thence, and from other evidence, which need not be restated, I formed the following conclusions.

“The use of alcoholic drinks is comparatively inoperative as causing disease of the kidney.

“Such fluids, when taken in great excess, may set up a state of renal catarrh; though, compared with other causes of tubal inflammation, this one is of rare operation, and almost insignificant in its numerical results.

“Certain liquors, by causing gout, may bring about granular degeneration, but this sequence is less common than when the gouty diathesis is connected with the absorption of lead.

“Granular degeneration may result from the extension to the kidney of a fibroid change, such as ardent spirit produces in organs more immediately exposed to its action—though this event is comparatively unfrequent, and the organic alteration, when it occurs, is generally less decided in the kidney than in the liver.

“But each of the issues which have been described is of rare occurrence. Alcohol attacks other structures in preference to the kidneys. As causes of renal disease, there are other agents which are greatly

more mischievous; and there is a great atmospheric power which overshadows all the other circumstances which tend to produce renal inflammation or granular change.”

Dr. William Roberts, of Manchester, than whom there is no one whose opinions on such a subject are more deserving of consideration, made these views the subject of attack in the *BRITISH MEDICAL JOURNAL* last November. He will have none of them. As for the kidneys, they shall not be excused; excuses shall not avail; not even in a small measure shall the old estimate of their susceptibility be lessened. The statements which I have made are so fallacious, or the reasoning so unsound, that, as Dr. Roberts says, “they are inadequate to disturb the old opinion respecting the effect of alcohol on the kidneys; and, in fact, the question remains just where it was before he” (*i. e.*, myself) “wrote his article.” Thus I have spent my labour in vain; it not only falls short, but goes for nothing—a discouraging result, if result it may be called—one which, though not unknown as regards criticism, is uncommon as the issue of the collection of facts, always supposing it to have been done in good faith. However, the respect which I entertain for Dr. Roberts is such that I would not hastily conclude him to be in error. When his remarks appeared, I was engaged upon a revision of the whole subject, in view to a forthcoming edition of my work, and I have postponed discussion until inquiries then pending were completed. I am now in a condition to resume the topic with the help of his strictures, and with a sense that, if I have gone wrong, there is no one more capable than himself of putting me right. I will first notice Dr. Roberts’ criticisms.

Condition of Kidneys.	Delirium Tremens :		Accident :	
	52 Cases.		52 Cases.	
	Average Age, 38.		Average Age, 41.	
Natural	...	28	...	32
Congested	...	14	...	5
Slight or Uncertain Change in Cortex	...	3	...	1
Large, Smooth, Mottled	...	3	...	1
Granular Surfaces	...	3	...	6
Cysts, without other change...	...	1	...	7

Comparing, in the table which is here reproduced, the state of the kidneys as found after death with delirium tremens and from uncomplicated accident, a slight excess only of renal disease appeared on the inebriate side. Upon this, Dr. Roberts announces himself struck, not by “the slightness of the alterations in the delirium tremens cases, but by the enormous proportion of serious organic disease in the accident cases”; and he adds: “If the delirium tremens cases be taken by themselves, they present a sufficiently strong indication of the evil effects of alcohol upon the kidneys.” What, to me, is more striking than anything else, is that Dr. Roberts should use such arguments. To take the delirium tremens cases by themselves is precisely what I decline to do. Comparison is the gist of the argument. Take the drunken cases alone, assign all their diseases to drink, and the indication of the evil effects of alcohol may well be called sufficiently strong. And thus, making no allowance for other morbid influences, we cannot fail, with Dr. Roberts, to be struck at the amount of renal disease among presumably temperate English adults. An insufficient allowance, indeed, for the amount of disease of this kind, which in this climate is inevitable, appears to lie at the bottom of Dr. Roberts’ estimate.

In the series of fifty-two presumably healthy men accidentally killed, occurred one instance of the large, smooth, mottled kidney, and six in which the kidneys had granular surfaces—a proportion of disease more astonishing to Dr. Roberts than to me. He asks “Is it to be believed that one in every seven or eight presumably healthy persons of the average age of forty-one is the subject of the granular kidney or of the large, smooth, mottled kidney? This point alone would seem sufficient to fatally vitiate these observations.” But is he not unconsciously testing pathological observations by clinical experience, forgetful that many or even most of the changes would make no sign during life? Many a smooth countenance goes with a very rough kidney. A perfectly healthy kidney, save in a child, is almost as rare as a clear conscience.

