

Indications for Research: IV

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Introduction

The Need to Think About Dryness

Nocturnal enuresis will continue to rouse curiosity and concern until all children over the age of five wake in dry and pleasant beds. That day will come sooner if time and thought is given not only to studying nocturnal enuresis and children who wet their beds, but also to dryness—to when children become dry, and to how and why each individual child becomes dry, whether this happens before or after the age of five. Further, although this book is mainly concerned with nocturnal enuresis, I believe such a change of approach would help to promote day-time dryness.

I would like to see a research review of the comparative biology of dryness in animals, including primates in their natural habitats and in zoos and laboratories, farm-stock animals, and domesticated pets.

Rethinking our Assumptions

Our current research—and indeed our thinking—on the development of nocturnal dryness is coloured by two assumptions: (i) that it is taught, and (ii) that if the training has failed, this is essentially because there is something wrong in the waterworks or the controls. Re-thinking of these assumptions is certainly a piece of research that is indicated. (We sometimes assume that research necessarily involves making new observations on two groups, one a control. But research can also be done by thinking, putting forward fresh ideas, and then testing these against existent knowledge.)

The morning on which children of age five wake dry will come sooner if we think harder and write more exactly. The terms 'development', 'maturation', 'teaching', 'learning', 'stimulus and opportunity', 'drive', and 'motivation' need to be used carefully.

It is too often assumed that when a new behaviour appears this is due to teaching. In our current state of uncertainty, it is better to avoid the phrase 'the child *learns* to be dry at night'. Even use of the word 'develop' is unwise in relation to dryness, because, as has been pointed out elsewhere (Mac Keith 1972), it is open to two interpretations. On the one hand, the phrase 'nocturnal dryness has developed' may be read or heard as having a descriptive meaning, *i.e.* that the behaviour expected to appear as the child grows older has, in fact, arrived. Alternatively, it may be taken to have an aetiological meaning, *i.e.* that anatomical and physiological growth and development have happened in the central nervous system as part of inherent, genetically-determined processes, and that *as a result* of this maturation the behaviour of nocturnal dryness has appeared. We can say the behaviour 'emerges' or 'appears', without implying causal factors of whose existence we are without good proof.

We need to remember the difference between symptoms, their underlying disorders, and the causes of these. We need always to remember that bladder control is influenced by a number of factors, and that some are positive and others negative. We may work towards some sort of cluster approach. Computer analysis may help, but I feel sceptical about the importance of correlations much below 0.5.

Is Nocturnal Dryness Learned?

I find it easy to accept as probable the idea that day-time dryness is learned, but I think it is time we stopped implying that night-time dryness is always due to the mother's training measures. (Buzzer training is in a different category, because its effects are so much more closely related in time to the wetting). I think we must stop speaking about 'enuresis', without specifying whether we are talking about nocturnal or diurnal wetting. Children 'potted' (in day-time) very early can have dry beds earlier (Young 1964), but severity in training leads to increased prevalence of bedwetting at five (Sears *et al.* 1957).

Day-time dryness is certainly taught, and it appears to be learned. Articles on toilet-training deal with the training of children who are wetting; they describe regimes for day-time wetting, and go on to discuss night-time wetting as if it were the same thing and as if similar training regimes were applicable. They *may* both be closely related or they *may not*; regimes useful for inducing day-time control may or may not be suitable for inducing night-time control. But it leads to confusion not to be exact in describing what is being discussed. I think it is certainly possible that nocturnal dryness is not taught but emerges, if it is given the chance to do so*.

Lovibond and Coote (1970), on page 374 of their book *Symptoms of Psychopathology*, state their view that dryness follows the development of cortical control, which they unequivocally say involves 'both neural maturation and the development of complex sequences of conditioned reflexes'. Curiously enough, on page 375 they imply that cortical control may happen from maturation alone, but this important possibility receives no further comment.

