

### Dietetics of Pregnancy

SIR,—Your correspondents Mr. W. T. Russell and Dr. W. J. Martin (Feb. 13, p. 204) are right in supposing that my knowledge of statistical technique is imperfect. It is, however, sufficient for me to appreciate the cogency of their correction and to be able to agree with them that the sampling error in the work of the People's League of Health is even greater than I stated. Now the purpose of my contribution to the discussion at the R.S.M. was to examine critically the claims made by some recent workers to have influenced the incidence of premature birth by dietetic methods. I suggested that the results of the work sponsored by the P.L.H. had no significance. If the statisticians who examined them now state that I have ascribed to these results a higher degree of significance than they ever claimed, it seems that there is no point at issue between us.

As regards the data which I presented before the R.S.M., the abbreviated report does not, perhaps, make the point as clear as it might be, but I should have thought that with the aid of the published histogram the facts could be deduced. These facts are that the periods of gestation recorded in my series of 147 observations are normally distributed around a mean of 41.2 weeks, with a standard deviation of about 12 days; this *does* lead to the expectation that one-eighth of all instances will terminate before the beginning of the 40th week. Of course, a normal curve is not symmetrical about a line which is not drawn through its mean point; even I, the imperfect statistician, do not expect that.

Your correspondents' comment on the failure of the P.L.H.'s dietetic measures to influence the mean birth weight—"especially as all premature babies are not subnormal in weight"—affords a perfect illustration of the way in which the main object of a piece of work can be forgotten if realistic standards are not set at the start and rigidly maintained. Infants die from immaturity, not from estimated prematurity.

While I fully realize that Mr. Russell is not responsible for the choice of experimental method, surely the fact remains that the estimated period of gestation—estimated presumably from the last alleged menstrual period—is too crude and unreliable a yardstick by which to measure the small differences which these dietetic experiments have achieved. To desire earnestly that some means, dietetic perhaps, may be discovered for the prevention of premature birth and the wasteful loss of infant life associated therewith is not the monopoly of the P.L.H. or of any section of opinion; but to prove the efficacy of such means is another matter. It would not be unfair, I think, to liken these advocates of dietetic methods for the abolition of prematurity to the leader of the deputation who closed his interview with the Minister of State with the words, "And those, Sir, are the opinions upon which I base my facts."—I am, etc.,

London, W.1.

DOYNE BELL.

SIR,—Dr. Doyne Bell's histogram does not represent a normal curve but a skew curve conforming to Pearson's Type I. Neither a normal nor a symmetrical curve can be expected to describe the length of gestation, since it is possible to have larger deviations below the mean than above. But even if we assume a normal curve, his corrected values of a mean of 41.2 weeks and a standard deviation of 12 days will not lead to 1/8 of the distribution falling before 40 weeks, but gives 24% of the total as occurring before 40 weeks. Perhaps Dr. Doyne Bell omitted to express the deviation from the mean in terms of the standard deviation when entering the tables of the probability integral.—We are, etc.,

W. T. RUSSELL.

London School of Hygiene and Tropical Medicine.

W. J. MARTIN.

### Ligature of the Innominate Artery

SIR,—Sir W. I. de Courcy Wheeler has asked me to put details of this case on record on the grounds that only about seventy have been reported, though there must be many more unpublished.

A woman of 67 came to the Leicester Royal Infirmary on account of a swelling above the right clavicle; this had been first noticed six months before. The right arm had recently felt dead and its power was diminishing. On examination there was an expansile swelling above and behind the middle of the right clavicle; the

right hand showed clubbing of the fingers and slight wasting of the intrinsic muscles compared with the opposite side. The grip was impaired on the right; there was no delay in transmission of the right radial pulse; the reflexes in both upper limbs were normal. Pupils reacted to accommodation but only sluggishly to light. Kneckerks were absent. There were clean-cut ulcers with yellowish bases on the inner and lower parts of both tibiae. Wassermann reaction ++; Kahn reaction ++++. X-ray examination of right shoulder girdle showed no bony change.

Operation, May 24, 1940: Incision along line of lower part of sternomastoid, out along clavicle, and down upper part of deltopectoral groove; lower ends of right sternomastoid, sternohyoid, and sternothyroid detached temporarily; middle third of clavicle divided. Aneurysm of the innominate artery extending into the beginning of the subclavian; scalenus anterior and part of brachial plexus thinned out over aneurysm. Innominate artery ligatured, and pulsation in aneurysm immediately stopped; tributaries of sac ligatured; three-quarters of sac excised and remainder oversewn; no drainage.

Post-operative course: Middle third of clavicle failed to unite; wound otherwise healed soundly. No signs of cerebral damage appeared at any time; no pulsation or thrill recorded in the aneurysmal site; pain in the arm persisted but was lessened; some diminished power of grip persisted in the right hand.

—I am, etc.,

Leicester.

DONALD MCGAVIN.

### Pleurisy and Pleural Effusion

SIR,—From the observation post at the Pleural Effusion Unit established by the L.C.C. at Queen Mary's Hospital, Sidcup. I wish to make a few comments on Wing Cmdr. R. R. Trail's very welcome article (Jan. 23, p. 98). The subject of pleurisy and pleural effusion is becoming more important, not only because of the increase in the number of cases concomitant with the general increase in tuberculosis, but also because idiopathic and secondary tuberculous pleural effusion appears to follow a more serious course than it used to, probably owing to the lowered general resistance of the average patient in wartime.

The importance of correct appreciation of the symptom pain is rightly stressed by Wing Cmdr. Trail; I would like to add that pain on laughing and on yawning can be an early symptom of pleurisy, just as it is one of the last to disappear.

In the differential diagnosis no mention is made of rheumatic pleural effusion, which can present some difficulties. I think mention should be made also of the so-called sulphapyridine effusion, as this term is starting to creep into medical language. Pleural effusion due to the action of sulphonamides does not exist; it has never been reported in the course of treatment of non-pulmonary conditions by these drugs. It is possible, though difficult to prove, that chemotherapy has caused an increase in the number of metapneumonic serous effusions, as some of these effusions would have become purulent in pre-chemotherapeutic days; that is all. It must be admitted that the differential diagnosis between pleural effusion and pneumonia with effusion can be difficult; some cases of primary effusion look really ill and are profoundly toxic, and have not "the appealing look, the clear complexion, and the bright eye of the average tuberculous patient." The great value of a white cell count can be offset, if not resorted to early, by the depressant effect on the leucocytes of sulphapyridine, which is nowadays frequently administered blindly.

We attach much significance here for the diagnosis *a posteriori* of tuberculous pleural effusion to the finding in the radiograph of grossly enlarged and fluffy hilar glands, frequently more conspicuous on the opposite side to the effusion. The persistence of these shadows three months or more after the onset of the effusion appears to be of fairly serious prognostic significance. It is, of course, a wise precaution to perform a tuberculin skin test in every case in which the tubercle bacillus has not been demonstrated.

Few people will agree with Wing Cmdr. Trail's statement that "the radiograph of the transudates of cardiac and renal failure generally shows bilateral effusion, small in extent, and confined to the costo-phrenic angles." In congestive heart failure the effusion is often of considerable size, and is much larger on the right side. To distinguish between a transudate and an exudate at the bedside, Rivalta's test can be of real aid; one or two minims of the fluid to be examined are dropped into a glass container filled with 1:100 acetic acid; a smoky cloudi-