

genes so isolated by the laboratories of Weinberg, Wigler, Cooper, and others (Research News, 14 May, p. 724) are striking in their similarity to viral oncogenes and to each other. If we were to accept the notion that these are the genes mutated by environmental agents, then the target size for the environmental mutagenic component of carcinogenesis would be very small indeed, about 1/100,000 of the cell genome.

I suspect rather that these oncogenes represent familial genes for susceptibility to cancer, rather than genes whose somatic mutated alleles are the result of environmental insult. If so, then the interesting question becomes, Which are the genes that the environment acts on? Here I think one can more fully elaborate Littlefield's point. Those genes are likely to be the only ones that cannot be obtained in the NIH 3T3 transfection assay, because NIH 3T3 cells are already transformed with regard to the phenotypes they control. Therefore I predict that they will be found as the set, probably a large set, of genes recoverable when NIH 3T3 is used as a donor, normal precrisis cells are used as recipients, and the selective assay is based on some of the differences between the two cells, such as colony-forming ability, growth in low serum, or infinite lifetime.

ROBERT E. POLLACK
*Department of Biological Sciences,
Columbia University, New York 10027*

Empirical Research in Economics

I write in response to Wassily Leontief's letter of 9 July (p. 104) criticizing academic economics.

The most powerful ideas are the most sweeping ones, and so they are necessarily the most abstract and require statement in precise and manipulable terms. It is not surprising that the official journal of the American Economic Association would seek out theoretical articles. They become the basis for empirical research or they are fundamental because they might challenge such research. For example, the hottest theory extant in economics is that of rational expectations, which challenges standard macroeconomic research.

Leontief focuses misleadingly on one journal. The explosion in economic study is reflected in an explosion in the number of journals, many of which specialize in empirical work.

There is no doubt that theory is more glamorous than fact-grubbing and that the more elegant the theoretical presen-

tation, the higher up stands the economist in the pecking order. Leontief argues that this distorts the allocation of resources between theory and empiricism in the profession. Whether the workings of a "free market" are interfered with by an academic elite or whether a "free market" would still not allow for externalities from an over-abundance of theory are perhaps key questions. My own feeling is that mathematical generalizations are the most powerful generalizations, and I do look up to those who demonstrate these skills in the pursuit of interesting (widely applicable) theories.

Leontief should cast more doubt on empirical research. Perhaps the problems may lie in the arbitrary assumptions that have to be made because of inadequate data. And Leontief has rightfully been in the forefront of those rebuking the U.S. government for budgetary cuts that affect the data base. But stochastic disturbances, the infinite variability of human behavior, make the results of empirical research relevant to a limited time-space context and invariably of little help in forecasting.

JACOB COHEN
*Department of Economics,
University of Pittsburgh,
Pittsburgh, Pennsylvania 15260*

Appeal from the Soviet Union

Well aware of the permanent attention given by your journal to the problems of international solidarity of scientists, I ask you to publish my letter. Only the really wretched and inhuman conditions in which I have been forced to exist for a long time are compelling me to write it in search of help.

In January 1981, I was fired from my position as a junior research member of the Institute of Philosophy of the U.S.S.R. Academy of Sciences, and since then I have not been able to find any other employment. The reasons for the dismissal had nothing to do with my professional performance. Being a specialist in science studies, I have published since 1974 about 40 papers in the fields of history, sociology, and philosophy of science; but, being a Jew, I committed an unforgivable crime when I began to exchange letters and offprints with foreign fellow students on my own 3 years ago, as my superiors decided that these communications clearly revealed my secret intention to find a job abroad. So I was discharged as soon as possible under the false pretext that my unpublished monograph *Aspects of Theory of*

Science did not correspond with the aims of the Institute of Philosophy. As a rumor that I was going to leave this country quickly spread through the circle of my colleagues, I was virtually placed on a black list, with my professional career completely ruined. For a year I have been trying to appeal to the Academy authorities, but all my attempts have been in vain. I meanwhile received a formal invitation to come to Caltech as a visiting professor, but I was not even able, being a person without an official status, to apply for an exit visa. At last, after 15 months had passed, I was compelled to ask for permission to emigrate from the U.S.S.R. On 23 July, I was refused on the absurd pretext that I had no sound motives for emigration.

I never intended to make my case public, but now I have no other choice. I am quite certain that I shall never be permitted to continue my professional life in the U.S.S.R. and, because of my health, I cannot even earn my living working as a yardkeeper. Any day I can be legally expelled from Moscow on the grounds of my so-called parasitic mode of life. I know the power of the public opinion of scientists, and now I am appealing to the international scientific community and setting all my hopes on its understanding and assistance.

ALEXEY E. LEVIN
*Vargui ulitsa 24, kvartira 90,
177133 Moscow, Union of
Soviet Socialist Republics*

Not Normal Littermates

In the caption of the photo accompanying the article "Brain receptors for appetite discovered" (Research News, 29 Oct., p. 460), the mice shown with an obese mutant animal are incorrectly identified as normal littermates of the obese.

The nonobese mice are, in fact, the mutants Himalayan and piebald spotting and illustrate size difference only. The obese (*ob*) gene is maintained on C57BL stock at the Jackson Laboratory, and the normal littermate is therefore also black.

PRISCILLA W. LANE
*Mouse Mutant Stock Center,
Jackson Laboratory,
Bar Harbor, Maine 04609*

Erratum. In the report "Color vision is altered during the suppression phase of binocular rivalry" by Earl L. Smith III *et al.* (19 Nov., p. 802), four entries in Table 1 (p. 803) were incorrect. The dominance scores at 460 nanometers should be 4.37* for both subjects E.S. and D.L., and the dominance scores at 640 nm should be 4.37* for E.S. and 4.64* for D.L.