In their study of 2½-year-old children in Minnesota, Roberts and Schoellkopf (1951) found that 7 per cent, presumably *without any 'training'*, had developed night-time bladder control by the age of one year. It is often assumed that we teach our children to walk, but they would walk at about the same age given the opportunity but no training (Peiper 1963). In piglets, walking *and* control of micturition and defaecation are present at birth. Evidently it is possible that the development of nocturnal bladder control in some, or even nearly all, human children is inherent. A quarter of a century ago, Spock (1946) said 'staying dry at night is another thing that the bladder learns itself'.

*A research that might be done by someone with access to a good children's residential home is to investigate whether, if they were provided with opportunity but no or little stimulus, children of two and three would develop *day-time* control. Is day-time 'training' a way of providing 'opportunity and stimulus', or is the training an essential requirement of the child if he is to become dry in the day. The very successful training method described by Brazelton (1962), and the high proportion of 2½-year-olds taking responsibility for day-time dryness in the Roberts and Schoellkopf (1951) Minnesota study, are at least not entirely against the idea that without 'training', but without inhibitory factors, children would acquire day-time dryness.

Koehler (1953) said of walking that, once the necessary neuromuscular mechanisms have matured, 'the full ability is suddenly there'; it might be that the same is true of nocturnal dryness. It might also be that, as well as opportunity, a *certain amount* of 'stimulus' and 'training' are needed for the emergence of the behaviour. Although Muellner (1960), Lovibond (1964) and Turner (this volume Chapter 23) all discount the importance of training in the acquisition of night-time dryness, one should not ignore the remarkable success of Brazelton (1962), who had 98½ per cent of a series of 1172 children dry at the age of five.*

I would agree with those who suggest that nocturnal bladder control emerges without much training, perhaps without any. *But* an essential condition for its emergence will be the *absence of deleterious factors* likely to prevent it emerging.

Is Primary Nocturnal Enuresis Caused by a Disease 'Enuresis', with Disorder Somewhere in the Urinary Tract from the Hypothalamus to the Urethral Meatus?

We need to tolerate some degree of ignorance. Our inability to do so has, I think, deformed our thinking in this as in other fields. When a child is brought with the symptom of bedwetting, the doctor, disliking simple words, calls it enuresis, and then drifts into thinking there is a disease 'enuresis'. He has only substituted one name for another, but he feels that he is moving on in the analysis of the situation, and goes on to search for anatomical and physiological manifestations of this 'disease'. He comes up with discoveries like long foreskins, verumontanitis, urethral valves, emotional disorders, small bladders, and maturational delay. Like Westminster Abbey, the literature is filled with tombs of one-time ruling monarchs now dead.

I don't wish to imply that none of these have any importance. Some have none, others at times play a part as one of a cluster of contributory factors. There are children who wet their beds to express hostility, or because they are allergic. Others do have bladders which empty at low capacity, although in such cases the small bladder capacity may be secondary, being attributable to the fact that the child has never slept through a night without wetting, and has never had to hold a whole night's secretion of urine. Rutter has discussed this lucidly in the previous chapter of this volume, and some further comment is made later in this chapter.

Maturational delay is a poor explanation (Mac Keith 1972), and is too easy a way out. An absence of nocturnal bladder control in a child over five often used to be attributed to 'maturational delay', when the only evidence was the fact that he was still bedwetting. Of course, at the age of three or four years, maturational delay may certainly play a part in making the emergence of dryness more difficult, but after the age of five this is unlikely, because in nearly all children the mechanisms *have* matured by then (Mac Keith 1972). The 'maturationalists' have now shifted their attention from general neuromuscular mechanisms to 'delay in cortical inhibition of afferent stimuli from the full bladder'. This explanation may be true, but we should be reluctant to accept it without any supporting evidence; it is again too easy a way out.

