ON THE USE OF DIGITALIS IN AORTIC DISEASE.

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IN Dr. Barrs's paper in the *BRITISH MEDICAL JOURNAL* of March 12th some theories have been advanced which are not considered orthodox and, if true, will disturb the lines of treatment recognised by our best, with some notable exceptions, clinical observers.

It is not my intention to discuss Dr. Barrs's able paper in its entirety, but I should wish to argue from clinical experience, as Dr. Barrs does, against some of the points which he has advanced.

First, I would point out that there is little or no comparison between the action of the aortic valve and that of the mitral. The strain on the former is, during diastole, much heavier, and more sudden and accentuated, than on the latter during systole. In other words, the closure of the aortic valve is akin to the slamming of a door, the strain commencing from the very moment at which systole ceases, whilst the apposition of the mitral folds is a gradual process, tension on the curtains to any marked extent occurring only at the end of ventricular systole, and is momentary only. And this seems to be more than emphasised by the existence in some of the lower animals not only of one tier but of several tiers of cusps in the aorta to withstand the rebound on the column of blood. The aovirdupois of about the equivalent to 6 inches of mercury has to be considered in the aortic valve, and this is supported not only by the cusps themselves, but as Savory has shown (and notwithstanding the tendency of modern physiology to disturb previously-accepted doctrines, this observation still seems to remain true), by the ventricle muscle itself. There is no such mechanism in the mitral valve, and therefore the incompetence of the two valves rests, to my way of thinking, on totally different physical bases. In the one it is always serious and alarming, in the other it may be, and frequently is, attended by only trivial inconvenience.

As Dr. Barrs admits, the danger in aortic incompetence exists from "sudden or gradual"—sudden rather than gradual—"failure of the left ventricle," a failure which, perhaps, is enhanced by a hitherto compensatory hypertrophy expelling a wave of blood with more power than normal into vessels of low tension, and the return of this volume must be accommodated not so much by "hypersystole" as by hyperdiastole of the left ventricle. For here, after all, is the rub. Undoubtedly digitalis increases the length of ventricular diastolic period. The heart, slowed by a course of large doses of digitalis, no longer repeats "one-two, one-two"; its muscular apparatus, after a more vigorous contraction, is slower in relaxing, and the sounds are converted into "one-two-o-o, one-two-o-o." Let anyone compare the sharp, short aortic obstructive *bruit* with the long sighing murmur like the noise of a distant blast furnace, and the difference will appear at once; and if this characteristic be further accentuated by digitalis, it is easy to see the risk of the drug.

But there are some cases of aortic incompetence in which digitalis may be given with advantage. Dr. Barrs speaks of cases of "uncomplicated double aortic disease which have been under his treatment for breathlessness interfering with work." It is within my experience that breathlessness is a more marked symptom in simple aortic regurgitation than when that condition is complicated by mitral insufficiency. But it is precisely in these latter cases, when cardiac failure begins to develop, that digitalis is of service. So long as the mitral regurgitation is present so as to allow of a "safety valve" overflow digitalis may be given with advantage and with safety. In the cases where there is aortic insufficiency alone the ventricle has to bear the strain of the return pressure; but given mitral incompetence in addition the over-distended ventricle is relieved by an expansile auricle, and accommodating pulmonary veins and plexus. It is said that this relief may be dearly purchased by a shortened life. Possibly so; but remembering the uncertain span of life in all cases of aortic insufficiency, remembering that even in the most promising cases sudden syncope may close the scene,

I do not quite see how we are justified in thinking that comparative comfort afforded by the addition of mitral regurgitation is a condition not to be desired.

Dr. Barrs's experience and also the experience of other able observers is in favour of digitalis as a remedy in aortic regurgitation. I cannot say that I have been so happy in mine. To quote one example. Some four years ago a young practitioner, who was a careful auscultator, came to me in distress owing to his having prescribed digitalis to three cases of pure aortic regurgitation, which were under his care at an infirmary. The result was disastrous in each case within 48 hours.

REMARKS ON THE USE OF HYOSCINE, WITH NOTES OF A CASE OF POISONING BY THE HYDROBROMATE.