Since the table in question was compiled, in 1865, twenty-five adult males, accidentally killed, have been examined in St. George’s Hospital. Their average age was 40. The kidneys are thus described: natural, 13; congested, 3; having slight or uncertain changes in cortex, 4; large, smooth, mottled, 0; with granular surfaces, 5; cysts without other change, 0; a proportion of disease fully as great as in the list which is the subject of comment. Granulation, it will be noticed, was found in a fifth; and I believe it is not too much to suppose that of average Englishmen, aged 40, at least one in seven carries a kidney, the state of which he would regard with much dissatisfaction, could *clairvoyance* show it him and pathology explain.

Looking more widely, I ascertained that among 2,350 *post mortem* examinations at St. George's, covering a period of ten years, including both sexes and all ages without exception of any kind, comprising, therefore, many children, the kidneys were granular in 250; large, smooth, and mottled or highly congested in 107; thus about one-seventh being the subject of serious disease.

From all the evidence, therefore, I cannot but regard Dr. Roberts's surprise at the amount of disease on the non-alcoholic side of the comparison, as based on misconception, and insist on the justice of the comparison on which I have founded this part of the argument.

The same estimate, which I think I have shown to be insufficient, of the amount of renal disease inevitable in this country, has led Dr. Roberts to look with what I venture to call an exaggerated regard at the frequency of this disease in the bodies of drunkards, as related by Dr. Ogston, of Aberdeen, and Dr. Peters, of New York. Upon this point I need say no more, save to allude to a remark of Dr. Roberts which I scarcely understand. He says that, in considering the changes recorded by these authors, as found in the bodies of drunkards, "I seem to have been unconsciously biased by my individual views of the types of renal disease." It is true that I condensed and arranged the descriptions of disease for the sake of necessary comparison, but I did not put them into scholastic divisions; and, if I had, the facts would have remained. It would be only shifting from one pocket to another, a process neither impoverishing nor the reverse.

I now pass to the last of Dr. Roberts's criticisms. I showed that, according to the reports of the Registrar-General, kidney-disease and alcoholism bear no constant relation; disease of this organ being often rare in drunken, and common in sober districts. Upon this, Dr. Roberts says: "My surprise was great to find that the conclusion of Dr. Dickinson was substantially correct; and that the death-rate from Bright's disease was as high in the more sober districts as in the more intemperate districts, and frequently much higher." I am well content to be so far confirmed by the testimony of an adversary. But Dr. Roberts then proceeds to show what, in my turn, at first astonished me: that the reports do not display any correspondence between drunkenness and liver-disease. Drink, as he says, undoubtedly is a frequent cause of cirrhosis; but the reports do not show it. By parity of argument, may not drink with equal frequency cause kidney-disease, though the reports do not show it?

As to the Registrar-General's reports, the average diagnostic ability which dictates the certificates of death upon which they are founded, does not warrant them as absolute truth. Sometimes an approximation may be reached by the counterbalancing of misstatements; and now and then a very clear and evident fact may be visible, when one with a less definite outline is lost in obscurity. Albuminuria, as a sign of disease, is less liable to be mistaken than most others, whence the returns relating to the kidney may have a flavour of truth. But with other organs there is more guessing, and with none more than with the liver. The liver is the refuge for destitute diseases; to it are referred all disorders unclaimed by other organs, or which the information of the practitioner does not enable him to billet elsewhere. Diseases are assigned to the liver the most often by the most ignorant. Pneumonia, pleurisy, peritonitis, typhoid fever, tubercular meningitis, most general disorders, and many disorders localised in other organs, are not unfrequently attributed to this much maligned viscus, and treated through the portal system. Not long ago, a woman was dug up with a mortal dose of arsenic in her stomach. According to the certificate, she had died of purging and disease of the liver.

Practically, therefore, I should not expect the returns of liver-disease to exhibit much correspondence with fact; and, on reflection, I cease to be astonished that Dr. Roberts has found them wanting in this respect. The kidney-returns may be worth something more, though but little, as compared with pathological observations or even with the clinical experience of a credited observer.

To these remarks I will venture to add, in brief, the results of a further inquiry, in so far as it has to do with the kidney. The particulars were brought before a recent meeting of the Royal Medical and Chirurgical Society, and the courtesy of the editor enables me to refer to them in the current number of the JOURNAL (page 592). The morbid effects of alcohol were deduced from a comparison of the *post mortem* appearances of traders in liquor, potmen, publicans, draymen, etc., with those of persons not presumably drunken or conversant with gratuitous drink. In the alcoholic class the peculiarities were so marked and consistent through the organs, as to warrant the general accuracy of the demarcation. The *post mortem* appearances in 149 alcoholic and 149 non-alcoholic traders were compared. The state of the kidney in each individual in both series is stated in the table.

Another table is appended, showing how often symptoms were observed pointing to the kidney.

Analysis of the state of the Kidney, as found after Death, in 149 persons following Alcoholic Trades, and in the same number of persons circumstanced otherwise.

