The choice in treatment lay between radical surgery and, if the patient was unfit for excision of the vulva, teleradium. In fact, with modern anaesthesia, few elderly women were unfit for surgery. He hesitated to disagree with Mr. Way, but he did not think that in the majority of cases it was necessary to remove the iliac glands, which were involved only very late. He also drew attention to the Stockholm technique, especially a paper by Berven in 1947 on electrocoagulation of the vulva and the application of teleradium to the groin. With electrocoagulation there was surprisingly little shock and pain, and a good deal of sepsis was avoided.

The first general principles for treatment of cancer of the vulva were that the whole vulva must be excised, preferably with the diathermy knife, or else destroyed by electrocoagulation. He preferred the former technique in most cases. The inguinal glands must always be removed. It was possible to start treatment with teleradium one week after excision.

Radical Excision

Mr. ALAN BREWS said that he encountered very few cases of malignant disease of the vulva, but like Mr. Way and many others he was dissatisfied with the poor results of superficial surgical procedures and with radiotherapy. In recent years he had been practising a moderately radical surgical excision of the vulva, with block resection of superficial and deep lymphatic glands from both groins. He had performed this type of operation 12 times—on an average two or three times a year. He had always used combined spinal and light general anaesthesia. He had used diathermy extensively for all skin incisions, and coagulation for all but the largest vessels. He had aimed at the removal of all superficial inguinal and femoral glands; he had not as yet removed the external iliac gland. He had always removed the upper part of the internal saphenous vein. The wound was kept at rest for as long as possible. Only after three weeks, with healthy granulations everywhere, did he allow the patient up, wearing a loose linen garment. She was then sent to a convalescent home for about six weeks, when healing was usually complete.

His cases were too few and recent for useful statistics, but he thought the results encouraging. He had had one primary death, with very extensive local recurrences within a month of the original operation, but he had had no other serious complications as yet. The absence of pyrexia and serious infection and the complete freedom from pain had been notable. His small series included two cases of malignant melanoma; both died within a year of operation, one with a mass of recurrences in the bladder and the other with widespread visceral secondaries in the lungs and liver. He endorsed Mr. Way's plea for concentration of these cases in a few units where an experienced team would give each patient a greater chance of useful survival. It might be that in each region only one such team would be needed.

After some general discussion, Mr. WAY, in reply, said that he could not help feeling that there was still an element of caution abroad. On the subject of palpable nodes, 36% of his cases in which nodes were not felt in the groin were nevertheless found to have cancer. The Berven treatment relied on the palpation of lymph nodes to ascertain whether or not there was involvement. He had never seen lymph-node involvement respond to *x*-ray or radium therapy. Until the cautious attitude towards the surgery of carcinoma of the vulva was overcome results would not improve. He added that this was a disease which could be practically eliminated by the surgical treatment of leukoplakia beforehand.

The Minister of Health is worried about the amount of time spent by members and officers of local authorities attending conferences. A letter from the Ministry asks local authorities to send representatives only to those conferences at which attendance is desirable in the interests of the council and that the number sent is no more than is sufficient for this purpose. Save in very exceptional circumstances the Minister would not sanction payment for sending representatives to conferences held abroad. The Minister hopes that councils will use their influence where possible to limit the duration of conferences and to see that they are held in reasonably accessible places.

Correspondence

Jenner and Medical Science

SIR,—Sir Edward Mellanby's oratorical contempt (May 28, p. 921) will not suffer him even to name the man whom Bulloch (not Bullock) thought the most learned man he ever knew. Poor Creighton! I must face alone, with such fortitude as I possess, the charges of forming "imbalanced" ("unbalanced"?) and wrong judgments, of having little knowledge of and making little allowance for human nature, and of killing Kruger with my mouth. I do not suppose many people have read my book—it is out of print and likely to remain so. I am not sure Sir Edward has read much of it. I dare say righteous anger led him to cast it aside before he reached my final conclusion that "Jenner was, directly or indirectly, the means of saving many hundreds of thousands of lives"; I added, "That is a less grandiose conclusion than some others have reached, yet, I submit, quite enough to entitle Englishmen to take pride in the recollection that Jenner was their countryman."

However, I still think Jenner was loose-thinking and, ultimately, fact-blind (not fact-blinded, as Sir Edward writes), and to explain how I reached this imbalanced, or unbalanced, judgment will not take much space. In 1798 Jenner wrote to a foreign physician: "At present I have not the most distant doubt that any person who has once felt the influence of perfect cowpox matter will ever be susceptible of that of smallpox."