The delayed maturation theory has the weakness that it is easy to put forward, yet difficult to prove, and there is some evidence against it. Nevertheless, it does seem that in comparison with non-enuretics enuretic children more often show other

*98½ per cent dry by age four has also been reported by Young (1964) in a series of 333 children.

signs of immaturity—such as late speaking, late walking, and, among fifteen-year-olds, puberty delay—and there is rather equivocal evidence of immaturity of electroencephalographic recordings. Here, indeed, is an indication for research, but it seems likely that delayed maturation will be found to play a part in only a small proportion of cases of nocturnal enuresis.

The Causes of Primary Nocturnal Enuresis

I believe that we may be spending too much time on studying children with established primary nocturnal enuresis, in our efforts to find existent factors to which the symptom may reasonably be ascribed. The search is likely to reveal factors which are causative only in a small proportion of cases, rather than the more common causative factors, as these are often no longer existent.

Let us define enuresis as bedwetting of a certain frequency after the age of five years. There is evidence from Young (1965), Cust (1958) and Douglas (this volume Chapter 15) to suggest that quite commonly nocturnal enuresis is caused by anxiety-provoking stresses, acting between the ages of 1½ and 4½ years, which inhibit the emergence of bladder control. (The report of Brazelton (1962) suggests that a notable proportion of these 'stresses' may well lie in aberrations of toilet training.) How 'stress' in, say, the third year, prevents emergence of the skill requires much research. But the error does not lie in the bladder, it is in the brain.

By the time a child is five and comes into our category of 'nocturnal enuretics', these stress factors and the various other factors together with which they act (*e.g.* delayed development of adequate bladder capacity, or delayed maturation of the necessary neuromuscular mechanisms) may well have disappeared.

Perhaps our thinking about primary nocturnal enuresis is sometimes confused by the fact that, although the behaviour—bedwetting—continues more or less unchanged from birth, the cause changes. Most one- and two-year-olds wet because their mechanisms have not yet matured; by the time they are four years old the necessary maturation will very probably have occurred, but adverse factors, notably stresses, may have interfered with the emergence of the desired behaviour.

By the time a child is six, he will have moved from the phase of the third year, in which 30 per cent of wet children become dry, into a phase in which only 13.5 per cent of wetting children become dry each year. Furthermore, although the child may not at first have shown concern, his mother is likely at some point to have begun to be worried about his ever becoming dry. By exerting greater pressure on him she is likely to have produced in him anxiety, which, in association with various contributory factors, is preventing him from becoming dry, and, indeed, has now become the cause of the wetting. It was a life-long symptom which, at the age of five, became a disease, *after* the stresses which at age three prevented the emergence of night-time bladder control had cleared up.

The Causes of the Continuance of Nocturnal Enuresis

Our studies of the physiology and psychology of the seven-year-old child with life-long bedwetting are unlikely to tell us the origins of his primary nocturnal enuresis. What they may be able to tell us are the factors which, at the age of seven, are *currently*

preventing the emergence of the desired nocturnal continence. They may be a sum of a continuing rather small bladder capacity, plus a genetic tendency, plus recent excessive pressure by the mother. The emotional factor may of course be one, such as family bickering, which was the adverse stress at the age of three or four, and is still present. Looking at the current situation of the six- or seven-year-old from this point of view could be more rewarding than a more traditional approach. This is another field in which research is indicated. It might mean that we could then use our buzzer treatment with fewer factors present likely to hamper its success.

How Should We Describe the Disorder Underlying Nocturnal Enuresis?

In crude outline, the idea is as follows. The period from $1\frac{1}{2}$ to $4\frac{1}{2}$ years is a relatively sensitive (not critical) one for the emergence of nocturnal bladder control (Young 1965, Mac Keith 1968). During this period, the percentage of wet children becoming dry at night is somewhere between 20 and 40 per cent each year. From the age of five, it is rather lower, being about 13.5 per cent per year.

