tutions, investigators, laboratory animals, or students. Misguided efforts to block our historic settlement with the USDA would force biomedical research to take a step backward.

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Agriculture, Food Systems, Energy, and Global Change

In "Greenhouse gases in intensive agriculture: Contributions of individual gases to the radiative forcing of the atmosphere" (Reports, 15 Sept., p. 1922), G. P. Robertson, E. A. Paul, and R. R. Harwood offer an excellent long-term and systems-based analysis of the relative impacts of different cropping systems upon global warming potential (GWP). They find that no-till management has the lowest net GWP, followed by organic and low input management (each with legume cover). These three all have much lower GWP than conventional tillage (Table 2, p. 1924). In concluding, they state that "[a]griculture... plays a minor role in the GWP economy of the U.S., yet net mitigation of agricultural fluxes could offset the current annual increase in fossil fuel CO_2 emissions." This kind of basic research is of great importance in setting intelligent research and policy agendas for agriculture, and it deserves further elaboration. It is also crucial that such research and analysis be placed in the larger context of food systems.

Research done in the 1960s and 1970s showed that (i) agriculture represented only about a third of the total energy used in the U.S. food system, (ii) the typical food calorie on a dinner plate required 10 calories of energy input (*1*), and (iii) the average food item was shipped some 1300 miles (*2*). Internationally, a 1993 estimate indicated that "only about 10% of the fossil fuel energy used in the world's food system is used in production" (*3*). We desperately need to update and improve the quality of these data and formulate an analysis of their GWP to determine where the greatest reductions are to be found.

Any search for more sustainable ways to structure our food systems will require more than an energy analysis of current long-distance industrial food systems. It will be necessary to review the underlying theories of social change in conventional energy approaches (*4*). In addition, the many significant social, health, and environmental externalities of industrial food systems must be included (*5*). Global warming studies should examine not only current industrial structures and food systems, but more localized alternatives—both traditional and emerging (*6*). Only with such a comprehensive and systematic approach will we be able to assess the full range of the costs and benefits of more global versus more local food systems.

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Measurements and Models of Soil Loss Rates

In their Policy Forum "U.S. soil erosion rates—myth and reality" (14 Jul., p. 248), Stanley Trimble and Pierre Crosson discuss sediment budgets and call attention to the large quantitative difference that exists between upland soil erosion and downstream sediment delivery (*1*). The authors are correct in their statements regarding the universal soil loss equation (USLE) and wind erosion equation (WEE) when they state that the models "predict the amount of soil moved on a field, not necessarily the amount of soil moved from a field" (*2–4*). The USLE predicts "soil loss," which is a technical term referring to the net loss of soil over the portion of the landscape that experiences a net loss over time. Soil loss does not refer to sediment yield.

We take issue, however, with the conclusion by Trimble and Crosson that "[t]he limitations of the USLE...are such that we do not seem to have a truly informed idea of how much soil erosion is occurring in this country." Despite the limitations of the USLE and its successor, the revised USLE (RUSLE), there exists today no other environmental technology that is based on a larger and more comprehensive set of measurements. More than 10,000 plot-years of data from 50 locations in 24 states went into the development of the original USLE (*3*), and many more data sets from many types of experiments have been used since that time to either improve or test the technology (*5*). Two recent studies of measured soil loss rates (as defined above) from more than 1700 plot-years of data from 205 research plots at 20 locations in the United States showed that the USLE and RUSLE predict average erosion rates reasonably well (*6*), even on recent, post-1960 conditions. Soil loss estimates from the USLE are quite reliable measures of upland erosion rates in the United States.

The United States would benefit from a better understanding of sediment movement and sediment redistribution within agricultural fields, as implied in the Policy Forum. Work is under way to develop new tracer technologies to make these measurements (*7*). We agree with Trimble and Crosson's call for increased field studies and monitoring of sediment mass, but we disagree with the contention that ground surveys are quick, cheap, and precise. Measurements of runoff and sediment movement from fields and in streams are costly, and some of the methodologies used, particularly those for bedload (the portion of the sediment load that moves by rolling and dragging at the bottom of a stream), leave much to be desired. Data collection programs on many streams have been abandoned due to the expense involved. Solutions to these problems must be sought through improved technologies and strategies, which will require significant research investments. In any case, these studies will often have little relevance to the quantification of on-site soil loss, as defined above.