As Sir Edward so rightly says, we must judge a man by what was known, or probable, when he wrote, not by what later experience proved. When Jenner wrote, it was known that natural smallpox did sometimes attack inoculated persons, and instances of two attacks of natural smallpox in the same person had been recorded. It was, I suggest, loose thinking to infer from his experiments that the new prophylactic was more efficient than the "natural" prophylactic. He might reasonably have hoped that it was equally efficient and less dangerous.

It was, I suggest, to be fact-blind to have written a quarter of a century later, after all that had happened:

> "My opinion of vaccination is precisely as it was when I first promulgated the discovery. It is not in the least strengthened by any event that has happened, for it could gain no strength; it is not in the least weakened, for if the failures you speak of had not happened, the truth of my assertions respecting those coincidences which occasioned them would not have been made out."

Sir Edward tells us that "the test of the acceptability of a discovery depends upon whether it is true and not whether it seems sensible or even whether it can be verified by the statistician." Or, in other words, a discovery is acceptable if it is a discovery. Certainly many discoveries—e.g., in the field of ethics—cannot be verified by statisticians, or even by biochemists, but is the discovery of a prophylaxis one of them? Pearson and Woodville in Jenner's time did not think so; the Medical Research Council in our time does not seem to think so.—I am, etc.,

Loughton, Essex. MAJOR GREENWOOD.

Treatment of Basal-cell Carcinoma

SIR,—In our article in the *Journal* of April 30 (p. 737) we stressed in the title the "especial reference to lesions on the neck, trunk, and limbs." For this reason we made no further reference to the 210 cases in which the tumour was situated on the face or scalp.

It is as a primary treatment of rodent ulcer that we especially recommend surgical excision. The following two tables are compiled from the personal case records of one of us (C. W.). They refer to basal-cell carcinomata on the face and scalp treated between 1915 and 1935: a complete "follow up" was conducted in 1938.

The first table shows the results of surgical excision as primary treatment of rodent ulcer; they are surely better than those shown by radiotherapy. The statement that "there should be no recurrence with adequate surgical excision" is

TABLE I.—*Results of Treatment by Primary Excision*

	No. of Cases Treated	Not Traced	Died of Intercurrent Disease	Recurred	Recurrence Free	Percentage Free from Recurrence
Patients treated between 1915–1925	51	18	10	1	22	96%
Patients treated between 1925–1935	62	20	16	0	26	100%

TABLE II.—*Results of Treatment by Excision Following Failure of X-ray Treatment*

	No. of Cases Treated	Not Traced	Died of Intercurrent Disease	Recurred	Recurrence Free	Percentage Free from Recurrence
Patients treated between 1915–1925	25	10	4	5	6	54%
Patients treated between 1925–1935	35	14	7	8	6	43%

more than an "expression of hope." If the excision is adequate there is no pathological basis for recurrence—all the tumour cells have been removed. Professor D. W. Smithers's figures (May 14, p. 865) show how difficult it is to eradicate all the tumour cells by radiotherapy.

The second table illustrates one of the special hazards of radiotherapy. Recurrences following *x*-ray therapy as the primary treatment are much more difficult to eradicate. Further recurrences are only too liable to occur. This is the sort of case that Professor Smithers will pass over to the surgeons. Perhaps this is what Professor Smithers means when he says, "Surgeons and radiotherapists do better by working together." In many fields we are only too ready to agree with him, but not in the treatment of basal-cell carcinoma.

We do not agree that irradiation gives a better chance of cure in lesions on the eyelids. Surgical excision will cure the patient, with far less chance of recurrence than irradiation. In the past irradiation produced a good cosmetic result more often than surgery; but with the development of plastic surgery the cosmetic results of surgery can now compete with those of irradiation.

So often, and Professor Smithers's figures show how often, irradiation results in delay in cure and makes further treatment more difficult. Therefore we recommend that all rodent ulcers should be excised when first seen. And if the patients are "worried" they may be "premedicated," for it is worth curing them at their first treatment.—We are, etc.,

CECIL WAKELEY.
PETER CHILDS.

London, W.1.

SIR,—On reading the recent correspondence concerning the treatment of rodent ulcer I was reminded of an interesting case, a female mental patient, who was admitted to hospital at the age of 51 in 1938.

At this time she showed a pale scar in the left infra-orbital area of the face about which she was unable to give any information, and as there were no signs of any pathological activity it was ignored. Towards the end of the year 1944 a new lesion was observed, this time on the forehead over the left eyebrow, but it showed little reaction and did not appear to warrant much closer investigation. However, its chronicity led to the calling of a consultant, who diagnosed rodent ulcer in 1947, and radium therapy was prescribed. This was carried out in the Christie Hospital, Manchester, the patient visiting for sessions of treatment once a month, commencing in September, 1947.

