

mental benefit analysis” for making decisions on the management of contaminated sites within the DOE nuclear weapons complex. We are not presently in a position to offer critique or endorsement of the specific frameworks and approaches referenced by Efroymsen. However, we encourage DOE to investigate the merits of such concepts with the goal of achieving cleanup decisions that provide optimal protection of both human health and environmental quality.

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Support for Steiger’s Policies

JOCELYN KAISER’S ARTICLE ON WILLIAM Steiger, the point person on international health for U.S. Health and Human Services (HHS) Secretary Tommy Thompson (“The man behind the memos,” *News Focus*, 10 Sept., p. 1552), is decidedly one-sided. She

interviewed me at length, and although I support his policies and admire his professionalism, none of my views, or those of other supporters, are mentioned.

Scientists cry “academic freedom” when their travel is cut, as happened with the AIDS conference in Bangkok. Steiger, as well as journalists and health experts, knew this conference had become unruly and unproductive (1).

Kaiser writes that Steiger’s support of the Bush administration’s “controversial” position on abstinence in HIV prevention programs “ruffled feathers” among researchers. She neglects to mention how promotion of abstinence is dramatically reducing HIV/AIDS infections in Uganda (2).

Furthermore, Steiger is doing his job when refusing to fund any conference that undermines the administration’s approach to procuring safe and effective AIDS drugs. Kaiser fails to mention how the administration’s position has been supported by the World Health Organization’s (WHO) removal of five AIDS drugs from its list because of unproven quality. South Africa has banned at least one of the drugs that the Administration refused to buy until tested.

The fact that Steiger approves HHS staff involvement in WHO activities may upset

some researchers. Yet all government agencies routinely approve which researchers attend meetings as U.S. representatives. It is Steiger’s job to coordinate the U.S. position with inputs from many agencies with expertise most appropriate for any particular conference. He does this job in the best interests of the country, and does it well.

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1. For example, see these op-ed pieces: S. Mallaby, “AIDS activists misfiring,” *Washington Post*, 18 July 2004, p. A17; L. Garrett, “Bragging in Bangkok,” *N.Y. Times*, 16 July 2004, p. A21.
2. E. C. Green, *Rethinking AIDS Prevention* (Praeger, Westport, CT, 2003).

The Gulf of Mexico’s Dead Zone

DAN FERBER’S NEWS FOCUS “DEAD ZONE FIX not a dead issue” (10 Sept., p. 1557) gives too much credence to assertions that reducing nitrogen pollution would not shrink the extent of hypoxia in the Gulf of Mexico. The EPA report focuses solely on ratios of concentrations of dissolved inorganic nitrogen (DIN)

and dissolved inorganic phosphorus (DIP), mainly in the lower Mississippi River, to suggest that inputs of phosphorus could control phytoplankton production on the continental shelf and, thus, the scale of summer hypoxia.

Decomposition of plankton biomass along the inner shelf west of the river mouth depletes dissolved oxygen in the denser bottom waters. In the spring and early summer, plankton biomass accumulates in surface waters enriched with DIN but often with extremely low DIP concentrations. If

there were severe phosphorus limitation, how can this biomass be grown? Almost certainly, it is because there are other phosphorus sources, including recycling from organic material, large reservoirs in bottom sediments, and the deeper Gulf of Mexico. Surface-water organisms rapidly take up any DIP supplied, keeping DIP concentrations very low, indicating that DIN:DIP ratios are notoriously unreliable indicators of nutrient limitation (1). These other phosphorus sources may be unconnected or indirectly connected to seasonal river inputs and, thus, may prove

difficult if not impossible to control.

More comprehensive assessment strongly indicates that nutrient pollution, particularly in the form of nitrogen from Mississippi Basin agriculture, is the principal cause of hypoxia and that improved agricultural practices coupled with restoration of wetlands in the river basin are the only solutions (2). With better understanding, it might prove effective to reduce both nitrogen and phosphorus inputs as is being pursued elsewhere (3), but a solid body of science indicates that substantial reductions in nitrogen loads are required to reduce the extent of hypoxia (4) and, further, that curtailing phosphorus without reducing nitrogen inputs might actually extend the effects of overenrichment to a larger area (5, 6).

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TECHNICAL COMMENT ABSTRACTS

COMMENT ON "The Origins of Genome Complexity"

Vincent Daubin, Nancy A. Moran

Lynch and Conery (Reports, 21 November 2003, p. 1401) claim a universal relation between genetic population size and genomic size and complexity, but their treatment of bacteria is invalid. Their estimates of polymorphism for bacteria largely reflect evolutionary divergence of independent clonal lineages rather than selection efficiency within cohesive species. An alternative measure of genetic drift shows no relation to genome size.

Full text at www.sciencemag.org/cgi/content/full/306/5698/978a

RESPONSE TO COMMENT ON "The Origins of Genome Complexity"

Michael Lynch, John S. Conery

Daubin and Moran claim that prokaryotes do not have larger effective population sizes than eukaryotes, and also argue that genetic drift is a minor force in prokaryotic genome evolution. These arguments are mutually inconsistent and are contrary to a substantial body of empirical and theoretical work.

Full text at www.sciencemag.org/cgi/content/full/306/5698/978b