When a child is between one and $4\frac{1}{2}$ years old, a combination of factors, including stress, can prevent the emergence of dryness, and if they do the child enters the after-five phase with 'primary enuresis'. It seems to me possible that this inhibition of the emergence of nocturnal bladder control before the age of five years is a frequent cause of bedwetting after the age of five. But although the five-year-old child has this symptom, he is very likely normal in all other ways. His necessary mechanisms have matured; his bladder capacity is sufficient; he has no other disorder of urinary system; he commonly has no emotional disorder, or if he has it is secondary to his bedwetting.

I find it difficult to find terms to describe the disorder that seems to me to be likely to be a frequent factor in the origins of nocturnal enuresis. I am sold on the idea that like walking and bladder control in the piglet (see Chapter 1), and like walking in man, nocturnal bladder control in man is inherent, is in the DNA, and is 'one of the things the bladder learns itself'. It seems to me to be different from reading, which is certainly taught, or from speaking, which needs stimulus before it emerges. Nocturnal bladder control certainly emerges in some children without any training. For those children who are dry, presumably opportunity was needed; possibly in certain children some stimulus helped. But nocturnal bladder control by age $4\frac{1}{2}$ is not 'taught' in any ordinary sense. What has happened in the child enuretic at age five, is that emergence of a behaviour has somehow been prevented by early stresses, probably often in combination with other contributory factors.

I believe that children 'enuretic' at age five commonly have no disorder of the urinary tract nor any emotional disorder. I am reluctant to call it a failure of learning, because I do not think that learning in any ordinary sense is usually needed. To call it a habit disorder seems to me to explain nothing, but this may be because I have not the necessary familiarity with learning theory nomenclature. The disorder seems to me to be in the cortex, rather than in the urinary mechanisms. And let me stress once again the need to see that, though the factors responsible for lack of nocturnal bladder control at the age of $4\frac{1}{2}$ may be the same as those preventing the emergence of the skill at the age of seven, they are commonly different.

Does the Cause of Continued Wetting Lie in the Child or in his Parents?

Brazelton's anecdote about his son (see page 281) confirms the idea that there is a danger in expecting always to find disorder in the child. Clinicians have all had children who become dry once their doctor had identified the specific fears of mother and child, put them into perspective, and given a general exposition of the natural history of bladder control.

Some seven-year-old primary enuretics are worried by their wetting, or are otherwise emotionally disturbed. They deserve treatment. But there are others, brought by worried parents, in whom there is no good evidence that they, the wet children, have any emotional disorder.

A possible research project would be to take a group of such children and give treatment restricted to treatment of their parents. The study might even be restricted to families where some earlier treatment has failed. The idea of *not* doing intensive physical investigations on, nor even treating, the child himself would have some advantages. Fewer boys would have unrewarding cystoscopies, fewer boys would be repeatedly reminded by questioning, pills, and visits to clinics that they are failures.

It might be that intensive parent therapy was effective in only a small proportion, but, if it worked, the boy would have 'cured himself'. The 'parent therapy' for nocturnal enuresis could be tried for a short period, and then recourse made to a well-planned buzzer therapy.

Further Indications For Research

Definitions: a Basic Need for Planning Research

As has been said above, we need definitions not only of 'enuresis' 'development', 'maturation', and 'stress' and 'anxiety', but also of 'training', 'teaching', 'opportunity and stimulus' and of 'drive'. We need to know and to specify what we mean when we use these words.

We need to be more careful in the use of the terms 'primary' and 'secondary' enuresis. Miller *et al.* (1960) published data showing that 29 per cent of boys wetting at the age of five years have had long periods of dryness. Are these children to be called 'primary' enuretics because they have not been dry for a whole year?

Epidemiology

We need longitudinal studies of when, how and why children become dry. For each child there should be an assessment of the atmosphere of the homes (using the Tizard's methods, mentioned by Rutter in his Indications for Research—see page 299), of the parents' bladder control history and attitudes to bladder control, and of the mothers' methods of 'training' (not only what she uses and when she uses it, but also how she does it).

