sural nerve stimulation produce a negative phase lasting longer than 80 msec in an adequately circulated unanesthetized spinal cord. In Zimmerman's experiment (*1*), the cord must have been severely depressed since the negative DRP lasts 135 msec. In the experiments of Franz and Iggo (*2*), all peripheral nerves had been sectioned, a procedure which will reduce central activity by abolishing the ongoing afferent barrage, and the negative DRP lasted 130 msec. In the experiment of Vyklicky *et al.* (*3*), the cord was sectioned at L1, and all but one afferent nerve from the leg had been cut; both these operations reduce the amount of central activity. We conclude that the results described (*1–3*) were from relatively inactive cords under conditions known to exaggerate the negative and eliminate the positive DRP's.

However, under the conditions of the experiments (*1–3*), certain unmyelinated fibers were shown to produce negative DRP's. Their stimuli or blocking did not allow a test of whether other fine fibers would have produced positive DRP's under those conditions. In our experiments where mixed volleys in fine fibers produced predominantly positive DRP's, we observed intermittent long latency components in the positive wave which, had they been observed in isolation, would have appeared similar to the C DRP of Franz and Iggo (*2*). An example of this negative trend during a positive DRP was illustrated by Mendell and Wall (*4*) in Fig. 4. It therefore seems likely that both negative and positive DRP's can be generated by volleys in fine afferents, depending for their relative sizes on the state of the preparation and on which specific group of afferents are stimulated.

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## Calcium and Salt Tolerance of Plants

LaHaye and Epstein (*1*) have reported that calcium increases the salt (sodium) tolerance of bean plants and imply that this effect has not been appreciated. However, a distinction needs to be made between the tolerance of plants to soil salinity and exchangeable sodium.

Since sodium salts affect soils in special ways, saline and sodic soils have been carefully distinguished (*2*). By definition, saline soils contain enough calcium to meet the ordinary nutritional requirements of plants. In studying the salt tolerance of plants, therefore, calcium is always present at a concentration of at least a few milliequivalents per liter in the root medium (*3*), and the reaction of plants to salinity does not involve calcium deficiency effects except as they may be induced by high concentrations of other salts (*4*).

In sodic soils, in which the concentration of exchangeable sodium is more than 15 percent, calcium and magnesium concentrations decrease as sodium increases. In nonsaline, sodic soils, therefore, calcium and magnesium are often deficient for plant growth. This phenomenon is well known, and the tolerance of plants to high concentrations of sodium and low concentrations of calcium and magnesium has been studied (*5*). As has been reported by LaHaye and Epstein (*1*), sodic soil conditions cause an accumulation of sodium in the tops of bean plants (*5*). Furthermore, the absolute concentration of calcium is critical since, with the same proportions of exchangeable cations, the effect of high concentrations of exchangeable sodium is apparent only when the soluble salt content is low and the absolute calcium concentration is therefore low (usually about 1 meq/liter or less) (*4, 6*).

Thus salt damage does not depend primarily upon a low or deficient calcium level and sodium-calcium relations of sodic soils and plants grown on them have been amply studied.

LaHaye and Epstein's statement that 50 meq of NaCl per liter in the presence of adequate calcium has no effect on the growth of beans is contrary to numerous reported studies in which appreciable reductions in growth and yield have been observed at such salt concentrations (*3*). The discrepancy is probably due to the short period of observation (7 days) by LaHaye and Epstein, to a probably mild set of growing conditions (not specified), and to considering the gross aspects of calcium deficiency rather than the more subtle effects on growth and yield.

The report by LaHaye and Epstein is related to tolerance of plants to sodic soil conditions, not to salinity. The results of LaHaye and Epstein can be attributed to their having followed the erstwhile frequent practice of studying salt uptake by plants from single-salt solutions in short-term experiments and concluding inappropriately that growth responses to salinity also involved single-salt and calcium-deficient solutions.

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The report of LaHaye and Epstein struck a responsive note in my memory, and I pulled from my files *Cornell Agricultural Experiment Station Memoir No. 2*, "The action of certain nutrient and non-nutrient bases on plant growth," by M. M. McCool. I quote from this 1913 publication.

"Kearney and Cameron (1902), employing alfalfa and lupine, found that the greatest endurable concentration of sodium chlorid is .02 mol., while in the presence of calcium chlorid the amount of sodium may be raised to .2 mol.

"When $N/50$ NaCl is employed, slight development of tops occurs but there is no root extension. When seedlings are placed in $N/100$ NaCl, the development of tops is appreciable and the elongation of roots is very slight. In mixed solution of $N/5000\ CaCl_2 + N/50\ NaCl$, the injurious property of sodium is entirely counteracted. Thus, calcium is a powerful antidote for sodium that is present in injurious concentrations."

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We are aware of the distinction between saline and sodic conditions. However, by definition, sodic soils bear on their negatively charged exchange surfaces high proportions of exchangeable sodium ions (more than 15 percent). Bernstein (*1*) has given many instances in which responses of plants were specifically related to soil exchangeable sodium, that is, to the sodic condition. This condition did not prevail in our experiments which were done in solution culture in the absence of soil or other cation exchangers.

