are in fact adaptive in both cases (i.e. can different and even opposite trait responses increase fitness in different species under a given set of external stimuli?). Proof of adaptive plasticity also requires analysis of fitness in multiple environments.

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References


Key words: adaptation, aquatic macrophytes, evolution, morphology, phenotypic plasticity.

Letters

The Cohesion-Tension Theory

In the June 2004 (162: 3) issue of New Phytologist, U. Zimmermann et al. published a Tansley review that criticizes the work of many scientists involved in the study of long-distance water transport in plants (Zimmermann et al., 2004). Specifically, the review attempts to ‘show that the arguments of the proponents of the Cohesion Theory are completely misleading’. We, the undersigned, believe that this review is misleading in its discussion of the many recent papers which demonstrate that the fundamentals of the Cohesion-Tension theory remain valid (Holbrook et al., 1995; Pockman et al., 1995; Steudle, 1995; Milburn, 1996; Sperry et al., 1996; Tyree, 1997; Melcher et al., 1998; Comstock, 1999; Stiller & Sperry, 1999; Tyree, 1999; Wei et al., 1999a; Wei et al., 1999b; Cochard et al., 2000; Cochard et al., 2001a; Cochard et al., 2001b; Richter, 2001; Steudle, 2001; Cochard, 2002; Tyree & Zimmermann, 2002; Tyree, 2003; Tyree & Cochard, 2003; Tyree et al., 2003). We wish the readers of New Phytologist to know that the Cohesion-Tension theory is widely supported as the only theory consistent with the preponderance of data on water transport in plants.


Key words: cohesion-tension theory, Tansley reviews, long-distance transport, water transport, xylem.

References

Editorial

Tansley reviews

Authors of Tansley reviews, which are fully peer-reviewed papers, are asked to consider two major themes in their writing. First, to deal with major research topics in some depth – to provide a 'touchstone' for those intending to enter the field. Second, to consider the review less as an exercise in literature documentation and more as a forum for the presentation of ideas. The balance between these two themes varies widely, depending on the subject and the individual, but we aim to make the distinction clear.

Where views and opinions are expressed in a Tansley review, or indeed any New Phytologist paper, these naturally belong to the authors. This is, we believe, clearly the case in the writing of the Tansley review by Zimmermann et al. in our June 2004 (162: 3) issue (Zimmermann et al., 2004).

The Tansley reviews and our forum section encourage debate in New Phytologist. We therefore welcome discussion, in this instance concerning the work of Zimmermann et al. through the comments of Angeles et al. (2004), which complement recent and relevant publications in New Phytologist by Brodribb & Holbrook (2004) and Sperry (2004).

Ian Woodward
Editor-in-Chief

References


Key words: Tansley reviews, peer review, forum, cohesion-tension theory, long-distance transport, water transport, xylem.

Letters

How dangerous is the use of fungal biocontrol agents to nontarget organisms?

Biological control of plant pathogens is a method based on the antagonism between microorganisms (Andrews, 1992) – fungi or bacteria known to be antagonistic to a given plant pathogen are artificially multiplied and then released into an agricultural field to control a plant disease. Most biocontrol agents (BCAs) of plant pathogens, similar to most plant pathogens they control, are fungi. Their use is considered, in general, as a safe and environmentally friendly alternative for plant disease control compared to the application of conventional pesticides (Whipps & Lumsden, 2001). Recently, Brimner & Boland (2003) published a review of the nontarget effects of fungal BCAs of plant pathogens in which they attempt to demonstrate the way in which many hazards may be associated with the use of fungi as BCAs of plant diseases. However, as the examples highlighted here indicate, their case was based mainly on unsubstantiated statements, which might mislead and be detrimental to the application of BCAs in the future.

Brimner & Boland (2003) use expressions such as ‘significant environmental impacts’, ‘significant threat’ and ‘unforeseen ecological repercussions’ in order to dramatize suggested damaging effects of fungal BCAs. However, none of the data reviewed in the paper support these serious warnings. Similarly, key statements such as ‘released BCAs have the
Comment on "Water ascent in tall trees: does evolution of land plants rely on a highly metastable state?" by Ulrich Zimmermann, Heike Schneider, Lars H. Wegner, and Axel Haase (New Phytologist 162: 575–615.)

Abstract:
The critique given by Zimmermann et al. (2004) of Holbrook et al. (1995) is fundamentally flawed. Figure 1 of Holbrook et al. (1995) is a stick figure schematic illustration of the experiment presented in the paper, with the leaf chamber size and details given in footnote 13. Zimmermann et al display a fundamental misunderstanding of the correct application of the centrifugal force equation used in the experiment. Their imaginary scenario produces tensions 1/225th to 1/2500th of those needed to produce the 1:1 observations of Holbrook et al., rather than produce tensions capable of providing an alternative path to the observed 1:1 observation. In addition, they mislead the reader by failing to point out that the water potential of the control leaves was independent of the angular velocity.

