

which are poorly matched to the demand unit. While a frequency mismatch between patient and pacemaker may be uncommon, a combination of this with borderline amplitude may explain the increased incidence of inappropriate fixed-rate pacing in sick sinus syndrome.

Thus, in summary, failure of inhibition of demand pacemakers may be expected to be more common in patients with sick sinus syndrome than in those with heart block because of a greater likelihood of myocardial infarction causing a fall in amplitude of the input signal to the pacemaker. Patients with sick sinus syndrome and a normal scalar QRS duration may have a sensed QRS in which the frequency distribution does not match the demand pacemaker.—We are, etc.,

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- 1 Radford, D. J., and Julian, D. J., *British Medical Journal*, 1974, 3, 504.
- 2 Sutton, R., Chatterjee, K., and Leatham, A., *Lancet*, 1968, 3, 645.
- 3 *British Medical Journal*, 1973, 2, 677.
- 4 Sutton, R., in *Cardiac Pacing*, ed. H. I. T. Thalen, p. 421. Assen, van Gorcum.
- 5 Norman, J., and Sutton, R., 10th International Conference on Medical and Biological Engineering (1973).

Indomethacin-aspirin Interaction

SIR,—We were interested in the paper by Dr. P. M. Brooks and others (12 July, p. 69), as their observations on the effect of aspirin on indomethacin plasma levels are similar to those of Champion *et al.*¹ but different to those of Jeremy and Towson,² and different also to our own.^{3,4}

Jeremy and Towson found that aspirin given "concurrently" decreased indomethacin absorption. Champion *et al.* found "concurrent" administration of buffered aspirin (Bufferin) did not significantly decrease indomethacin absorption. Dr. Brooks and colleagues found that soluble aspirin (300 mg aspirin, 30 mg citric acid, 100 mg calcium carbonate, and 3 mg saccharin sodium) given concurrently did not significantly decrease indomethacin absorption. We found that pretreatment with, and simultaneous administration of, a buffered aspirin (Bufferin) increased the rate of indomethacin absorption.

Simultaneous administration of certain antacids with an acidic anti-inflammatory agent, naproxen, has been shown significantly to increase naproxen absorption (Segre *et al.*⁵). Ambre and Fischer⁶ have shown that coadministration of a weakly acidic coumarin drug with magnesium hydroxide produced higher and earlier peak plasma levels than when the drug was given with water.

Possibly the concurrent use of buffered aspirin by Champion *et al.* and partially buffered aspirin by Dr. Brooks and colleagues accounts for the difference between their results and those of Jeremy and Towson, who apparently used a plain aspirin preparation. We have suggested that the increased rate of absorption of indomethacin found in our studies was due to the simultaneous administration of a buffered aspirin, perhaps causing local changes in gastric pH, thereby possibly increasing the dissolution rate of indomethacin. Perhaps Dr.

Brooks did not administer the two drugs simultaneously; the results would be consistent with this.—We are, etc.,

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- 1 Champion, G. D., *et al.*, *Clinical Pharmacology and Therapeutics*, 1973, 13, 239.
- 2 Jeremy, R., and Towson, J., *Medical Journal of Australia*, 1970, 2, 127.
- 3 Garnham, J. C., *et al.*, *British Journal of Clinical Pharmacology*, 1974, 1, 178.
- 4 Garnham, J. C., *et al.*, *European Journal of Clinical Pharmacology*, 1975, 8, 107.
- 5 Segre, E., *et al.*, *New England Journal of Medicine*, 1974, 291, 502.
- 6 Ambre, J. J., and Fischer, L. J., *Clinical Pharmacology and Therapeutics*, 1973, 14, 231.

Sexual Life after Gynaecological Operations

SIR,—In his paper on sexual life after gynaecological operations (14 June, p. 608, and 21 June, p. 680) Mr. A. G. Amias suggests (in Part I) that "some of the poor results of hysterectomy can be directly attributed to the harmful effects of newspaper medicine," and also that "erroneous notions about the operation are fostered by opinionated and ill-informed comment in the lay press."

