the limits of the conventional homologous recombination methodology (frequency of conversion ranging from 1 to 1000 to 1 in 10^6) led us to select methods of detection effective over a range of frequencies. Apparent conversion can always be argued as contamination or random reversion of a mutation, so we used multiple methods of detection. The automated sequence analysis methodology that uses fluorescent dyes is approximately equal in sensitivity to a properly controlled restriction polymorphism analysis. In response to this criticism, we tested whether a mixture of the β^S and β^A cells would generate the expected results and pattern. We established that when a mixed sequence is detectable by sequencing, it is also evident in the polymorphism assay. Consequently, the lack of Bsu 36 1 digestion (*2*) in the sickle homozygous cells indicates that the cells are not, nor did they become, polymorphic. The conversion experiment has been carried out many times, with parallel detection by polymorphism and sequence assays, and although there is variability in the frequency of conversion, we can detect the conversion event in untreated cells by both assays, which indicates that the methodology does represent an advance over existing technologies. The appearance of double and even triple peaks in automated sequence chromatograms is a common occurrence and is rarely indicative of contamination. We have sequenced both strands in several samples to rule out this possibility.

3) Cloning. It is imperative that the stable inheritance of the introduced mutation needs to be demonstrated to rule out trivial explanations. Because we encountered difficulty in cloning the cells used in this study, we have relied on demonstrating conversion of the beta allele in two different cell types, human HeLA and HUH7, using the SC2 oligonucleotides described in the report (*2*). Both cells are amenable to cloning by limiting dilution, and we have completed experiments that confirm stable genetic changes at the specific base of the β-globin gene. Other workers have recently, independently completed a study showing the targeted conversion of both the alkaline phosphatase and the β globin genes in the genome of HUH7 cells (*4*).

We are aware that our two papers (*1*, *2*) have led many (including ourselves) to test the method on multiple targets and in different cells. We mentioned in our report (*4*) the potential variability of different cell types and frequency of conversion by highlighting our lack of success in the TK6 cell line. Future studies should reveal how this technology is applicable to different cell types and genes.

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Synergistic Effect of Environmental Estrogens: Report Withdrawn

I write to formally withdraw the report "Synergistic activation of estrogen receptor with combinations of environmental chemicals" (7 June 1996, p. 1489) (*1*), for which I was corresponding author.

We have conducted experiments duplicating the conditions of our earlier work, but have not been able to replicate our initial results.

Also, since our publication in *Science* (*1*),

others have been unable to reproduce the results we reported (2). Meanwhile, people in many walks of life have, on their own, put great weight on this report as the basis for much discussion, thought, and even public policy.

Whatever merit this publication contained, and despite the enthusiasm it generated, it is clear that any conclusions drawn from this paper must be suspended until such time, if ever, the data can be substantiated.

In our laboratory, for the last 6 months, we have conducted experiments to elucidate the mechanisms to explain the phenomenon of synergy between estrogenic chemicals. These studies have been directed toward understanding the roles that estrogen receptor levels (3), the difference between monomer and dimer conditions of the estrogen receptor (4), and chemical transport across the cell membrane (5) play in the action of weak estrogens. None have provided a satisfactory mechanism to explain our earlier findings. Taken together, it seems evident that there must have been a fundamental flaw in the design of our original experiment. As a consequence of our efforts and those of others, and considering the impact our report has had in so many quarters, we have decided to formally withdraw the paper and its finding. We take this step in recognition of the reliance so many have placed on our work.

Our laboratory will continue to aggressively conduct research on environmental endocrinology. We believe there are important and verifiable discoveries to be made.

The co-authors have concurred in writing with this decision.

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5. S. F. Arnold *et al.*, in preparation; D. Q. Tran *et al.*, *Biochem. Biophys. Res. Comm.* **235**, 669 (1997).

Corrections and Clarifications

In line 3 of the report "Hyperplasia of lymphatic vessels in VEGF-C transgenic mice" by M. Jeltsch *et al.* (30 May, p. 1423), "platelet-derived growth factor" should have read, "placenta growth factor."

In the letter by L. Jenkins and A. Maxam (p. 1178), under the title "Acquiring language" (23 May, p. 1177), reference 1 should have cited "*Biolinguistics: The Unification Problem*," by L. Jenkins, as "in press."

In the profile of Carl Woese by Virginia Morell, "Microbiology's scarred revolutionary" (Frontiers in Microbial Biology, News, 2 May, p. 699), William Whitman was incorrectly described as a graduate student of Woese. Whitman was a postdoctoral associate in the laboratory of Ralph Wolfe at Indiana University in the late 1970s.

Letters to the Editor

Letters may be submitted by e-mail (at science_letters@aaas.org), fax (202-789-4669), or regular mail (*Science*, 1200 New York Avenue, NW, Washington, DC 20005, USA). Letters are not routinely acknowledged. Full addresses, signatures, and daytime phone numbers should be included. Letters should be brief (300 words or less) and may be edited for reasons of clarity or space. They may appear in print and/or on the World Wide Web. Letter writers are not consulted before publication.