We need to know at what age each child has his or her first dry night, for that announces the onset of maturation of neuromuscular mechanisms. Boys more commonly have nocturnal enuresis. Is this because of a sex difference in the age of maturation of mechanisms, such that girls enter a 'sensitive period' for emergence of this behaviour earlier, and so spend longer in this sensitive period?

We need to know the details of each child's general growth and development, if we are to discover how closely late walking, talking and puberty *etc.* are associated with late dryness.

Mechanisms

We need to know the amounts voided at each micturition by the child as he grows older, and whether these amounts commonly increase when the child, either spontaneously or as a result of some treatment, comes to be able to sleep through the night without wetting.

Genetic Factors

Apart from Hallgren's (1957) great work, most genetic studies are sketchy, or concern relatively few families. Primary and onset enuresis, and nocturnal and diurnal wetting, are not sufficiently separated to identify in which types genetic factors are or are not important. We need to confirm or correct Hallgren's finding that a family history of wetting is associated less with wetting by day and at night than with wetting at night only. It seems probable that his finding might well have to do with genetic transmission of late maturation of neuromuscular mechanisms.

Bakwin (Chapter 9) has postulated another inherited syndrome, that of the irritable bladder. This too needs further research for confirmation or correction.

Hallgren (1957) also noted that where husband and wife both had a history of bedwetting, the marriage was more likely to break down. This seems to be an indication for further research, for it suggests an environmental rather than a genetic cause of the bedwetting in their children.

Development of Bladder Awareness and Control

Duché's elegant observations set out earlier in this book (see Chapter 2) are of great interest for understanding the physiology, maturation and emergence of both day-time and night-time bladder control; the infants he observed when they were 'but a few months old' appear to have woken from sleep to announce their need to urinate.

A repetition of these observations would ideally lead on to a follow-up study of the children's later development of bladder control, and of the differences between the later histories of the infants who did and did not wake before micturition.

Learning to be Dry at Night

Spock (1946) suggested that the 'bladder learns itself', without help from outside. We need to study how.

The possibility that, provided there are no adverse factors, nocturnal bladder control emerges more or less spontaneously is a real one that we need sharp verbal tools to dissect. We must be more careful to avoid saying 'the child learned' unless we have good evidence that he did learn.

What is required is observation of children over the first five years of life, with an attempt to identify helpful *and* adverse factors related to the emergence of nocturnal dryness.

What sorts of stress seem to prevent its emergence? Douglas (this volume, Chapter 15) has made a major addition to our knowledge in this field. Brazelton's (1962) success with a 'child-oriented' regime of toilet training strongly suggests that many of the adverse stresses lie in the mismanagement of toilet-training, rather than only in such events as the birth of a younger sib, an operation, or going into care, though these are associated with a sizeable proportion of enuretics.

Are there other skills commonly emerging, like nocturnal dryness, in the third year? Are these affected in the children in whom, because of 'stresses', nocturnal dryness does not emerge. This topic would probably be better studied in relation to transient stresses than to stresses which continue.

The earlier observations of Cust (1958) and Young (1965) and those of Douglas reported in Chapter 15 of this volume cry out for confirmation or refutation.

We need to repeat the study reported by Brazelton in 1962, in which 98½ per cent of the children were dry at five years of age. Anyone taking on this task in the U.K. needs to be aware that, while in the USA the middle-class mother may well look to her paediatrician for guidance about elimination training, in the U.K. such guidance is likely to be an area in which the grandmother's views carry greater weight than do those of the consultant children's physician. However, that Brazelton has highlighted a highly important indication for research is evident. This is both for its theoretical implications (though these are not essential to the theory that maturation has occurred by age five in nearly all children) and also because this work offers us the most attractive possibility of prevention of what is now much too common a trouble.

