transfer (SCNT) for research is an attempt to develop generalizable knowledge. Research cloning would not, under any circumstances, be followed by embryo transfer and thus would have no connection with reproduction.

The closest analogy to a federal ban on research cloning would be a legal ban on all human embryo research in the United States. Although the U.S. Congress has restricted federal funding for such research, it has not enacted a nationwide ban on the research. Thus, my argument stands: A federal statutory ban on SCNT research would be unprecedented.

Meilaender contends that I favor federal preemption of state policies on SCNT research. The laboratory of the states has been actively engaged with the topic of cloning. During their 2003 sessions, legislators in 25 states have introduced bills that explicitly discuss cloning; in one of these states and in three additional states, nuclear transfer for embryonic stem cell research (without mention of the word "cloning") has also been the focus of proposed legislation (*1*). In my Letter, I advocated only that SCNT research should be subject to transparent public oversight, not that this oversight should be provided by the federal government. If states—for example, California— develop their own approaches to regulating this area of biomedical research, their policy innovations should be welcomed. However, even if uniform national standards were adopted or a federal regulatory system were enacted for SCNT research—for example, through the Food and Drug Administration or a newly created advisory committee—such national guidance or oversight would be qualitatively different from a preemptive federal ban on performing the research.

Meilaender's contention that I want federal control of SCNT research—by bioethicists, no less—is without foundation. Nowhere did I advocate such a policy, nor do I think it would be a good idea. In fact, Meilander and other members of the 10-7 PCBE majority, who argue for a temporary federal ban on human SCNT research, are the people advocating federal preemption of policies that have been negotiated and adopted at the state level.

Finally, Meilaender and I disagree on what public policy should be chosen when ethical opinion on research cloning is sharply divided and while national and international ethical discussion and debate continue. Here I must acknowledge advocating a conservative, incremental political philosophy. In my view, any legal ban on research should be enacted only as a last resort and in the face of serious harms to individual persons or a clear and present danger to society. The burden of demonstrating that SCNT research presents this kind of imminent threat—and that no less extreme measures will be adequate to address this threat—should be borne by the commentators and the policymakers who would prohibit the practice of such research. Cloning research is not a threat to society, and making a line of research a criminal act is not the least restrictive way to deal with any threat that critics may perceive.

Ethical standards for research cloning should be developed and adopted. One hopes that critics of the research will assist in creating those standards. In the interim, however, this promising line of biomedical research should be permitted to continue, not banned by an act of Congress.

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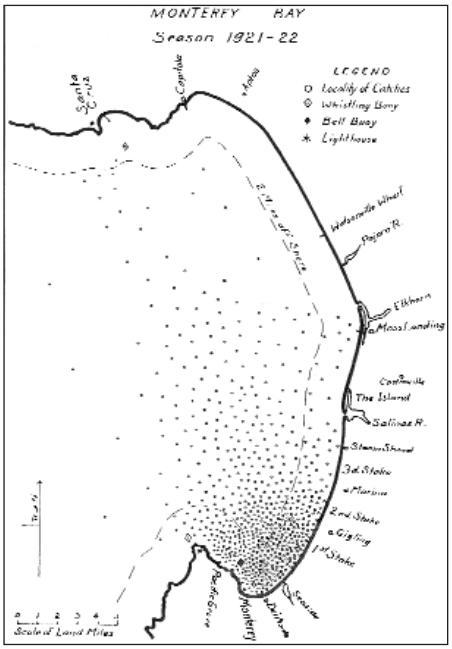
Reference

1. J. Brainard, *Chronicle of Higher Education*, 28 March 2003, p. A22.

Sardine Fishing in the Early 20th Century

IN THEIR REVIEW "FROM ANCHOVIES TO sardines and back: multidecadal change in the Pacific Ocean" (10 Jan., p. 217), F. P. Chavez *et al.* describe regime shifts in the Pacific that are supposed to correspond with cycles in the populations of anchovies and sardines that are "difficult to explain on the basis of fishing pressure." For example, they describe a cool "anchovy regime" from about 1900 to 1925, followed by a warm "sardine regime" from about 1925 to 1950, which they link with the boom in the California sardine fishery. Perhaps Chavez *et al.* are correct in general, but the California sardine fishery in the early part of the 20th century was documented in remarkable detail in the Fish Bulletin series of the Division of Fish and Game of California, because it was recognized that detailed knowledge of the fishery "is particularly necessary when applying any form of catch analysis… as a means of demonstrating the presence or absence of depletion or of natural fluctuations in supply" (*1*, p. 5). This history indicates that sardines were abundant during the supposed "anchovy regime" of the early 20th century.



