SESSION III
Otologic Evaluation and Diagnosis
(Moderator: Abraham Shulman)

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General Comments

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I TOOK some notes and I'm trying to make some sense of a few statements I've heard. I was very much impressed by what Dr. Feldmann has told us. He stated that in masking between two signals, as classically described by Wegel and Lane, there is frequency dependence. We also know that masking, neurophysiologically, first occurs in the first order neuron. Up to that point, there is no evidence of masking. But there is another phenomenon. Dr. Wever has shown us, probably 20–25 years ago, that there is a frequency-independent interference on cochlear microphonics, a phenomenon which is distinctly different from masking. I like to suggest that this may be the phenomenon we're looking at in Dr. Feldmann's curves. Interference is an effect that is not neurally mediated but produced mechanically. It seems to me that the type of tinnitus Dr. Feldmann has described may occur before neurosensory transduction, and I think that is an important point. Dr. Feldmann also told us about his forward masking experiments, which I found very interesting. I discussed this with him outside this room and I would like to offer the following explanation: if you have, as he tells me, a masker and if you increase the forward masking interval, you don't eliminate masking until you reach about 350 milleseconds. However, when you do the opposite, that is backward masking, and you let the masker go in the opposite direction, you can only go to about 100 msec. before masking is eliminated. I'd like to offer the following explanation: the event is obviously described by an e-function. If you put signals very close together, your second masker will intercept the e-function at a relatively high level, and if you keep on expanding this it takes a long time until you run out of the masking effect; but when you do the opposite, there has been full recovery, and if you now make the interval shorter and shorter you can go farther because the effect of the previous signal has worn out. What I have learned from Dr. Feldmann, which I didn't understand before, is that he offers these signals continuously. This is the masker for that signal and that's the masker for the next signal, and the next signal, and the next signal. There is no break between them. And now the final point: the contralateral masking phenemonon vs. the ipsilateral one. Dr. Feldmann showed us one case where there was a masking level difference of about 34 db. I agree with him that this might probably be a cross-over effect. But then Dr. Feldmann showed us another case where the level was equal in both ears. I noticed, when he showed his masking curve, that the masking was not quite as frequency-independent as in his other cases. The effect came down, there was a little hump, and then it went

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farther down. Therefore, I would suggest that this may be a separate type of tinnitus. What I like to suggest here is that his method, together with what Chuck Berlin has told us (may be) the way to go about to pin down the site of origin of these various tinnitus cases. Chuck Berlin has shown us that, when there is a continuous type of tinnitus present, there is a desynchronization which effects the total wave form of the evoked brainstem response. What we must not forget is that the BER wave form has a very complex origin. There is a response, say, from the cochlear nucleus and that response hasn't quite worn out when the next response from the superior olivary complex sets in. And that in turn hasn't worn out when the next one from the inferior colliculus begins, and so on. And if you just manipulate one of these various responses you will change the whole picture very pronouncedly. This would be a slightly different way one might think about desynchronization. The last remark I'd like to make is about what Jack Pulec told us. I don't think his two explanations, that of lateral inhibition à la Békésy, or that of efferent inhibition à la Galambos, hold water. I say this for the simple reason that both of these are very transient effects, lasting perhaps a few hundred milliseconds at the most. That could not explain what you call residual inhibition lasting minutes, hours, or even five or six hours. For that reason, I'd like you to be a little cautious using the word inhibition, because inhibition in the neurophysiological sense means something which is very different. In the example of an optical experiment he cited, it was a case of forward masking I am sure; but don't forget the optical system has a much longer time constant than the auditory system. Therefore, as far as the numbers are concerned, these are two entirely different ball games. Visual forward masking is a much slower event. The principle is the same, but you cannot compare the times.

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