In December of that year a third ulcer appeared in the right infra-orbital region, and therapy was applied to this also. In January, 1948, the irradiation produced a conjunctivitis and blepharitis on the right side necessitating local palliative treatment, and the sessions were discontinued temporarily. In March, 1948, a further lesion appeared on the left side of the nose, which was watched carefully; surprisingly it regressed, and healed of its own accord. In June, 1948, the lesion over the left eyebrow succumbed to a simple pyogenic infection, which was treated locally with gentian violet with success, and in September the irradiation therapy was discontinued, as the ulcers had healed.

The patient has been examined by the consultant at three-monthly intervals since, and no further activity has been observed, nor has there been any glandular involvement up to March, 1949. She will be seen again in June, but the prognosis is considered very good.

The interesting point about this case seems to be the recurrence of the same disease in different but close areas, and the possibility of minute traces of active pathological tissue being still *in situ* after apparent healing gives rise to the suspicion that direct spread has insidiously taken place. The directions of spread are such as to make this supposition improbable, so that one is forced to assume each lesion was a neo-neoplasm *per se*. Perhaps some reader might clarify this point.—I am, etc.,

Winwick, Lancs. P. A. M. ROBERTSON.

Penicillin-resistant Staphylococci

SIR,—Dr. N. G. B. McLetchie (April 23, p. 725), in criticizing our use of the word "strain," has raised a matter of general interest. He appears to wish to limit its use to that usually given to "type."

The word appears to have two slightly different meanings, both of which may be used without ambiguity. This was done by Dr. G. Martyn (April 23, p. 710), who writes, "The high incidence of penicillin-resistant strains probably reflects the strain circulating in the hospital." Here strain in the second place bears the meaning "kind" or "sort," and suggests that had typing methods been available the circulating strains might have belonged mainly to one particular type.

Allison and his colleagues use "strain" in a quite different sense from "type"—a type including many strains from different patients. In summarizing an outbreak of staphylococcal food-poisoning these authors state that "the 125 strains submitted or isolated were typed by serological and bacteriophage methods. . . . 113 belonged to the serological type III, phage type 6/47" (*Mon. Bull. Min. Hlth.* 1949, **8**, 38).

It is legitimate to regard any single organism on a swab as potentially a strain, just as in horticulture each individual seedling in a stand may give rise by vegetative propagation to a clone. It is here that difficulty arises in comparing the figures given by different authors. While one worker may accept into a series several strains from swabs and cultures from different parts of the body of a single patient, another will add only one to his collection. The figures will then require very careful analysis if a comparison is to be made.

We have taken a total of 315 strains from 241 persons. This in practice means that each patient gave at least one strain and sometimes two. The second was added if some definite difference in colonial appearance could be observed and reproduced in subculture, or if some definite difference in penicillin sensitivity or in coagulase production were present.—We are, etc.,

A. VOUREKA.
W. HOWARD HUGHES.

London, W.2.

Transfusion Compatibility Tests

SIR,—A recent case observed at hospital illustrates how an error in rhesus grouping, together with reliance on the saline compatibility test, led to the administration of rhesus-positive blood to a rhesus-negative individual previously immunized by repeated blood transfusions.

A man aged 34 had suffered from ulcerative colitis since 1944. On four previous occasions 19 pints (10.8 litres) of group A blood had been transfused without any reaction other than mild pyrexia without rigor; on the last occasion the temperature after transfusion had reached 102° F. (38.9° C.), and a mild asthmatic attack had occurred. On April 6, 1949, the patient was again admitted for transfusion. An experienced laboratory technician recorded his blood group as A rhesus-positive, and accordingly a compatibility test using patient's serum and donor cells suspended in saline only was employed. This showed no incompatibility after one hour at 37° C. The blood was accordingly issued as compatible. After about 50 ml. had been transfused the patient experienced a rigor and complained of severe throbbing headache with pains all over his body. The temperature rose to 102° F. (38.9° C.). In view of this reaction the ward sister stopped the transfusion after 150 ml. of blood had been given. Recovery was uneventful. The following day a sample of patient's serum was again incubated with the donor cells, after which an indirect Coombs test was done. This showed very strong agglutination of the donor erythrocytes. Subsequent investigation showed the patient to be in reality rhesus-negative, and his serum to contain anti-Rh (anti-D) blocking antibody active against the cells transfused in a dilution of 1/80. The donor cells suspended in 25% bovine albumin were strongly agglutinated at 37° C. by his serum