Estimating Biomass (Response)

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Joachim Guenther (*1*), Sergio Gloor (*2*), Jurg Sommer (*3*), Melitta Dihanich (*4*), and Hana Suidan (*5*), who are past or present collaborators at the Friedrich Miescher Institute, and Marie-Charlotte Hoffmann (*6*), who is associated with Cordula Nitsch's group at the University of Basel.

The demonstration that thrombin acts on neuronal cells by activation of a specific receptor (*5*), initiating still unknown cascades, possibly through a linkage with a G protein (*7*), indicates that the classical coagulation pathway may not be the primary mode of action in the nervous system, as Marx points out. Similar results supporting this concept were recently obtained by Wouter Moolenaar and his colleagues in Amsterdam (*8*). In addition, David Small and his collaborators at the University of Melbourne have demonstrated that PN-1 is a potent inhibitor of a secretase of the amyloid precursor protein which can be associated with acetylcholinesterase and is thought to process the protein from the cell surface or from the extracellular matrix (*9*). Together with the presence of messenger RNA for prothrombin (*4*) and thrombin receptor (*10*) in neural structures, these results indicate that these proteins are not only involved as safeguard components to prevent serious damage from local rupture of the blood-brain barrier, but that they could have additional functions important for the development and plasticity of the nervous system. These novel aspects will render research in this field even more exciting in the years to come.

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Estimating Biomass

Estimates of global and continental biomass and carbon storage are rarely based on data intended for that purpose. This is the case with the data used as a baseline by Pekka E. Kauppi *et al.* (Articles, 3 Apr., p. 70). The source they cite for baseline biomass estimates of European forests (*1*) is a compilation of many unrelated estimates of timber stocks. They convert these estimates to assess biomass and carbon storage and include no estimates of error, without which it is difficult to evaluate discrepancies among estimates or test the significance of suggested trends. Examination of this source and others (*2*) cited by Kauppi *et al.* reveals that the data they contain are not well documented, and it is difficult to evaluate their merit.

Under the heading of "Universal-global tendencies" Kauppi *et al.* cite a source (*3*) that states that growing stock and timber growth potential in the United States have been repeatedly underestimated. M. Clawson, however, states at the outset of (*3*) that his study, like many other historical reviews, is "limited by the paucity, suspected inaccuracy, and noncomparability of available data." Kauppi *et al.* cite this study and conclude that underestimation may be common. On the contrary, it has been shown recently that the biomass and carbon storage of North American boreal and Eastern deciduous forests have been vastly overestimated (*4*). Whether this is true for Europe we do not know, but it is a question that should be examined. In addition, a recent publication about North American forests (*5*) from the source of the authors' primary data (*1*, *2*) suggests that in Canada growing stock is declining, which apparently contradicts the same data source. How valid are the results and conclusions of a study that depends on questionable data with no independent measures or confirmation?

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Response: We appreciate the comment by Botkin *et al.*, which we think supports the recommendation we made in our article about research priorities with regard to estimating the carbon budget of ecosystems. Confidence limits can only be calculated when the primary measurements are taken from sample plots located randomly or with a systematic grid.

Botkin and Simpson have estimated the carbon storage of aboveground forest vegetation on a continental scale with unbiased sampling (*1*). Their study area covered 5.1 million square kilometers. Y. Ilvessalo published an early corresponding national study, an unbiased forest inventory covering 0.38 million square kilometers (*2*). Although his study and subsequent forest resource surveys in Europe were not designed for carbon assessments, they can be used in this context because all trees reaching breast height (1.3 meters) were included in the samples. The large pool of belowground carbon was not measured in either (*1*) or (*2*).

It is useful to distinguish between carbon storage and the *change* of carbon storage. It is the change that counts in budget calculations. Therefore, we need periodically repeated, statistically representative measurements. Forest inventories have been repeated periodically since the 1920s and are probably the only relevant studies providing unbiased time series data for carbon storage in forest vegetation. The sampling grid in these inventories has extended at best to national geographic scale.

In Europe, forest inventories have been carried out and repeated in Finland, Sweden, and Austria and, with some interruptions and shortcomings, in France. They cover a total of 14% of the European forests area (18% if France is included). The growing stock, an indicator of aboveground carbon storage, increased from 1971 to 1990 by 28 ± 2.0% in Finland, 14 ± 2.0% in Sweden, and 24 ± 2.5% in Austria (*3*). The development was similar in France.

Our conclusions were based on five kinds of references: (i) complete forest inventory records (from Finland, Sweden, Austria, and, with reservation, France); (ii) incomplete forest inventory records (from Germany and Switzerland); (iii) official statistics on forest resources from the remaining countries; (iv) reviews and primary research articles on growth and yield; and (v) forest products statistics. The data consistently showed a trend of increasing forest biomass, forest growth potential, and accumulation of forest products. The criticism of Botkin *et al.* applies only to category (iii).

