electroencephalographic activity is lowered as unconsciousness replaces full awareness.

The observed variables in the electroencephalogram are amplitude, frequency and pattern, both spatial and temporal. A high level of activity can scarcely refer to the last, and must be concerned with frequency or amplitude, probably the latter. While it is true that in all the four listed states of unconsciousness there is a general tendency toward lowering of frequency, in the lighter stages of anæsthesia (particularly after barbiturates) and of sleep, there is usually an initial rise in frequency. Amplitude, on the other hand, except in the most profound unconsciousness when 'mind' has long since left its home, commonly rises.

As an example of the mistaken premise on which Prof. Eccles's hypothesis is based one might take the case of 'flat' electroencephalograms. These are unusual, but not rare, in normal fully conscious adults; no activity can be recognized with an amplitude of more than a few microvolts. If the possessor of such an electroencephalogram goes to sleep, it is likely that quite a pronounced rise in amplitude of both alpha rhythm and 12–14 c./s. sleep spindles will occur, followed by slow irregular waves with an amplitude of 50–100 μV.; at no time will there be any lowering of activity until the subject is once again fully awake.

It is a pity that Prof. Eccles's attractive proposition should contain such a statement as "an explanation is provided for the invariably observed failure of mind–brain liaison when the electroencephalogram reveals depressed cortical activity".

Nothing in the chapters of mine to which he refers in support of the hypothesis set out above seems to me to bear the interpretation put upon it, and I can only regret any ambiguity of expression which allowed such a conclusion.

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I regret that Dr. W. A. Cobb takes exception to my quotation of his article in "Electroencephalography" in support of my statement that the unconsciousness of concussion is associated with a depressed activity of the cerebral cortex as revealed by the electroencephalogram (E.E.G.). The misunderstanding apparently has arisen because Dr. Cobb is considering merely the wave form of the electroencephalogram and I was specifically referring to the "activity in the [cerebral] cortex as revealed by the electroencephalogram". I must confess that electrophysiologists must accept responsibility for such misunderstanding, because we have given the electroencephalographers very little help in the interpretation of their electroencephalogram records, and they perforce have invented a descriptive nomenclature and empirically related the different types of wave forms to various cerebral disorders. In this context reference may be made to my review on the "Interpretation of Evoked Potentials in the Cerebral Cortex"[1]. An attempt is there made to show how these potentials may be explained by the known electrical responses of nerve cells. The relevant point for the present discussion is that the electroencephalographers assess activity by simple reference to the electrical wave forms they record, whereas my phrase "activity in the [cerebral] cortex" refers to an assessment of the level of active neuronal responses in the cortex. Admittedly there is much uncertainty in such assessment, but there is good evidence that intense patterned neuronal activity would give a very small and rapid electrical wave-form, whereas large slow potential waves indicate a low level of activity which would be largely attributable to synaptic potentials that are generated in cortical neurones by afferent discharges to the cerebral cortex. A further point is that recording from the scalp, as is usual in clinical electroencephalography, is so inefficient that the small rapid electrical activity is likely to be lost in the general noise background, as presumably occurs in the so-called 'flat' electroencephalogram on which Dr. Cobb bases so much of his argument.

An electrophysiologist would place much more weight on records directly from the cerebral cortex. My reference to Dr. Cobb's article in "Electroencephalography" referred to his account of concussion, in which he referred specifically to the work of Williams and Denny Brown on concussion in animals. Complete silence of the electroencephalogram was observed, and is a highly significant observation because it was directly and immediately recorded from the exposed cerebral cortex. The reported recordings in cases of human concussion were always made much later (usually many hours) after the concussion, but they too gave a general indication of lowered neuronal activity. On carefully re-reading Dr. Cobb's article, I can only say that to me it bears precisely the interpretation that I originally placed upon it in my article.

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[1] *E.E.G. Clin. Neurophys.*, 3, 449 (1951).

Chairs of Chemistry in Great Britain

In *Nature* of January 26, Prof. Alex. Findlay rightly challenges the claim that the regius chair of chemistry in the University of Glasgow founded in 1818 was the first chair of chemistry in Britain. Had he confined himself to Scottish chairs, his plea for the recognition of the Aberdeen chair founded in 1793 would have been valid, but surely he goes too far in claiming the latter as the first in Great Britain. At least one chair—that which, under the revised title of organic chemistry, I have had the honour of occupying since 1944—antedates the Aberdeen foundation by close on a century. According to the University Ordinances, the chair of chemistry in the University of Cambridge was created (albeit with no stipend) in 1702, its first occupant being John Francis Vigani, who had been settled in Cambridge for some years before that time as a private tutor in chemistry. Vigani's successors in office (with dates of their election) were: J. Waller (1713); J. Micklebourgh (1718); J. Hadley (1756); R. Watson (1764); I. Pennington (1773); W. Farish (1794); S. Tennant (1813); J. Cumming (1815); G. D. Liveing (1861); W. J. Pope (1908). An account of the history of the chair and of the University Chemical Laboratory was published in 1928 by Dr. F. G. Mann (*Chem. and Indust.*, 6, 690 (1928).

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Jan. 28.