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Response

We are grateful for the comments of Nearing and colleagues. First, we express our admiration for the USLE and RUSLE, for the thousands of plot-years of data required to derive them, and for the scientists who have developed and improved them over the past half-century. Because they are based on plot data, these equations predict gross erosion rather than net erosion, as stated by Nearing *et al.* These equations were developed for agricultural and other land development planning, and for those purposes, they have no peer. However, when one goes from the plot scale to field, regional, or national scales (geomorphological scales) and to long time periods (the scales, of almost a quarter of a century, of the National Resource Inventory), then results are problematic and problems of interpretation can occur (*1*).

Nearing, Norton, and Zhang recently stated that "[s]omewhat over half the approximately 5 billion tons of soil eroded every year in the United States reaches small streams" (*2*). Because this figure refers only to agricultural erosion, significant amounts must be added for nonagricultural erosion (for example, roadside gullies and construction) and ongoing geologic erosion (*3*). But even the minimal amount specified above would mean that more than 2.5 billion tons of sediment reach U.S. streams each year. This is more than five times the annual sediment yield of U.S. streams of 0.5 billion tons (*4*), which itself is largely augmented by channel and bank erosion (*5*). Thus, a minimum net average of more than 2 billion tons of sediment from agricultural erosion must be deposited in streams and on floodplains each year. Where is it? Such mass would be unevenly distributed so that deposits would be deep and accumulating rapidly in some places, and thus be easily detectable. But no one has found this missing mass, to our knowledge. Accounting for the other half of the 5 billion tons—presumably lost as colluvium on fields or between fields and streams—is also problematic. Surely, soil scientists would be finding and systematically reporting on such rapid accretions of colluvium. Such large predicted masses must eventually be accounted for, because only about 1% of eroded soil is soluble.

Although we applaud the limited verification of the equations over the past 20 years, verification of national erosion rates must take place at the geomorphological scale taken on by the National Resource Inventory to be scientifically acceptable, and this clearly has not been done. Having been concerned with field measurements of sediment for more than 30 years, we appreciate the effort it takes to make such field measurements, and we apologize if we implied that it was easy. However, usable data, especially for off-farm damages, will require hard work. We look forward to scientists' use of different approaches at different scales, working together to ascertain problems and prescribe solutions.

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Thoughts on the Causes of Tree Mortality in Appalachia

In an article on the history of atmospheric sciences by Paul Crutzen and Veerabhadran Ramanathan (Pathways of Discovery, "The ascent of atmospheric sciences," 13 Oct., p. 299), there is a picture of dead trees in the Great Smoky Mountain National Park (p. 301). The caption indicates that these are red spruce (*Picea rubra*) that have died from acid rain fallout. First, the trees pictured are most likely Fraser fir (*Abies balsamea*) and, if so, they died from an introduced insect, the balsam woolly adelgid (*Adelges tsugae*), not from air pollution. Second, mortality rates for red spruce in the southern Appalachians are not elevated above what are considered to be normal background levels.

And finally, there is no scientific evidence that the pictured trees—or, for that matter, trees anywhere in the eastern United States—have died from either acid rain or ozone pollution. This statement should not be construed to mean that air pollution is not a problem. Rather, it simply emphasizes that air pollution in the eastern United States has not yet reached levels that allow researchers to make a direct link to tree mortality.

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Insects, not air pollution, were most likely the culprits.

Editors' Note

Although many trees have been damaged by acid rain, an environmental fact that the image was intended to illustrate, the particular tree damage apparent in the picture is most likely due to insects. The editors regret the error.

CORRECTIONS AND CLARIFICATIONS

News Focus: "For 'father' of abortion drug, vindication at last" by M. Balter (6 Oct., p. 39). Although Gregory Pincus held a titular faculty appointment at Boston University, he did not perform his pioneering research on the oral contraceptive there, as stated in the article. That work was done at the research institute he cofounded, the Worcester Foundation for Experimental Biology in Worcester, Massachusetts.

NetWatch: "Computer nostalgia" (29 Sept., p. 2235). The Computer Museum History Center is located in Mountain View, California, not Palo Alto.

News of the Week: "Element 107 leaves the table unturned" by R. F. Service (25 Aug., p. 1270). In the penultimate paragraph, the chemical name of BhO_3Cl is bohrium (not barium) oxychloride.