the twig morph directly, or as Faeth and Hammon suggest, through their interaction and binding with proteins, thus lowering the amount of digestible protein. The feeding experiments (*1*, table 2) do not distinguish between these two alternatives.

The main purpose of the paper (*1*) was to report the existence of this diet-induced developmental polymorphism. Although I suggested that polyphenolic compounds may be important, I had hoped to make it clear that we still do not know what it is that induces the development of the different morphs by stating, "This developmental polymorphism *may* be triggered by the concentration of defensive secondary compounds in the larval diet" (emphasis added) and "a possible mechanism is that receptors respond to tannin levels. . . ." I thank Faeth and Hammon for clarifying these points and for stating more explicitly another hypothesis concerning the mechanism involved.

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Does Voltage Affect Excitation-Contraction Coupling in the Heart?

In their report of experiments investigating excitation-contraction coupling in cardiac muscle, Näbauer *et al.* (*1*) say they have demonstrated that "it is *exclusively* Ca^{2+} influx through calcium channels . . . that regulates Ca^{2+} release in cardiac myocytes (emphasis added)." The experiments they present demonstrate that internal Ca^{2+} release can occur when the membrane is depolarized under conditions that permit calcium influx and that if Ca^{2+} influx is prevented, Ca^{2+} release does not occur. Their experiments therefore confirm previous work demonstrating that membrane depolarization alone cannot trigger release of Ca^{2+} from internal stores (*2–4*). However, they did not examine the effects of varying membrane potential under conditions that allow Ca^{2+} influx. Thus their data do not contradict the records of Cannell *et al.* (*3*), which show that repolarization can switch off calcium release from the sarcoplasmic reticulum (*3, 4*), nor do they refute our suggestion (*3*) that "Ca^{2+}release may be mediated by a mechanism that requires a Ca^{2+} influx but which may be modulated by changes in voltage directly." (We did not suggest that voltage alone can trigger Ca^{2+} release, as might be inferred from the context of the citation to our work.) Since modulation of calcium release by voltage can explain all experimental data to date, more critical tests of this hypothesis are needed.

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Response: The main issue addressed in our report (*1*) is the regulation of Ca^{2+} release from the sarcoplasmic reticulum in cardiac myocytes. Our experiments showed that there is no Ca^{2+} release when there is no Ca^{2+} current through the Ca^{2+} channel. Nor was there any release if Na^+ or Ba^{2+} carried the charge through the Ca^{2+} channel [this finding was independent of holding potentials in the range from −60 mV to −90 mV, which rules out possible inactivation of gating charge (*2*)]. We feel justified, therefore, in maintaining that it is exclusively the influx of Ca^{2+} through the Ca^{2+} channel which regulates Ca^{2+} release in cardiac cells.

Our findings also suggest that membrane potential plays no direct role in the release process. Previously we showed that the voltage dependence of the intracellular Ca^{2+} transients closely follows that of the inward Ca^{2+} current (*3*). It is not correct, therefore, to say that we "did not examine the effects of varying membrane potentials." In addition, others have also found that early estimates of the Ca^{2+} transients are well correlated with the inward Ca^{2+} current, even when a more slowly developing Ca^{2+} transient is present (as it may be in Na^+–loaded cardiac cells) (*4*). The confusion surrounding the voltage dependence of the Ca^{2+} transient may result from comparison of the Ca^{2+} current with the much later occurring maximum value of the Ca^{2+} transient (*5*). Such a comparison may be flawed, not only because of the presence of other Ca^{2+} transport mechanisms, but also because of saturation of the Ca^{2+} indicator dye or exhaustion of the releasable Ca^{2+} pools.

As for the question of whether membrane potential plays a limited modulatory role, Cannell *et al.* point to their observation that the rising phase of the Ca^{2+} transient at 0 mV is interrupted by early repolarization to the holding potential (−40 to −80 mV). We repeated these experiments and found in addition that the rising phase of the Ca^{2+} transient could also be interrupted by further depolarization to +80 mV (ECa), where Ca^{2+} transients are generally completely suppressed. We interpret this as indicating that the Ca^{2+} release is stopped when the Ca^{2+} current is interrupted either by repolarization-induced deactivation or by depolarization to the reversal potential. Repolarizing pulses to −80 mV, in addition, would activate the Na^+-Ca^+ exchange to extrude Ca^{2+} and help suppress the Ca^{2+} transients. Therefore, even these experiments support our basic idea that it is not the membrane potential as such which is important for Ca^{2+} release; rather it is the degree to which the membrane potential activates the Ca^{2+} current.

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