measurement of the production of acid-soluble ultraviolet-absorbing material. If any such material is released, the lot is not offered for sale. In addition, heavy metals may not exceed 5 parts per million. All lots sold by Schwarz/Mann, including the one referred to by Greenberg *et al.* (*1*), meet these specifications. There is no ribonuclease, and heavy metals are less than 5 parts per million.

We apply these strict specifications because of the many varied and critical procedures in which this sucrose may be used. The Schwarz/Mann density gradient sucrose used by Greenberg *et al.* came from a lot purchased in 1972 (lot 3927). Using his procedure, we confirmed the turbidity and isolated the material responsible for it. Our investigations showed the presence of a polysaccharide, possibly amylose or cellulose. This conclusion was drawn from the fact that the material (i) has no appreciable ultraviolet absorbance at 260 or 280 nm, (ii) has an infrared spectrum similar to that of saccharides, (iii) no amino acid content (except for small amounts of methionine), and (iv) responds positively to classic starch tests.

As a result, all lots of Schwarz/Mann density gradient sucrose sold from October 1972 through the present (Y1205, Y1229, Y1488, Y1562, Y1668, Y1786, Y1862, and Y3102) were reassayed by the alcohol-water test (*1*). All lots were found free of this material. In addition, this test will be included in assaying all Schwarz/Mann density gradient sucrose prior to release for sale. The enzyme grade mentioned by Greenberg *et al.* is not recommended for use in density gradients.

It is regrettable when any researcher finds one lot of our sucrose unsuccessful in his procedure. When this happens, Schwarz/Mann offers to replace the material involved. If the procedure has merit, Schwarz/Mann includes the assay in its quality control analysis for the product. A product evolves as its specifications are adapted to the changing needs of research, and we invite your correspondence as to your specification requirements for sucrose or any other Schwarz/Mann product.

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7 May 1973

Somatic Evoked Response Recording: An Adequate Test of Deafferentation?

Cohn *et al.* (*1*) recorded somatic averaged evoked responses from the scalp of monkeys (*Macaca mulatta*) and a baboon (*Papio*) to test for the completeness of forelimb deafferentation resulting from dorsal rhizotomy. No responses were seen after rhizotomy. The authors concluded that all somatic input from the limb to cerebral cortex was eliminated and that their procedure "constitutes a new critical determinant of the functional effectiveness of the experimental surgery." In our opinion these conclusions are not justified by the data presented, and cannot be by the technique used, for several reasons.

It is tenuous to conclude on the basis of scalp recording that cortical evoked activity is absent. Scalp recordings have an inferior signal-to-noise ratio relative to cortical recording because of significant attenuation of the response by the skull and scalp, and because of the introduction of electrical "noise" from extracerebral sources. Cohn *et al.* used Michel clips to record from the scalp. This is not a commonly used scalp electrode and the quality of the records obtained with its use has not been demonstrated. Recordings of better quality can be obtained in animals directly from the surface of the brain and are more appropriate when attempting to demonstrate the absence of an evoked response. Figure 1 shows somatic evoked responses recorded directly from the cortical surface in four chronically implanted *M. mulatta*; a large primary positive wave with a peak latency of about 14 msec is followed in most animals by a positive-negative sequence at about 40 and 60 msec. A scalp recorded response of similar waveform (but smaller in amplitude owing to the attenuation noted above) is seen also in the *Cebus* monkey (*2*). The evoked responses shown by Cohn *et al.* do not appear to resemble these responses, although comparison with their figure is difficult because evoked response component resolution is inadequate and response polarity is not indicated. This lack of characteristic waveform in their responses from the intact animal could result from inaccurate location of the scalp electrode with respect to the primary cortical sensory area. This suggestion is further supported by the fact that their responses from an intact animal are hardly larger than the presumed spontaneous activity recorded from the rhizotomized animal. For any or all of the above reasons, the lack of a response in their operated animals could indicate merely that the rhizotomy produced a partial deafferentation which reduced the cortical response to a level undetectable by scalp recordings.

