1310) accurately portrays the central controversy. On one side, a growing research community supported by a substantial body of analysis argues that mechanical control of chemical synthesis can be developed and can eventually be used to build devices of surprising performance. On the opposing side, silicon micromachine researchers, seeking to dismiss molecular nanomachines, resort to mere name-calling. Their failure to indicate even one scientific weakness in the analysis undercuts their plea for a summary dismissal.

The article represents as my central thesis an obvious absurdity: general-purpose molecular manufacturing systems that are "protein-sized," that is, smaller and simpler than those actually proposed by roughtly six orders of magnitude. This absurdity can easily be dismissed, but it isn't what I said.

If current analyses of molecular manufacturing are essentially correct, what is at stake? The prospects (at the end of an arduous, multidecade development path) include mechanisms able to position reactive moieties in vacuum at a rate of 10^6 per second with root-mean-square positional errors of less than 0.3 angstrom, thereby directing site-specific synthetic steps on large structures with error rates of less than 10^{-15}, thus enabling the manufacture of diverse products. These include macroscopic diamond-fiber composite structures with about 75 times the strength to density ratio of aerospace aluminum, arrays of submicron computers delivering about 10^{16} instructions per second per watt with 10^9 instructions per second per CPU, ad nauseam, ad incredulum. These conclusions are based not on wishes, but on calculations founded on standard physical models, with statistical mechanics, molecular potential energy functions, radiation damage, modeling errors, and so forth taken into account. If the case for molecular manufacturing is essentially correct, then recognizing this would reveal productive directions for research. If it is erroneous or incomplete, then identifying its failures would be a public service.

Over the last 10 years, many ideas for nanomechanisms have been rejected because physical principles have shown them to be unworkable. Others have been rejected because the shortcomings of available molecular modeling techniques make them impossible to analyze. Clearly, then, these ideas can be critiqued. The challenge for the critics is to show that fatal flaws (or crucial uncertainties) remain in the surviving family of proposals. Thus far, they seem content to make empty attacks on person and style. Perhaps they can muster a more intelligent argument. I'd be happy to respond.

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Your 29 November issue on nanotechnology clearly illustrates the rapid advances being made in this area. In January 1989, we conducted a survey of 25 scientists already working on nanotechnologies (*1*). Respondents believed that advances would generally occur first (and be of more commercial value) in the nonbiological arena; for example, they thought that tools for manipulation at the molecular level would be available within 2 to 5 years for nonbiological structures, but in 5 to 10 years for biological ones. Computer interfaces with nanoscale devices were thought to be possible in 5 to 10 years for nonbiological structures and in 10 to 25 years for biological ones. Self-replicating microstructures, however, which are an important component of Eric Drexler's vision, were seen as more likely earlier in the biological arena—within 5 to 10 years compared with more than 25 years for nonbiological structures. Respondents were also asked to identify technologies most important as precursors to full-scale nanotechnology: research on molecular structure was named most often, followed closely by electronic microstructure fabrication, the scanning-tunneling microscope, bonding, molecular electronics, and mechanical microstructure fabrication.

Our survey was part of a larger study concerning the possible social and economic effects of nanotechnology and of the possible role of government in stimulating research. We found that the most serious impediment to progress was likely to be the fact that members of the many diverse disciplines working at the nanoscale would not be aware of the rapid advances made outside their own fields. We suggested establishing an interdisciplinary committee whose task would be to exchange information and ensure that others in their own disciplines were aware of new developments elsewhere.

We also suggested that the committee include some nonscientists, including perhaps an ethicist and a philosopher. The potential for nanotechnologies to undergird a program of massive social control is at least as strong as their ability to provide small, cheap electronic and biological machines that could stimulate economic development.

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REFERENCES

1. *Assessing Moleculor and Atomic Scale Technologies (MAST)* (Policy Research Project on Anticipating Effects of New Technologies, Lyndon B. Johnson School of Public Affairs, Austin, TX, 1989).

Gene Mappers at Cold Spring Harbor

I would like to comment on the article by Leslie Roberts (News & Comment, 15 Nov., p. 932) about the "marriage broker" role that the Human Genome Organization plans to adopt in response to concern from the mapping community that the Human Gene Mapping workshops be preserved. Roberts ascribes to David Cox, a genome researcher at the University of California, San Francisco, the opinion that two annual meetings for "physical mappers," one organized by *Science*, the other by Cold Spring Harbor Laboratory, "have a high-tech focus that essentially ignores the old-style gene mappers. . . ." While the Cold Spring Harbor Genome Mapping and Sequencing meeting, held annually in the spring since 1988, has focused on the technical developments that have revolutionized genome mapping and analysis, the platform has always been and remains open to all relevant disciplines and approaches. Indeed, the presentation by Cox of his radiation hybrid mapping method was a highlight of the 1988 meeting. The development of the technologies for variable number of tandem repeats and for micronucleotide repeats for genetic mapping of Mendelian and polygenic traits has also been featured. Given all of the remarkable technical developments, it is notable that this meeting has consistently highlighted the power of the genetic approach and the study of mutations. It has introduced the best in yeast artificial chromosome cloning, fluorescence in situ hybridization, microdissection cloning, contig analysis, and all forms of polymerase or ligase chain reaction and DNA sequencing technology to eager audiences. The continued aim of the Cold Spring Harbor meeting will be to reflect new directions and progress in all aspects of genome mapping.

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Erratum: The hypothetical wheel of carbon atoms shown in the illustration on page 1311 accompanying the News Report "The apostle of nanotechnology" by Ivan Amato is a chemically dubious structure generated by computer program. It was inadvertently published instead of a different, correct structure.