discharge and that ion exchange might account for the discharge; in fact they strengthen the case.

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R. Khanin and L. Segel

Department of Applied Mathematics and Computer Science
Weizmann Institute of Science
Rehovot, Israel

H. Parnas and E. Ratner

Department of Neurobiology and Otto Loewi Center for Cellular and Molecular Neurobiology
The Hebrew University
Jerusalem, Israel

Response to Khanin et al.

I am indebted to Khanin, Parnas, and Segel for stirring my interest in MEPC rise times, so I regret our continuing differences about interpretations. The disagreements are laid out in the two papers (Kahin et al., 1994; Van der Kloot, 1995) and in their letter for all to judge. Two points should be reiterated, because they clearly show that we are not communicating.

In regard to their hypothesis that the exchange of a counterion is required for acetylcholine release, they proposed that the counterion is Na^+. In passing, I noted that Katz and Miledi (1969) had recorded MEPCs in isotonic $CaCl_2$ solution, showing that Na^+ does not play a special role in release. However, the data presented in my paper showed that rise times were about the same as usual when the MEPCs were recorded in isotonic sucrose solution, in which the concentrations of potential counterions must be greatly reduced (see also Miledi et al., 1980). They respond to the passing comment but not to the data.

They continue to make much of the high Q_{10} for the synaptic delay, which is the latent period between the nerve terminal action potential and the beginning of the endplate current (Katz and Miledi, 1964). The time required for the acetylcholine to diffuse across the cleft is very short, so the delay is largely occupied by the steps occurring before release is initiated. I remain confused about how this delay, or its temperature sensitivity, provides (as stated in their letter) "... good diagnostic information about the brief discharge process," as it occurs before discharge begins.

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William G. Van der Kloot

Department of Physiology and Biophysics
SUNY at Stony Brook

Received for publication 22 December 1995 and in final form 22 December 1995.

0006-3495/96/04/2032/01 $2.00