Might we be given references to support this? We normally expect to find such references to other published material drawn upon for learned articles; why should we not be given them when they derive from the lay press? I am particularly interested since for nine years I have been writing a regular medical column for the 6m. readers of *Woman's Own*—a far from negligible section of the lay press. So I can say with some confidence that this magazine certainly has not during this time produced "erroneous notions about the effects of the operation [of hysterectomy]." Nor, to my knowledge (obviously I take a close professional interest in the matter), have other women's magazines. On the contrary, I have, like my medical journalist colleagues, been at some pains to reassure readers that the operation certainly is not the end of the road as far as sexual satisfaction or enjoyable living is concerned, while at the same time making the point that neither is it a panacea for all ills—that it cannot, for example, cure an unhappy marriage or a clumsy lover—a point with which few doctors will disagree.

That there is a need for such articles in widely read popular magazines is undoubted. A very large section of the readership writes to us seeking "the reduction of fear, anxiety, and guilt by explanation and sympathy" which Mr. Amias says is so vital, yet which they fail to obtain from the people who perform their operations and look after them during recovery. May I therefore suggest to Mr. Amias that he has not perhaps studied the lay press of which he is so scornful quite as carefully as he might have done? Perhaps, on this occasion, we have been treated to "opinionated and ill-informed comment" published in the *medical press*.—I am, etc.,

CLAIRE RAYNER

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** We showed Mrs. Rayner's letter to Mr. Amias, whose reply is printed below.—ED., *B.M.J.*

SIR,—Mrs. Rayner is welcome to leaf through the pages of "Doctor's Own," but would she kindly note that my words were aimed at practising doctors whose job it is to see and treat real patients face-to-face every day. They will not require references to remind them of countless people frightened by a "medical" item in the press—such a common experience that no one sets down chapter and verse every time it happens. The main purpose of my paper, however, was not to assail Mrs. Rayner or her paper but to urge the medical profession itself to explain as well as prescribe.

The need for this is underlined by the glimpse Mrs. Rayner affords us of her own prose style on the effects of hysterectomy. We are treated to an array of bland generalizations complete with the obligatory female snigger at "a clumsy lover," which no doubt impresses her 6m. proxy patients but not, I suspect, a worried woman with a problem unique to herself. The "need for such articles in . . . popular magazines" is very genuinely doubted by me. To satisfy Mrs. Rayner's yearning for a reference may I dedicate the following to her from an unimpeachable (lay) source: "The lady doth protest too much, methinks."—I am, etc.,

A. G. AMIAS

- 1 Shakespeare, W., *Hamlet*, act III, scene 2, line 242, c. 1601.

Normal Sexual Response

SIR,—I have read Professor R. W. Taylor's interesting paper (7 June, p. 543) and wish to challenge large parts of its content as being erroneous, unsubstantiated (because it is largely not open to substantiation), and containing more that is fantasy than fact.

In the first place Professor Taylor makes the grievous but common error of divorcing "sexual activity," from "reproductive activity." This error is entirely fundamental as it relies on the hedonistic quality of the sexual act instead of the "whole thing" of reproduction, of which coitus is the physiological linking mechanism. The reproductive act is a whole physiology in its own right, commencing at spermatogenesis and oogenesis, linking at coitus, and terminating (for the male) in ejaculation and (in the female) at parturition.

Professor Taylor then makes the modern error of making the male and the female similar, as he states clearly in his opening paragraph. Nothing could be farther from the truth. The male and female are not homologous but heterologous—that is, "of equivalent nature, but different in sex." He then goes on to describe certain physiological responses—the erection of the nipple and/or clitoris during coitus—that are no part of my experience nor those of my patients. Quite frankly I don't believe a word of it. The clitoris is in fact a functionless vestigial remnant of an organ inappropriate and useless to its owner's sex (like the male nipple). Again, it is heterologous with the penis, not homologous with it, as is usually (but wrongly) believed.