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IN the *Journal of Mental Science* for July, 1891, Dr. Lionel Weatherly supports the use of hyoscine in cases of mental disturbance in which there are restlessness and excitement. "To rapidly subdue delirious and maniacal excitement," he says, "it is certainly a valuable agent in experienced and careful hands." But in cases of mental depression he obtained with it no good result. In one such case, which occurred in my practice about two years ago, and in which the depression was accompanied by great restlessness, I tried this drug, giving $\frac{1}{10}$ of a grain of the hydrobromate for a dose. In this quantity, however, it produced very little effect, the patient remaining quite wide-awake, and exhibiting his customary motorial fidgetiness. Nor was it until the dose was increased threefold—which was done gradually—that somnolence was seen, and this threefold dose ($\frac{3}{10}$ of a grain) had on most nights to be repeated in an hour before actual sleep was got. Thereafter the patient would generally obtain a good and refreshing sleep, and in the morning following would be distinctly better so far as the restlessness was concerned, though still somewhat drowsy. The melancholia was not appreciably affected in any way. Whether the abnormally large dose required here was due to an impurity in the medicine I cannot say. I had no opportunity of watching the effects on other patients of further quantities of the same sample. For, as several of the leading chemists in Edinburgh had not at that time any hydrobromate of hyoscine on hand, the friends of the patient had the prescription dispensed and forwarded by a London chemist. That the dispensing itself was not at fault may be inferred from the fact that, when the bottle was refilled from London, the dose of the new mixture required to be increased in the same way as the dose of the former had been. The results in this case, therefore, differ somewhat from those obtained by Dr. Weatherly. But the next occasion on which I tried hyoscine illustrates the reasonableness of Dr. Weatherly's warning against "the indiscriminate use of it as a powerful and sudden hypnotic."

I subjoin the notes of the case made at the time. The dispensing on this occasion was done by an Edinburgh firm.

Miss R. S., aged 33, exceptionally tall and robust, had been suffering for several days from slight ulceration of the throat and general *malaise*, which caused sleeplessness. For the latter symptom I ordered, on September 2nd, 1889: R Hyoscin. hydrobromat. gr. $\frac{1}{2}$; aquæ $\mathfrak{z}$ iv. Solve. Sig. Ten drops in a wineglassful of water at bedtime. To be repeated in an hour if patient still awake.

The patient received ten drops (about $\frac{1}{2}$ of a grain) at 9.45 P.M. Shortly after this she felt numbness of the body and limbs, extra dryness of the throat, and dimness of vision. Her face was flushed. She talked in a rambling fashion, but, as this was regarded as part of the legitimate action of a narcotic, no special notice was taken of it, and the friends left the bedroom in the hope that quietude might induce the patient to sleep. During this period, as I learned later, she was quite aware of what was going on, and evidently was conversant with the flight of time, for (there being a clock in her room) she got up at 10.45 P.M., an hour after receiving the former dose, and proceeded to measure out another dose for herself. She remem-

bers pouring out three drops carefully, but after that her hand shook, and she saw a quantity of the draught run into the measure glass. How much really ran into the glass she does not know, but subsequent examination of the bottle makes it unlikely that more than twenty drops in all had been poured out. However, believing that her calculation was approximately correct (that is, that she had measured off about ten drops), in which estimate it is probable she was fairly accurate, she added water to the quantity in the glass, and drank it off. The friends, hearing her moving about, returned to the bedroom, and learned what had occurred. Almost at once she began to talk nonsense as before, but now more rapidly and incoherently. After a time this behaviour grew so alarming that I was summoned.

I saw the patient at 12.45 A.M., about two hours after she took the last dose. She was, of course, still in bed. The pulse was small, 92 per minute. The pupils were dilated and stationary, not reacting to light. The respiration was sighing, and general depression was very marked. The face was not flushed, and, though the patient was delirious, she was not noisy, nor was there causeless laughter, as in poisoning by atropine.

By way of treatment I gave at once $\frac{1}{4}$ grain of morphine sulphate by means of the hypodermic syringe. At 1 A.M. the pupils had begun to react to light slightly, and the patient was then just asleep. At 2 A.M. the patient was sleeping quietly, but was easily roused. The pupils responded to light a little more markedly than they did an hour previously. Complaint was made of dryness in the throat.

Next day the patient was almost quite well again, but the pupils were still dilated, and the vision yet imperfect. During the next few days the recovery was rapid and complete, but fully three days elapsed before the pupils and vision had returned to their normal condition.

AN OUTBREAK OF ENTERIC FEVER APPARENTLY TRACED TO AN ANTECEDENT CASE AFTER AN INTERVAL OF TWELVE MONTHS.¹

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THE following summary of the circumstances attending an outbreak of enteric fever is the outcome of an inquiry, conducted on behalf of the Staffordshire County Council, in which I was assisted by Dr. Hosegood, of Cannock, the medical officer of health of the district in which the cases occurred. The interesting feature of the outbreak, which occurred in the autumn of 1890, is the long interval that apparently elapsed between the original introduction of the specific contagion into the district and its second manifestation.