Official forestry statistics can be biased. For some countries (in the worst cases) the information is based on expert opinion. However, we believe that listing and reviewing results from different studies from different countries represents scientific progress as compared with the state of the

in the 1970s. Others have concluded, tiie basis of limited and inconsistent ı, that nontropical forests would act as sources of atmospheric carbon (4).

n this century, land use in Europe has ıged markedly. Cattle grazing on forest l has decreased, the use of small-sized d for fuel has also decreased, fire control improved, and loggings have shifted ı primary to secondary forests. Some utants have acted as fertilizers. These ıges have contributed to the trend of easing forest biomass.

The development in other continents beyond the scope of our paper. How-, we presented a hypothesis that "[i]f e has been similar development in oth- continents, biomass accumulation in tropical forests can account for a large ortion of the estimated mismatch be- en sinks and sources of atmospheric on dioxide." We look forward to a rt of a second periodic measurement of "Botkin and Simpson grid" (1) after 5 0 years, which will test our hypothesis. n we might approach a solution to the lem of the so-called "missing carbon."

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Previous Sol-Gel Enzymes

ur report of 28 February (p. 1113), apsulation of proteins in transparent us silicate glasses prepared by the sol- nethod" (1), after the sentence "En- s immobilized in or on inert matrices been studied extensively as catalysts, the matrices in general have not been ble for use in optically based molecular ors because they are opaque," we cited work of S. Braun *et al.* that described preparation of opaque samples contain- enzymes encapsulated using the sol-gel od (2). It has been called to our ition by those authors that our method ferencing did not give them adequate it because it did not specify that they used sol-gel methods to encapsulate mes. We wish to state that our sentence summarizing enzyme immobilization was too terse and was in no way intended to diminish the important contributions of these authors.

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Oct-3 and Mammalian Development: Correction of Discussion

In our Perspective of 12 July 1991 (p. 144) [*Science* **253**, 144 (1991)], we discussed the role of the POU domain protein Oct-3 in mouse development. One of the papers to which we referred, by M. H. Rosner, R. J. De Santo, H. Arnheiter, and L. M. Staudt (1), which dealt with the role of Oct-3 in the one-cell embryo, has since been retracted because the experimental evidence was fabricated by M. H. Rosner without any knowledge by the other authors. It therefore follows that our discussions of this *Cell* paper should be disregarded. We emphasize that no doubt attaches to any of the other work we reviewed.

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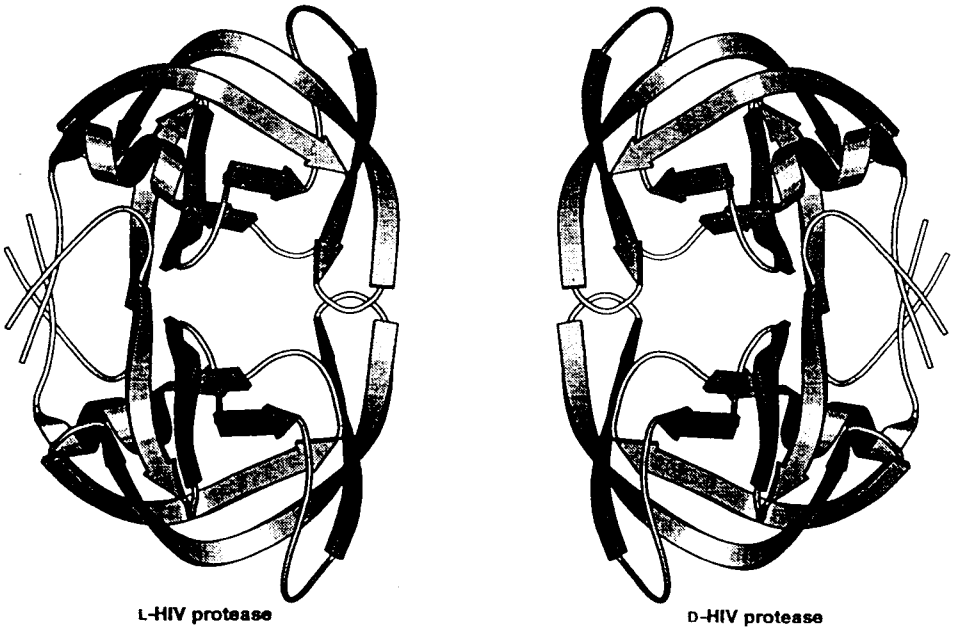
Corrections and Clarifications

The title of the 5 June report on page 1445 by R. C. deL. Milton *et al.* should have been "Total chemical synthesis of a D-enzyme: The enantiomers of HIV-1 protease show reciprocal chiral substrate specificity." Figure 3 in the same report (p. 1447) was inadvertently printed upside down. The labels "L-HIV protease" and "D-HIV protease" were therefore under the wrong illustrations. The correct figure is printed below.



L-HIV protease D-HIV protease