Finally, without defining it, Professor Taylor implies that orgasm in the female is similar to, or homologous with, orgasm in the male. Again this is absurd. Orgasm is the climax of the sexual act. In the male this is characterized by the expulsion of the

product of the gonads by reflex muscular effort, or it is that reflex which would expel these products were they present in sufficient quantity and were the appropriate pathways clear. This activity is known as ejaculation and the female has no homologue of this activity, which is unique to the male.—I am, etc.,

T. RUSSELL

Hayes, Middx

** We showed Dr. Russell's letter to Professor Taylor, whose reply is printed below.—Ed., *B.M.J.*

SIR,—It is interesting that having challenged some of the things I wrote as being incapable of substantiation Dr. Russell then goes on—in the sentence—to claim that what I say is more fantasy than fact. This must surely be equally difficult to substantiate.

It is true in one sense that coitus is but part of a complete reproductive act. I might go further than he does and say that the care of the offspring is also a part of the process. However, to reach the conclusion he does is to ignore the possibility that with the advent of human sensibility orgasm has come to have a satisfaction, and perhaps indeed a purpose, quite separate from that of reproduction. The fact that there is no clear pattern to the acceptability of intercourse by the woman would suggest this. Evolution is occurring in patterns of behaviour as well as in our physical attributes.

The physiological response to sexual stimuli, including the contraction of the muscle deep to the areola and the erection of the clitoris, have all been demonstrated and well documented in the sources I quoted, so to prove that they do not occur an equally impressive body of experimental evidence must be cited. Equally, the occurrence of changes in the shape of the vaginal vault and contractions of the levator ani muscles and of the uterus which accompany the female orgasm are also documented by research workers of good reputation. If one was to seek a purpose for these changes it may very well be found in the speed with which spermatazoa can make their way into the Fallopian tubes. It is, I think, too much to postulate that they always make their way by their own power. The "orgasm" of the female can at least assist the process. However, speculation on these lines is likely to be as unprofitable to me as it appears to be to others, so I will desist.—I am, etc.,

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Picking a Diuretic

SIR,—Your excellent leading article "Picking a Diuretic" (7 June, p. 521) glosses over, I think, the most important advantages of the "loop" diuretics. I write late as I had expected a more brisk response in your columns.

The advantage of a quick-acting diuretic is that the time of day can be chosen when it is socially as well as medically most convenient. I usually recommend that it be taken at 5 p.m., when a diuresis is unlikely to interrupt work and when the patient can be resting with his feet up to enhance the efficacy of the diuretic. Further, during the

diuretic phase the patient is not so likely to compensate by taking large quantities of fluids, and so he goes to bed at his most dehydrated. As you so rightly mention the reduction in nocturia—often the first sign of falling cardiac output—can be gratifying to the patient.

Surely we give a diuretic not for the cosmetic advantage of reducing ankle oedema but to reduce the complications of left ventricular failure? The virtual absence of paroxysmal nocturnal dyspnoea since I started to give frusemide at 5 p.m. has not only given the patients better nights but me. Yes, thiazides for hypertension but a quick-acting diuretic at the appropriate time for oedema. *Choose the right time* and the dose, and cost, can be reduced. Surely this was the conclusion that Dr. A. S. Playfair (5 July, p. 42) could have reached.—I am, etc.,

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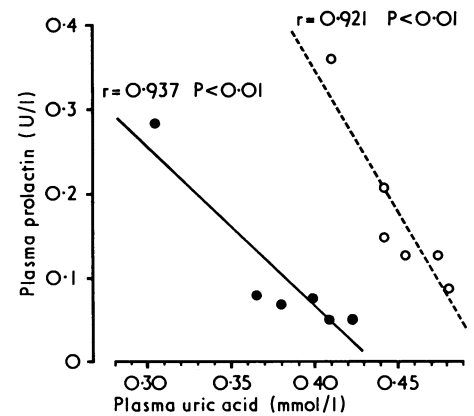
Prolactin and Uric Acid in Man

SIR,—The relationship between high levels of prolactin in plasma and high or increasing plasma uric acid concentration recently reported in patients with pre-eclampsia by Dr. C. W. G. Redman and others (8 February, p. 304) suggested that prolactin might have a direct influence in the control or modulation of uric acid metabolism or excretion in pregnancy. We have examined the relationship between plasma prolactin and plasma uric acid in normal subjects in the unstressed state, and again after inducing increases in the level of plasma uric acid by administration of frusemide followed by sodium restriction.