Details:
On page 615 of the article "Water ascent in tall trees: does evolution of land plants rely on a highly metastable state?" by Ulrich Zimmermann, Heike Schneider, Lars H. Wegner, and Axel Haase (New Phytologist 162: 575–615), the authors state:

"The experiment of Holbrook et al. (1995) is faced with the same shortcomings as the various ‘vulnerability’ methods. They used an excised stem segment with a single leaf at its midpoint, and mounted the midpoint of the stem on the rotating axis of a centrifuge-like set-up placed in a closed chamber. After centrifugation the authors removed the leaf and determined the balancing pressure value. In the light of the discussion in section III.3 tension in the xylem should be released instantaneously upon cutting (provided that no breakage of the water columns had occurred). Nevertheless, the authors found a 1 : 1 relationship between the (relative) pressure calculated from the centrifugation force and the balancing pressure. A possible explanation for the 1 : 1 correlation is that water was shifted into the periphery of the leaf (including the intercellular spaces) during centrifugation. Then, Newton’s law that action has to equal reaction requires that the same force is needed to push water back from the tissue into the xylem. The problem for interpreting the data is the control experiment of Holbrook et al. (1995). It is obvious from the sketch in Fig. 1 of their paper that the control leaf, being not attached to the branch, was spun simultaneously, but not fixed close to the rotor axis of the centrifuge. In
this case, the leaf is pressed against the wall of the chamber during rotation and, in turn, the centrifugal forces may act in a different way on the water in the control leaf compared to the fixed leaf. This could explain why the $P_b$-values of the control leaves were significantly lower than those of the attached leaves by 0.2 to 0.4 MPa. A correlation with the calculated rotational tension can also not be expected under these conditions. However, the finding that the $P_b$-values of the control leaves were significantly higher than the values of untreated leaves (0.05 MPa relative to atmosphere) evidences clearly that the control experiments were not properly designed."

To propose their hypothetical "flaw", Zimmermann et al. ignore the data in footnote 13 and instead propose an imaginary scenario they believe will produce the result they desire. *Taking Zimmerman et al.'s statements at face value,* the forces Zimmerman et al. describe shifting the water to "…the periphery of the leaf (including the intercellular spaces) during centrifugation" range from factors 2-3 orders of magnitude too small to account for the 1:1 relationship of the Scholander reading to the "calculated rotational tension" presented in Figure 2 of Holbrook et al. As clearly stated in footnote 13 of Holbrook et al., the lengths of the *Cercis occidentalis* branches ranged from 30-100cm, thus the R’s used to calculate the "calculated rotational tension" (x-axis of Figure 2) ranged from 15-50cm while the maximum extension from the rotation axis any part of a leaf even using Zimmerman et al.'s incorrect description of the *Cercis occidentalis* leaves is 1cm, limited by the chamber size clearly stated in footnote 13. The induced tension in the water column at the rotation center is $T=0.5\sigma\omega^2R^2$, where $\sigma$ is the density of water, $\omega$ the angular velocity, and $R$ is the distance from the axis of rotation to the end of the water column, which Holbrook et al.’s Figure 2 x-axis, was half the branch length. Zimmermann’s imaginary scenario results in a discrepancy of a factor of the square of half the total branch length to the square of the 1 cm leaf chamber radius, thus a discrepancy of $15^2:1$ to $50^2:1$ (225:1 - 2500:1 ) from the observed 1:1 ratio. In other words, since the x-axis of Figure 2 of Holbrook et al. is a calculated value using R’s of 15-50cm, and the induced tensions anywhere there is an intact water column depend on the water column $R^2$, leaves with a maximum moment arm ($R$) of 1cm cannot have tensions induced between their centers and peripheries sufficient to come anywhere near the calculated values where $R$ ranged from 15 to 50cm. (The 1:1 line of Figure 2 of Holbrook et al.) Zimmermann et al.’s (erroneous) scenario could, based on simple physics any High School physics student would understand, induce tensions 1/225th to 1/2500th of those needed to produce the 1:1 observations of Holbrook et al. Thus *Zimmermann et al.'s imaginary "possible explanation" results in tensions 2-3 orders of magnitude too small to produce the observed 1:1 ratio of Figure 2.* Furthermore, had Zimmermann et al.'s imaginary scenario been valid, it would have had to also occur in the test leaves which would have resulted in the test leaves being indistinguishable from the control leaves, in direct contradiction with the actual observations.

In addition, had Zimmermann et al. looked up what *Cercis occidentalis* leaves look like, it would have been obvious that both the test and control leaves had to be wrapped together around the inner chamber walls, thus experiencing exactly the same forces and thus precluding their proposed scenario from occurring even with its deficiencies. In other words, the experiment of Holbrook et al. is even more robust than it may appear to an inattentive reader.
Given the overall tenor of Zimmermann et al., we will leave it to the reader to speculate as to why Zimmermann et al. would advance the critique on Holbrook et al. that they did, hidden in the second appendix their paper and without presenting any estimates for the magnitude of the hypothesis they proposed despite such estimates being in a realm that any High School physics student could easily calculate.