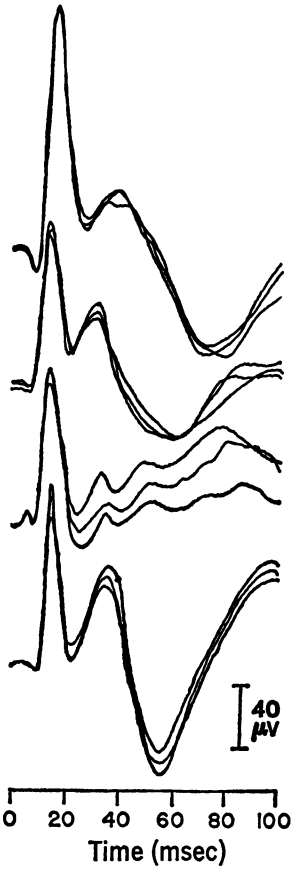

Fig. 1. Averaged somatic evoked responses in four *M. mulatta*. Responses were recorded epidurally from primary somatic cortex in the postcentral gyrus, to percutaneous stimulation of the contralateral median nerve. Each trace is the average of 16 responses; for each animal three averaged responses are superimposed to indicate variability. Positivity at the active electrode is upward referential to an electrode in bone overlying frontal sinus.

Although in man, and perhaps in other primates, somatic evoked responses of the type recorded by Cohn *et al.* are mediated mainly by the dorsal column–medial lemniscal afferent pathway (*3*), the anterolateral tracts of the spinal cord also provide input to various cerebral regions, for example, the midbrain reticular formation (*4, 5*) and the thalamic nucleus centre median (*6*). The activity thus evoked requires higher stimulus intensities (*4, 5*) and is potentiated by chloralose anesthesia

(6). The anterolateral tracts receive much of their input from the small fibers of the lateral division of the dorsal roots (*4*, *7*). It is these fine rootlets which would be the most likely to be incompletely severed during rhizotomy. Therefore, in our opinion a minimal procedure for testing the completeness of deafferentation would include not only recording directly from the surface of primary somatic cortex, but also recording from a structure such as nucleus centre median under chloralose anesthesia and at high stimulus intensities.

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The argument of Allison and Goff would carry some force if they had shown that in the deafferented monkey there were cortical and/or thalamic activity generated by appropriate peripheral nerve stimulation.

The failure to designate polarity in our figure [figure 1 in (*1*)] was an oversight. The polarity is such that if the positive pole of a battery is applied to grid I, the deflection is upward. When our figure 1B (*1*) is recorded with increased sensitivity (as shown in the present Fig. 1), the waveforms are remarkably similar, during the initial 100 msec, with those of Allison and Goff, if their figure, obtained from the adult monkey, is inverted. The absolute amplitude differences may be the direct result of the fact that our animals were 8 months of age.

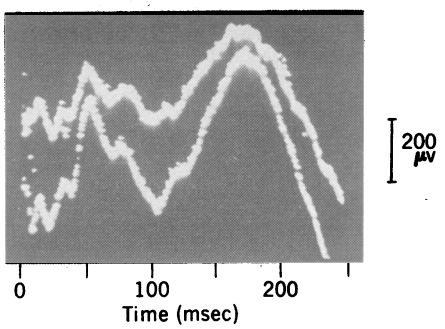

Fig. 1. Summated cortical response to right median nerve stimulation. This is the same as figure 1B in (*1*), but with a threefold increased computer sensitivity. Positive polarity is upward.

The 250-msec sweeps were used to make certain that no relatively long latency responses were overlooked.

There is no punctate cortical position for the response to median nerve stimulation; there is, in fact, an *area* of maximum response. The spacing of our electrodes certainly covers any reasonable area of cortical response from the sensory strip.

As seen in our figures, particularly in the higher sensitivity tracing (Fig. 1) of our earlier figure 1B, the Michel clip forms a good, if not ideal, electrode. Its use should be more frequent under conditions where the animals must be clinically and electrically studied over long intervals of time. Epidural electrodes with only 0.5-mm intracranial protrusion will significantly damage the local superficial nerve cells. This potential damage to the neurons could not be tolerated in our ongoing experiments. Our animals are still alive and being studied, without having been subjected to intracranial invasion.

In previous work Taub *et al.* (*2*) made recordings from the exposed cortex of the deafferented monkey without being able to detect measurable evoked activity.

The suggested use of chloralose anesthesia as an activator is just as suspect as subconvulsive doses of Metrazol and strychnine used by some investigators for this purpose, because such animals as monkeys exhibit "myoclonic" activity.

We are in sympathy with Allison and Goff, as we believed at first that the return of function in the deafferented animals was due to failure to cut all the rootlets. But after considerable work it now seems that the datum for action does not arrive at the cortex in a coherent synchronous way, at least. Our theories of action are discussed in our report (*1*). We of course had planned a series of detailed further studies of possible subcortical areas of coherent response, but were unable to obtain the necessary funds.

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