The cases in question—seven in number—occurred within a period of two months in various houses in different parts of the parish of Church Eaton, and, with one exception—the last case that occurred—all the patients were, in one way or another, in daily communication with a farm house (the Church Farm) in which Cases iv, v, and vi occurred. The following are the circumstances attending each case:

CASE I.—A child, living a quarter of a mile away from the village, in whom the first symptoms appeared about August 20th. The mother of this child was laundrywoman at the Church Farm, and went daily to the farm, taking the child with her. Both mother and child were in the habit of taking meals at the farm, and drinking the well water there.

CASE II.—A child living in a cottage near to the Church Farm, in whom the fever showed itself about the same time. For some months previously the well belonging to this cottage had been dry, and during that time the family had been in the habit of sending for water to the Church Farm well.

CASE III.—A young man, living a quarter of a mile away from the village, who worked at the Church Farm, and who habitually took his meals with him to the farm, and drank the well water there. This case occurred towards the end of September.

CASE IV.—A youth who lodged at the Church Farm and who was attacked a day or two after the preceding case.

CASE V.—A child, the grandson of the tenant of the Church Farm, who lived at the farm, and who was attacked immediately after the previous case.

CASE VI.—A young man who worked on the Church Farm and lived in the house. This case followed the previous one very closely.

CASE VII.—A child living in a cottage in the village, some distance from the Church Farm. This was the only case that occurred in which the contagion could not be traced directly to the farm.

The farmhouse in question was not directly connected with the drains, so that the cause of the fever could not be traced to within the house. The outside arrangements corresponded with those generally found in farm buildings. The well, a comparatively deep one, sunk in the gravel and built in loose stonework (not set in cement), was close to the back door; it was covered over and had a pump attached. Within eight yards was the proverbial privy, with the large, foul, leaking cesspit, and, close by, a pigsty, the drainage from which lay on the surface. The house slop water was thrown on to an open channel on the paved yard close to the pump, and discharged into a catchpit, about five or six yards off, into which the manure heap drained, and from which there was an overflow pipe communicating with a cesspool some distance off.

The premises adjoined the churchyard, which, however, at its nearest point was 30 yards distant from the well and on a lower level. With regard to the quality of the Church Farm well water, an analysis had recently been obtained and it was pronounced to be good and fit for domestic use. As, however, no safe chemical standard of purity can be laid down, in order to ascertain whether the quantity of organic matter present corresponded with that in adjoining well waters, I analysed three samples and found that there was an excess both of free and albuminoid ammonia and also of chlorine in the Church Farm well water as compared with the other two, additions which could only have come from an impure source, namely, soakage from the privy or other collections of filth on the surface.

So far, then, I think we have conclusively traced six of the seven cases to the well water at the Church Farm; for, although three of the cases occurred in the farmhouse itself, and it might therefore be said that two of them had resulted from carelessness in dealing with the discharges of the other, this is contraindicated by the fact that only a few days had elapsed between the appearance of the disease in the three patients.

From whence, then, was the specific contagion introduced into the well? It was said that some years previously cases of "low fever" had occurred from time to time in the village, but for four years, with one exception, no illness of that nature had been heard of. To this exception it would seem the cause of the outbreak in question is to be attributed. In the autumn of 1890, twelve months previous to the subsequent outbreak, two cases of enteric fever occurred, one (imported) in the Church Farmhouse, and the other in a family whose milk supply came from the farm. From this time until the occurrence of the cases just reported, no case of enteric fever had been heard of in the neighbourhood.

To summarise the facts just related, After an interval of four years, during which time no cases of enteric fever had been heard of in the neighbourhood, a case was imported into the village, and gave rise, in all probability, to a second case. Twelve months then elapsed, and seven cases occurred in five different houses within a period of two months, six of these, if not the seventh, being directly connected with the farmhouse in which the imported case occurred a year before.

Was the second outbreak connected with the first, and, if so, why the long interval between the two? Had the interval between the original case and the second outbreak been shorter, one could easily have understood the connection, considering the proximity of the well to the privy cesspit, into which, no doubt, the discharges from the first case had been thrown. As, however, twelve months elapsed between the two, if we are to look upon the second outbreak as the outcome of the first, the contagion in the interval must either have lain dormant, or it must have taken that time to travel from the cesspit to the well, which is hardly likely, seeing that the one was so close to the other, and that the daily consumption of water must have been considerable.

As regards the outbreak in question, I venture to suggest that its sudden occurrence was the result of renewed vitality and not of evolution; that, in fact, the specific virus remained for twelve months in a dormant state until conditions favourable to its development were present. In any case, the out-

¹ Read at a meeting of the Staffordshire Branch of the British Medical Association.