anomalous because the most consistent change in growth rate in phototropism usually occurs at the illuminated side where growth virtually ceases[2]. If there is no detectable change in $[Ca^{2+}]_{cyt}$ at the illuminated side then there is obviously no correlation between $[Ca^{2+}]_{cyt}$ and cell extension.

Surprisingly, Gehring et al. state that the observed changes in $[Ca^{2+}]_{cyt}$ occur only if the apex of the coleoptile is illuminated; they claim that this is consistent with the known site of perception of the phototropic signal. Unfortunately, this old misconception about the site of perception keeps cropping up despite the fact that it has been shown[3] that a phototropic signal can be detected by all growing regions of coleoptiles. If the changes in $[Ca^{2+}]_{cyt}$ are really involved in phototropism, they should be looked for in decapitated coleoptiles and in coleoptiles subjected to unilateral illumination at any place in the elongation zone.

Gehring et al. used the Cholodny–Went model of tropisms and ignored the many experiments conducted over the past two decades which question the validity of this model — for instance, Went's original experiments have recently been repeated using modern analytical methods[4]. Contrary to Went's original claim, the flow of auxin from the illuminated and shaded sides of phototropically stimulated coleoptile tips are identical. Gehring et al. also ignore the careful analyses of Weiler and co-workers[5] which have failed to detect any significant tropistically induced changes in auxin concentration during gravi- or phototropism. Finally, they still seem to believe that auxin movement across tropistically stimulated organs is relevant despite the fact that the two isolated halves of longitudinally bisected organs show a gravitropic response[6]. The fact that tropic responses can occur in the absence of auxin redistribution and that, even when auxin redistribution does occur, it seems slower than the reported changes in $[Ca^{2+}]_{cyt}$ should alert Gehring et al. to the possibility that auxin does not mediate the changes they report.

RICHARD D. FIRN
JOHN DIGBY

Department of Biology,
University of York,
York YO1 5DD, UK

GEHRING ET AL. REPLY—The Cholodny–Went model has been extensively supported for phototropism in maize, the material used in our study (see, for example, refs 7,8). Although a definitive study measuring auxin levels remains to be done, the extant evidence for maize does favour a lateral redistribution of auxin during phototropism[7,9]. Therefore, our use of the model is appropriate.

For phototropism in general, the "consistently observed growth inhibition" on the irradiated sides of various species of bending stems usually results from non-tropic light–growth responses being simultaneously induced along with the phototropic response[10]. By contrast to maize, where the phototropic response resembles what is known for that of many other plants[7], the phototropic response of Avena seems to be unique in several respects: it is the only plant in which a region of negative curvatures occur in the phototropic fluence response curve[11]; the pattern of growth response is apparently more varied than in other plants[2]; and measurements of auxin concentrations in oat have given confusing and contradictory results[9]. Therefore, oat would seem a poor model system for phototropism. Further, problems in applying the model to oat are irrelevant as we studied maize.

On the technical points raised by Firn and Digby, we accept that the use of blue light as a inductive stimulus would have been preferable to white light. Nevertheless, we are confident that the observed $[Ca^{2+}]_{cyt}$ and pH_{cyt} effects are specific for the phototropic response. In upright coleoptiles illuminated with the laser beam only, no changes in $[Ca^{2+}]_{cyt}$ and pH_{cyt} occurred: such changes were observed only when phototropic bending was induced, hence our conclusions that these effects are specific for phototropism.

Firn and Digby have promulgated a critique of the Cholodny–Went model in which they demonstrated several areas where evidence in its favour was either lacking or unconvincing[13]. Their analysis has spurred considerable investigation, which has in many instances been supportive of the model, especially for maize[7]. We suggest that this is a rare instance of being able to teach an old dogma new tricks.

C.A. GEHRING
R.W. PARISH

Department of Botany and
Centre for Protein and Enzyme
 Technology,
La Trobe University,
Bundoora, Victoria 3083, Australia
D.A. WILLIAMS
S.H. CODY

Department of Physiology,
University of Melbourne,
Parkville, Victoria 3052, Australia

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Air pressure and methane fluxes

SIR—The increase in the atmospheric concentration of methane gas[1,2] is an important factor in the analysis of global warming trends. At Mirror Lake, New Hampshire, we observed that sporadic methane bubble releases (ebullition) from the sediments were correlated with changes in local air pressure. This is the first time such a correlation has been reported. Low-air-pressure events usually associated with storm systems appeared to induce ebullition (18% increase per millibar drop in air pressure), whereas high pressure inhibited ebullition.

Mirror Lake is shallow (11 m) and unproductive, yet has thick organic sediments and becomes anoxic below 9 m in depth by mid-summer[3]. Here we discuss only deep sites in the hypolimnetic zone (see ref. 4 for additional details and methodology). Briefly, rising gas bubbles were collected continuously during the summers of 1987 and 1989 in large inverted funnel traps[4,5]. Gases were removed every 2–6 days and transferred immediately to the laboratory for analysis by dual-column TCD gas chromatography. The loss rate of methane from the traps was approximately 3% per day.

During 1987, methane ebullition rates were erratic, with an average rate of 0.76 and a high of 4.26 mmol m^{-2} per day at the deepest sites (10 m). The proportion of methane in the bubbles was relatively constant $70 \pm 7\%$. Ebullition rates and hypolimnetic temperatures increased during the summer, until 6 October 1987 (day 279), when autumn circulation occurred in the lake, and the hypolimnion was re-oxygenated. All sites, including shallow-water sites, showed a remarkable synchrony in bubble release during 1987. We observed many bubbles rising to the surface of the lake on some days and none on others. The ebullition rates at two sites with the longest continuous records correlated highly with each other ($r^2 = 0.71$, $a < 0.01$, $n = 42$), and showed large deviations from the regression of ebullition versus day of the year (a in the figure).

We examined several factors which might influence ebullition, including wind speed, water temperature, solar radiation, lake level, local water table and air pressure (b in the figure). Only the lowest air pressure in each sampling period explained a large and significant amount of the residual variation in ebullition rate at the two deepest sites ($r^2 = 0.42$, $a < 0.01$, $n = 42$) with ebullition increasing 18% (0.14 mmol m^{-2} per day) for each millibar drop in pressure.

In 1989, ebullition was again related to the lowest air pressures ($r^2 = 0.22$, $a < 0.05$, $n = 26$). During a 64-day period,