	Alcoholic.	Non-Alcoholic.
Natural	49	48
Congested	18	18
Enlarged and congested...	0	5
Coarse	3	3
Large and coarse	4	1
Fatty	2	1
Large and fatty	3	3
Slight or uncertain change in cortex ...	2	4
Large, smooth, mottled...	5	9
Small, smooth, mottled (cortex shrunk)	1	2
Lardaceous... ..	3	6
Slightly granular... ..	10	11
Highly granular	18	15
Granular + tubercles	1	0
Granular + cretaceous mass	0	1
Granular + pyelitis	1	0
Granular + stone in kidney	1	0
Cysts without other change	1	2
Cysts + depressions	3	1
Pyelitis	0	2
Scattered abscesses, with pyelitis ...	1	6
Pyæmic abscess	1	1
Abscess, unexplained	1	0
Tubercles... ..	8	4
Tubercles + cysts	0	1
One converted into cretaceous mass ...	0	1
Cancer	1	1
Fibrinous blocks	0	2
Stone, without other change	2	1
	149	149

*Analysis of the Symptoms referred to Albuminuria among the 149 Alcoholic and 149 Non-Alcoholic Cases comprised in the preceding Table.**

	Alcoholic.	Non-Alcoholic.
Prominent symptoms of renal disease ...	20	26
Renal oedema	14	18
Internal renal dropsy	4	6
Albuminuria discovered... ..	23	29
Cerebral uræmia... ..	5	3
Death directly due to renal disease ...	10	14

It will be at once seen that the kidney in the two classes shows no great disparity—a disparity far less than is exhibited by other organs. The total of kidney-disease is not increased by the alcoholic trades. The uniform changes of cortex which tend to albuminuria were slightly, and but slightly, more frequent under liquor, occurring in 86 persons of the alcoholic to 82 of the opposite series. The most marked increase under alcohol was with kidneys described as enlarged and congested, or large and coarse, conditions often undeclared during life. Granulation was also increased, but not to the same extent as cirrhosis, its hepatic analogue. Associated with, but not entirely dependent upon, this increase, there was also under alcohol a marked preponderance of cerebral apoplexy and hypertrophy of the heart, results of vascular deterioration.

Under the same influence of alcoholic trade, the "large, smooth, mottled kidney" (a description which may refer either to advanced tubal nephritis or to lardaceous infiltration) was diminished, as also was the lardaceous change of a distinctly recognisable sort. Thus the more rapid and obvious forms of renal disorder were in abeyance. Such increase as occurred was insidious and latent—rather exposed after death than traceable during life. Dropsy, external or internal, discovered albuminuria, and death directly attributable to the kidneys, were less frequent with liquor than without. Cerebral uræmia, the natural ending of granular degeneration, preponderated on the other side.

* In this table are only included the symptoms assigned to Bright's disease or albuminuria. Those of stone, tubercle, cancer, tumours, pyelitis, and abscesses, are omitted.

In deducing the effects of alcohol from this comparison, it is necessary to allow for the somewhat lessened exposure to weather, and also to accidental violence, involved in the alcoholic pursuits—exemptions which, so far as they go, would diminish the liability to tubal nephritis and to lardaceous change. This may help to explain the comparative rarity of these disorders with the alcoholic class. The increase in fibroid and chronic tubal changes must be referred to the liquor alone; but this increase is not great.

Taking all the facts together, I do not see how it can be doubted that the effects of alcohol in causing renal disease is less than used to be asserted: that to paraphrase Dr. Roberts, if he will allow me to do so, this agent does not rank higher, but lower, than other circumstances—to wit, external temperature and the absorption of lead—as a determining cause of Bright's disease. Although we can now and then trace the existence or aggravation of renal symptoms to intemperance, we do so with far less frequency than with disorders which have other sites. Pathological and clinical evidence combine to show that the evil influence tells more upon the nervous system, the liver, the blood-vessels, and probably in the production of tuberculosis, than upon the kidneys. The facts adduced are consistent with those formerly advanced, and with the quoted conclusions which have been the object of attack.

THE ADMINISTRATION OF ETHER, AND ITS SUPERIORITY TO CHLOROFORM AS AN ANÆSTHETIC AGENT.

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As the question of the superiority of ether over chloroform is daily attracting attention, any contribution towards our more efficiently applying this anæsthetic, and proving by facts its advantages, must be interesting. In the last two issues of the JOURNAL I have seen some remarks on the subject, and some more particularly in the last number, which are apposite, as they bear on the inconveniences which are met with in the method which has been most generally used, of administering the ether to a patient by pouring it over a sponge or towel. Although, no doubt, etherisation can be successfully induced by this method, every one who tries it will, I am sure, agree with Dr. J. W. Haward in his observations on the inconveniences caused by the administration of ether in the rough and ready method referred to, and then by force applying it to the patient. Mr. Clover, also, in his observations intimates that a mixture of air, as with chloroform, is desirable. This may be doubted. Dr. Squibb and Dr. Lente of New York, in common with others, believe that the more freely ether is administered the better will be the result. In this opinion I concur, and have found by experience that it undoubtedly acts best when not diluted with air.