Bernstein states, "By definition, saline soils contain enough calcium to meet ordinary nutritional requirements of plants." Salinity of soils is defined in terms of the conductivity of the saturation extract, without any reference to calcium (*2*).

We disagree that our results are due to the short period of observation, mild growing conditions, and a neglect of subtle effects on growth and yield. (i) In the presence of adequate calcium, we have grown plants at the same salinity (50 m$M$ NaCl) to maturity, and obtained a normal yield of beans. (ii) We have not studied the effects of climatic conditions systematically; the experiment reported by us was done under midsummer conditions at Davis; temperatures in the greenhouse sometimes exceeded 35°C.

Bernstein says that our results can be attributed to our "having followed the erstwhile frequent practice of studying salt uptake by plants from single-salt solutions in short-term experiments." He errs; the plants in our experiments grew in complete nutrient solutions, as has been stated in our report.

We are aware that there are some prior mentions of the role of calcium in salt toleration by plants [see, for example, references 10, 11, and 12 in (*3*) and the note from Shear]. We called attention to this matter because it is frequently neglected when the effects of salt on plants are studied. We specifically focus on mechanisms of salt damage to plants, including the propensity of sodium ions at high concentrations to displace calcium from root cells and, especially, from their membranes. This phenomenon cannot usefully be studied in experiments in which the ratio of Na to Ca is kept constant, as has frequently been the practice (*4*).

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## Intermixture and Selection

Reed (*1*) has reviewed studies designed to estimate the proportion ($M$) of Caucasian ancestry in different American Negro populations, and, in particular, he provides a criticism of studies (*2, 3*) in which it was suggested that a comparison of the $M$ values for different genes could indicate which of those genes may have been affected by (directional) natural selection during the past 300 years.

Reed's discussion of estimates of $M$ for the ABO blood group system shows some misunderstanding of the statistical principles involved. His analysis (*4*) of gene frequency data from a study of Negroes in Oakland, California, shows that $M(\text{ABO}) \cong M(Fy^a)$ and that $M(\text{A}) \cong M(\text{B}) \cong M(\text{ABO})$. From these results, and the hypothesis that the frequencies of the $Fy^a$ allele have not been affected by natural selection, he concludes that the estimate of $M$ for the ABO locus is not greatly disturbed by selection and that there is no suggestion of selective differences between the $A$ and $B$ alleles. These observations are, however, not "pertinent" to a consideration of differential selective pressures on the $A_1$ and $A_2$ alleles, as he suggests they are, since, if there are differences in selective forces acting on the alleles at a locus, his joint likelihood estimates of $M$ for the ABO locus, or the $A$ "allele," need not reflect such differences. For interallelic comparisons it is the relation between estimates of $M$ for different alleles that is of interest and not their numerical values per se. Similarly, if we wish to detect differences in directional selection pressures among genes at different loci, joint likelihood estimates of $M$ are inappropriate and we must consider the magnitude of an estimate of $M$ relative to that of some other gene ($Fy^a$ or $R^0$) presumed to be unaffected by directional selection.

For example, Workman (*3*) suggested that an examination of the distribution of frequencies of the $A_1$, $A_2$, and $B$ alleles in several West African, Western European, and American Negro populations indicated that $M(\text{A}_2) \gg M(\text{A}_1)$ in American Negro populations. Reed's own data from Oakland would appear to support this hypothesis. The $\text{A}_1\text{A}_2\text{BO}$ gene frequencies given by Mourant (*5*) show that, in West Africa, the frequency of $A_1$ is almost always greater than 0.095 and that of $A_2$ is generally less than 0.05. Using these values and the frequencies of the genes in Oakland Negroes and Western Europeans (*4*) we find that $M(\text{A}_2)/M(\text{A}_1) \cong 2$. This is the minimum value likely if the range of published West African frequencies is considered. However, if frequencies of $A_1$ and $A_2$ more commonly observed in West Africa (*5*) are considered—say, 0.10 and 0.05, or 0.105 and 0.045—then the ratios would be $(0.578)/(0.194) = 2.98$ or $(0.685)/(0.142) = 4.82$, respectively.

Reed suggests that, by his examination of the hypotheses involved in obtaining estimates of $M$, results which formerly provided claims that selection has been demonstrated now appear only "suggestive but far from conclusive." In fact, all of the underlying assumptions and the restrictions they place on our ability to draw conclusions were discussed by Workman (*3*). For example, one could test the hypothesis that $M(\text{A}_2)/M(\text{A}_1)$ is greater than 1 by means of a sign test of the ratio of the $M$ values from a series of populations, but, as discussed elsewhere (*3*), even when a ratio such as that noted for the $A_1$ and $A_2$ alleles is observed in several different American Negro populations, the conclusion that selection is involved still depends upon assumptions about ancestral African frequencies.

Finally, a concern with standard errors is appropriate when the problem