Locality of sardine catches. The fishing season 1921–22 is represented and illustrates the concentration of catches in the cove of the bay near the town of Monterey, where the canneries are located. Locality names are those used by fishermen. [Reprinted from (*1*)]

Sardine fishing in Monterey began around 1903, and the fishery was substantial enough by 1919 that the division established a laboratory at Hopkins Marine Station (adjacent to the developing "Cannery Row" in Monterey) to monitor it. Presumably, the canneries were built in response to the existing rather than anticipated abundance of sardines, and catch was limited by demand rather than supply. Figure 3 from Fish Bulletin 19 (*1*) (at left) shows the location of catches in Monterey Bay in the 1921–22 season, which was described as typical for 1919–26 (with no suggestion that abundance had recently increased). About 75% of the catches were made within 5 miles of the canneries. This did not last. Fish Bulletin 19 notes that "[p]eriods of scarcity of sardines in the bay have been repeated since 1925, and the intervals of failure are apparently increasing in duration. This has led to a belief among many fishermen that the greatly increased seasonal catch of the last four years (1926–1929) has been too great a drain upon the local supply of fish" (*1*, p. 9). The catch continued to increase, but only as the small, open, lampara boats that characterized the fishery during the supposed "anchovy regime" were replaced by purse seiners that were large and fast enough to work up and down the open coast.

The developing understanding of oceanic regime shifts should be a great help to fisheries management, but fisheries still need to be managed. I am perhaps unusually sensitive on this point, having participated briefly in the Monterey sardine fishery near the time of its final collapse in the late 1950s, when spotters in small planes helped us find the few remaining small schools along the Central California coast.

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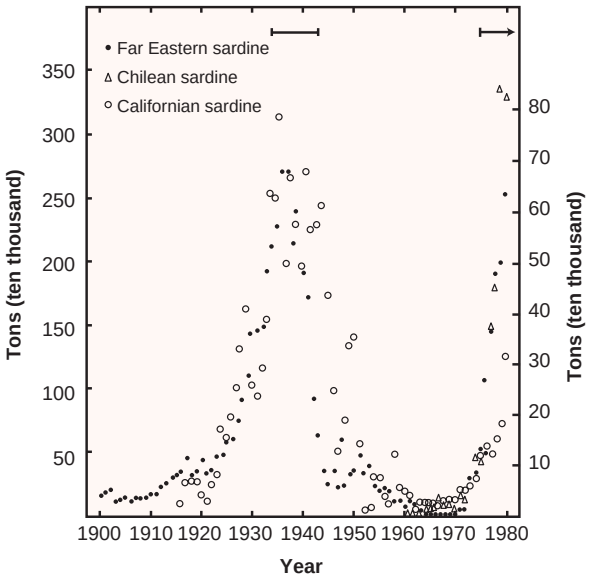
Reference

1. W. L. Scofield, *Sardine fishing methods at Monterey, California*, *Fish Bulletin No. 19* (Division of Fish and Game of California, Sacramento, CA, 1929).

Response

OUR REVIEW DESCRIBES BASIN-SCALE synchrony in the catches of several commercially important stocks of small pelagic fish. It is this synchrony that we suggest is "difficult to explain on the basis of fishing pressure." We thank Williams for calling our attention to the early literature on the California sardine fishery, but argue that the data he presents is consistent with our Review. Figure 1 (above) from the seminal paper by Kawasaki (*1*) shows that the California sardine catch in the 1921–22 season was very similar to the catch after the decline of the fishery in the 1950s. These levels are about an order of magnitude smaller than the highest catches recorded around 1935. The sharp increase in catches began a few years after 1922. Further, although Monterey Bay was the nucleus of the California sardine fishery, catch in Monterey Bay does not always represent that for the entire California domain (including Baja). Monterey Bay may be a refuge during environmentally adverse periods. We must also make clear that the period for the multidecadal changes fluctuates between 40 and 60 years. A close look at Fig. 1 in our Review shows that in panels A, B, and C, the change of sign occurs closer to 1920. Finally, there is no mention in our Review of not managing fisheries, only that knowing about this natural variability, as Williams himself notes, should be a great help to fisheries management.