In the pages of the *Medical Press and Circular*, I have written a series of papers, explaining the advantage of the method of using ether which I had arranged, and with which I had operated for a considerable time before, and which, I must add, I find to be perfection as an anæsthetic apparatus. It consists of an inhaler, furnished with a flexible diaphragm, which corresponds with the inspirations and expirations of the patient when being applied. The ether vapour is diffused over the interior by a framework covered with thin flannel, and is conducted to the mouth by a flexible tube and well-fitting mouthpiece. The ether is poured in, and its vapour is breathed over and over by the patient, as the flexible diaphragm allows sufficient mobility, and the effect is made known by the production of stupor and insensibility in a few minutes, varying from three to five or seven. By confining the ethereal vapour in this way, the odour or stupefying effect is not extended to the bystanders as it is in the rougher method of pouring the ether on a sponge or towel, and with even the assistance of a cone of pasteboard. The surgeon has usually to change his dress to get rid of the odour. There is also no necessity for violently overwhelming the patient; on the contrary, the patient may with a certain amount of confidence apply the mouthpiece himself at the commencement—a proceeding which renders the administration much more agreeable.

There is no chafing or irritation of the skin of the face or neck, as happens from the application of ether by the sponge or towel. There is no waste of ether as with the inhaler: two, three, or four ounces will suffice, where ten or fifteen ounces would be used by the sponge. The etherisation can, with a little caution, be used by the inhaler with candle or artificial light, where the more wasteful and diffused application by the sponge would be inadmissible, from the danger of igni-

tion. By the more direct application of the ether-vapour by the inhaler, there is less excitement than when the sponge or any other method which allows air to be more or less freely used with the vapour. The excitement and tediousness as well of etherisation being, I find, proportioned to the amount of air which is admitted with the ether-vapour, or, in other words, to the perfection of its administration: in fact, there is hardly any excitement stage; in the majority of cases, none at all. Sickness of stomach follows much the same rule; but if the stomach be empty at the time of etherisation, there is no sickness usually. In excitable females or nervous persons it does occasionally happen—certainly not more so, if as much, as in chloroformisation under like circumstances.

A remarkable advantage of ether over chloroform is that of its allowing a speedy reaction. After etherisation, a patient is in a few minutes perfectly restored; and, if the operation of it be not too severe, he will even walk from the operation-table.

We have in ether an agent which is proved by statistics to be eight times more secure than chloroform, and to be the safest of all anæsthetics in use. It has held its own for a quarter of a century in America, and confidence in its powers is unbounded. Certain inconveniences are to be remedied, such as—its diffusion among the bystanders; the local irritation caused by contact with the fluid; the waste of fluid; the excitement of delay; the danger of its use with artificial light. These are avoided by the use of the inhaler. There is a precaution worth bearing in mind, that the patient's head should be always well raised, specially in elderly and apoplectic individuals. Congestion of the face is to be watched in such cases, and discretion used in pushing the influence of ether or of any other anæsthetic.

I have used ether in hundreds of cases, and I have seen no ill effects. The day I etherised six patients in periods varying from three to five minutes, the anæsthesia was in all cases perfect and most satisfactory; the patients sometimes becoming insensible while applying the mouthpiece themselves, while the inhaler gets rid of all the inconveniences that have been found in the rougher and more wasteful method of applying by the sponge or towel.

The appended woodcut illustrates the mode of administration and the position which suits best.

ON ANÆSTHETICS.

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FULLY impressed with the truth of the main points ably insisted upon in the leading articles in this JOURNAL of the 2nd and 16th instant, on the question of ether and chloroform as anæsthetics, I am induced to state, as briefly as I can, the views which I have formed after having had somewhat extensive practical experience on the subject. And as another reason for troubling the readers of this JOURNAL with these remarks, I might mention my anxiety to keep up the interest of the profession in the superior claims of ether as an anæsthetic (so thoroughly do I believe in this fact myself) until all surgeons, and all engaged in the administration of anæsthetics, are induced to try it, and persevere in its use until they become proficient in its administration. I am quite sure that, if thus fairly tried, ether will soon replace chloroform in this country as the anæsthetic in general use.

I. In both public and private practice, I have administered chloroform to more than six thousand cases. I have used in its administration Snow's and Ellis's inhalers, Clover's bags, Skinner's apparatus,