The concentrations of prolactin, determined by radioimmunoassay,¹ and uric acid were measured in plasma obtained from six male volunteers (aged 20-25) at 6 p.m. Frusemide 80 mg was then given orally and the subjects consumed a diet containing 20 mmol sodium and 100 mmol potassium for 38 hours. After 38 hours the sodium deficit was 255 ± 60 mmol (mean \pm S.E.M.), and at this time measurement of plasma prolactin and plasma uric acid was repeated.

The initial unstressed plasma prolactin correlated negatively with the plasma uric acid ($r = -0.94$, $P < 0.01$) (see fig.). After acute sodium deprivation there was a rise in plasma uric acid from 0.36 ± 0.05 mmol/l (6.1 ± 0.89 mg/100 ml) to 0.45 ± 0.03 mmol/l (7.6 ± 0.55 mg/100 ml) ($t = 5.88$, $P < 0.01$; Student's t test for paired observations). Plasma prolactin (initially 0.13 ± 0.088 U/l) also increased to 0.18 ± 0.097 U/l ($t = 4.03$, $P < 0.01$). However, this rise would be within the expected range of normal circadian variation in man. There was still a correlation between the plasma prolactin and the plasma uric acid ($r = -0.92$, $P < 0.01$). The slopes of the two regressions were not significantly different but there was a significant difference between the y intercepts ($P < 0.001$).

These results confirm that there is a relationship between plasma prolactin and plasma uric acid. Under the present experimental circumstances, however, the relationship was in a direction opposite to that observed in pre-eclampsia. It seems unlikely that the plasma prolactin has a consistent role in determining uric acid disposition as the re-



Relationship between plasma uric acid and plasma prolactin in 6 male subjects (—●—●—) and in the same 6 subjects after a sodium deficit of 255 ± 60 mmol (mean \pm S.E.M.) (—○—○—).

Conversion of SI to Traditional Units—Plasma Uric acid 1 mmol/l \approx 16.8 mg/100 ml.

lationship has differed in each of the situations examined. Probably some more fundamental process is influencing both prolactin and uric acid disposition. A further understanding of this might provide an insight into the physiological significance of prolactin in man.—We are, etc.,

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E. COLE

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¹ Cole, E. N., and Boyns, A. R., *Hormone Research*, 1973, 4, 261.

Coxsackie Viruses, Muscles, and Exercise

SIR,—Your correspondent Dr. J. V. Jones (12 July, p. 100) may perhaps consider himself lucky. Dr. D. Lewes and colleagues¹ recently suggested that myalgia in acute viral infections might be an indicator of silent myocarditis. They drew attention to reports of deaths in circumstances suggesting that in man as in mice exercise during acute infection might enhance the severity of the disease.^{2,3} Coxsackie viruses belong to the same enterovirus group as polioviruses, and there may be an analogy with the enhancement of the severity of paralysis by strenuous exercise during the early phases of poliomyelitis. "Work if off" seems to be a dangerous injunction in systemic infections. Childbirth combines strenuous exercise with hormonal influences and post-partum myocardiopathy may well be due to exacerbated coxsackie-virus infection.⁴—I am, etc.,

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¹ Lewes, D., Rainford, D. J., and Lane, W. F., *British Heart Journal*, 1974, 36, 932.
² Sutton, G. C., et al., *Aerospace Medicine*, 1967, 38, 66.
³ Monif, G. R. G., Lee, C. W., and Hsiung, G. D., *New England Journal of Medicine*, 1967, 277, 1353.
⁴ Sainani, G. S., Krompotic, E., and Stodki, S. J., *Medicine*, 1968, 47, 133.