Emotional Factors in Persisting Failure to Become Dry at Night

I have noted above the need to differentiate in children aged five years or more between what prevented night-time dryness emerging before the age of five and what is currently preventing the emergence of the behaviour. It seems probable, both from theoretical considerations and the occurrence of occasional dry nights, that the basic bladder control mechanisms are mature in children aged over five years.

Why are the reported successes with the buzzer so different? Again we need to know not only what the mother does to train the child and when she does it, but also how she does it.

Could one recognise these children who, if their own and their parents' fears were pointed out and explained and then put in perspective, would without further treatment become lastingly dry at night?

In children and adults with some forms of incontinence of urine, the doctor's listening and conveying he is interested leads to a reduction in the frequency of wetting. Would it be possible to identify children whose only emotional insecurity was about their wetting? It seems that many enuretic children do not have emotional disturbance across the board; are there some with 'insecurity' only in this area, who are otherwise coping well? Can these children be identified in surveys?

The Use of the Buzzer

However interested one may be in the origins of enuresis and in preventing it arising, there are and will be many children with this symptom. Further research

studies of the use of buzzer treatment are desirable. It seems to be the most useful treatment. It seems that the success rate and diminution in relapse rate is related to the attention paid to details, such as are noted by Sylvia Dische in Chapter 24 of this book. Young and Morgan's recent description of overlearning by giving water to drink appears likely to be a useful way of reducing the proportion of relapses (Young and Morgan 1972).

The use of amphetamine as an adjunct to buzzer treatment deserves further study. It might act by reducing depth of sleep or by improving conditionability, but its empirical value deserves further study, if only for those cases who, despite careful attention to detail, do not respond to buzzer training.

There are certain difficult groups, such as adult institutionalised mentally handicapped people, whose lives would be improved by bladder control. Azren and Fox (1971) have described a rapid method of training for day-time dryness by reinforcement. Some of those who developed day-time control began to get up spontaneously at night, but not all. The use of a similar approach in association with well-planned buzzer training seems another research that is indicated.

The Concept of Small Bladder Capacity

The use of drugs in treatment will attract attention from some research workers. I wish to remind them of the studies of Vincent (1959, 1964), who has produced evidence suggesting that 'we may also consider the commonly postulated "low bladder capacity" a false idea.'

There is a tendency to look for the origins of a 'small bladder capacity' in the wall of the bladder and in its sphincter, and hence in drugs which affect these. Perhaps it would be profitable to give more attention to the pelvic floor. Vincent (1959, 1964) has shown that to a considerable degree the symptoms of 'low bladder capacity' or of 'irritable bladder' originate not so much in the bladder or its sphincter, as in the effect on the bladder's functioning of the position of the pelvic floor. 'A high proportion of the adult "irritable bladder" patients get instant relief from their frequency and urgency by raising the pelvic floor artificially...' Muellner (1960) has also emphasised that it is the descent of the pelvic floor which is the stimulus to the detrusor to contract. While it may be that Vincent's perineal pneumatic pressure device is not practical for use in children with nocturnal enuresis, the physiological implications of his work deserve very earnest consideration. It may be that if we want relief of nocturnal enuresis in children, drugs which act on the bladder sphincter, if there are such, will be less efficacious than drugs or other methods, for example electrical methods (British Medical Journal 1972), which are found to raise the pelvic floor.

It may also be that, in assessing the cholinergic action of a particular drug with the aim of using it to affect the functioning of the bladder, evidence of how it affects salivary secretion is not necessarily significant. The story of our failure in asthma to get useful results from skin testing, as opposed to nasal and bronchial challenge, may have a lesson to give us here.

Conclusion

I hope that general paediatricians in the community health service and general

practitioners will find among these 'indications for research' items that they will take up and study. I hope that they will discuss their plan of research with a statistician before they embark on it, and I hope that in planning it they will also discuss it with their local paediatrician—and that he will be sympathetic and helpful—and with their local psychologist, for psychologists have training in the planning of research.

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