Large-scale variations in catch of three species of sardines, Far Eastern, Californian, and Chilean. [Redrawn from (*1*)]

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Reference

1. T. Kawasaki, *FAO Fish. Rep.* **291**, 1065 (1983).

African Food Security and Population Growth

IN THEIR POLICY FORUM "SCIENCE FOR African food security" (21 Feb., p. 1187), G. Conway and G. Toenniessen fail to prescribe for a key factor in the equation for food security: rapid population growth. They say it now averages about 3% annually across Africa.

Their prototypical "Mrs. Namurunda" is presented as a single mother with four children. At that rate, the population will double in every generation. If a generation is measured by the age of the mother at first birth, which I would guess is about 16, the population and food demand will double in about that time. Actually, I am surprised they did not depict Mrs. Namurunda as having another child or two as a result of increased fertility, thanks to better nutrition.

So the story should include an acronymic organization working to bring fertility down to replacement levels, at the same time as the Rockefeller Foundation is working to increase food production to stabilize current demand levels.

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Response

TANTON SEEMS TO HAVE MISSED ONE OF THE key points in our story of Mrs. Namurunda. Increased crop productivity on her farm not only improves the family's nutrition, it also generates income to pay for the children's health care, to send her daughters to school, and to raise the family's overall economic well-being.

In numerous developing countries, such farm-based economic growth has been shown to lower desired family size and reduce fertility (*1*). Kenya began such a demographic transition in the early 1970s, with its total fertility rate dropping from over 8 births per woman to 4.15 currently (*2*). At this rate, the population is still rising rapidly and may double within a generation, but Mrs. Namurunda's daughters and their classmates are likely to choose to have still fewer children, leading to a replacement-level fertility rate in one or two generations. Fortunately, as Tanton suggests, there are a variety of local, national, and international organizations—yes, all with acronyms—helping to provide the agricultural technologies, health care, education, and family planning needed to make this a reality.

GORDON CONWAY AND GARY TOENNIESSEN

Rockefeller Foundation, New York, NY 10018–2702, USA.

References

1. J. Bongaarts, *Science* **263**, 771 (1994).
2. *World Population Prospects: The 2000 Revision* (United Nations Population Division, New York, 2000).

CORRECTIONS AND CLARIFICATIONS

News of the Week: "Congress fiddles with law while scientists burn" by N. Lubick (6 June, p. 1486). The article misrepresented researcher Peter Tyack's views on the effects of sound on marine mammals. Tyack believes that ship noise and other sounds that impact broad geographic areas have the potential to affect large numbers of marine mammals. Also, the court decision last January that halted Tyack's research on sonar and marine mammals in the Pacific found that the National Marine Fisheries Service, not Tyack, had violated the National Environmental Policy Act when it amended Tyack's research permit without conducting an environmental assessment.

Reports: "Regulation of aging and age-related disease by DAF-16 and heat-shock factor" by A.-L. Hsu *et al.* (16 May, p. 1142). The authors stated incorrectly that Lithgow's lab had previously used an *shsp::gfp* fusion to assay *shsp* gene expression. Instead, this group used an *shsp::lacZ* fusion and anti-SHSP antisera [G. Walker *et al.*, *J. Gerontol.* **56A**, B281 (2001)]. In addition, as the Hsu *et al.* paper was going to press, Walker and Lithgow reported that overexpression of *hsp-16* can extend *C. elegans*' life-span [G. A. Walker, G. Lithgow, *Aging Cell* **2**, 131 (2003)], and, using microarrays, McElwee *et al.* observed an increase in *shsp* expression in insulin/IGF-1 pathway mutants [J. McElwee *et al.*, *Aging Cell* **2**, 111 (2003)], as did Hsu *et al.*

Reports: "Genetic evidence for local retention of pelagic larvae in a Caribbean reef fish" by M. S. Taylor and M. E. Hellberg (3 Jan., p. 107). Much of the locality information used in Fig. 1 was based on work performed by Patrick L. Colin [P. L. Colin, *Neon Gobies* (T.F.H. Publications, Neptune, NJ, 1975); P. L. Colin, unpublished data], which was not acknowledged. The authors regret this oversight.

Research Articles: "The genome sequence of the malaria mosquito *Anopheles gambiae*" by R. A. Holt *et al.* (4 Oct., p. 129). The name of the 33rd author was spelled incorrectly. It is Kabir Chaturvedi, not Kabir Chatuverdi.