

Reflections

Three Moles

Paul A. Samuelson



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Paul A. Samuelson, a Fellow of the American Academy since 1942, is Institute Professor of Economics Emeritus at the Massachusetts Institute of Technology. He was awarded a Noble Prize in Economics in 1970.

After Easter 1945, within the World War II research labs, conviction grew that Hitler's defeat was just around the corner. Understandably, hopes for a return to peacetime academic life began to emerge. Canny guys within the Office of Strategic Services and intelligence units knew that Germany, after its Stalingrad defeat, could not hope to win the war. In the Pacific, after the Battle of Midway, Allied code breakers had made certain that Japan, too, could not win its war. But Main-Street Yanks and Brits, almost up to the last gunshots, could still fear the worst. (As a dramatic example, Joseph Schumpeter, my Austrian Harvard mentor, isolated in Cambridge, Massachusetts, from December 7, 1941 to August 1945, when the nuclear bombs fell on Japan, could still believe until very late that Hitler was winning the war.)

In that 1945 springtime, as one of the few mathematical social scientists in the Radiation Laboratory at the Massachusetts Institute of Technology (MIT), I was sounded out for the job of writing the history of the Los Alamos nuclear bomb project – a paradoxical offer since officially I couldn't know that there was such a project. But no matter: wild horses could not have drawn me to that, or any, history job. Postwar macroeconomic challenges were already keeping me awake at night.

However, a second challenge arose that I felt I could not, in good conscience, refuse. Vannevar Bush, former Vice President of my own MIT, had become Roosevelt's virtual czar for science. To map out the government's peacetime organizations for science, based on lessons learned during World War II itself, Bush was formulating the basic document that became *Science: The Endless Frontier*. Advising Bush was a stellar committee of representative eminent scientists, including I. I. Rabi from Columbia and elsewhere; Oliver Buckley, head of the prestigious Bell Labs; and *wunderkind* Edwin Land, a Harvard dropout who pioneered Polaroid, where organic chemist Bob Woodward had just synthesized quinine.

A member of and secretary to Bush's committee was my MIT colleague, Rupert MacLaurin, son of Richard MacLaurin, the former President of MIT who in 1916 converted what had been Boston Tech into the modern Massachusetts Institute of Technology. Rupert, a dynamic go-getter who earned the first Harvard Business School PhD in economics (and who was the first to ski over the Andes), knew that as a non-scientist he would need to recruit a knowledgeable staff of helpers. Three of us were picked as scribes to the secretariat and, thus, indirectly to Bush's scientific advisors and potentially to Bush himself.

John (Jack) Edsall, a biochemist at Harvard, was the oldest of us three. Next came Robert (Rob) Morison, physiologist, M.D., and head of biology for the powerful Rockefeller Foundation. (Rob and his brother, my MIT colleague Elting Morison, were cousins of Harvard historian Samuel Eliot Morison.) I, not yet thirty, was the most junior, but I was the one most conversant with the mathematical branches of the social sciences.

Throughout it was made clear to one and all that we three were to be solely helpers in drafting and in arranging and recording interviews of myriad viewpoints. We finished our part of the job within a couple of months, I think, but the three of us learned a lot that went beyond what we knew about the Ivy League or the Big Ten. There was much to learn about labs at AT&T, IBM, Mayo, Westinghouse, Brookings, or United Shoe Machines. We learned that at President Robert Hutchins's University of Chicago, my undergraduate alma mater, never were equal percentage pay raises ever given. In terms of 1945 dollars, a tenured woman full professor in classics might have a \$3,900 salary, while a physicist-chemist might have a \$70,000 salary, a vast difference traced, partially, to how much of a chemist's consulting earnings accrued to the university itself.

On Bush's advisory committee there was a diversity of opinions: cautious, conservative, activist. (Bush himself never met personally with his committee's deliberators.) For brevity's sake, I'll focus on the main split in scientists' views and in academic administrators' views.

Many persons, maybe most, were impressed with how much had been accomplished during the war in governmental scientific agencies: early radar at the National Bureau of Standards; operations research at MIT and the Air Force; underwater sound research at

Harvard; research in the Radiation Laboratory at MIT; physics research in labs at Chicago, Columbia, and UC Berkeley; and the Los Alamos project. There was cryptology research, too, but this was hush-hush.

By contrast, a minority on the committee with strong libertarian views feared these accomplishments, lest the camel of government take over the whole tent. Two reputable presidents of great universities (who can be nameless) favored dividing whatever billions the federal government would allocate to science in strict proportion to state and county populations. Equal-sized geographical counties in, say, Massachusetts and rural South Dakota should have the same dollars to “spend on science.” Otherwise, they alleged, certain pushy New York City scholars with sharp elbows would end up with the lion’s share of federal grants. (Remember that notions of political correctness change a lot every half century, and I have softened their language.)

At another extreme, a committee member like Edwin Land favored U.S. merit grants to support university dropouts, like Land himself had been and what Bill Gates was later to be.

As the only living survivor of our trio, how should I describe the rather eclectic middle-of-the-road policies we three came to hope for? The best policies of what the Edsall-Morison-Samuelson trio actually hoped for did come to be realized – fortunately realized – by what *Science: The Endless Frontier* recommended, including, prominently, Pentagon support for technical innovations; National Institutes of Health (NIH) for broad medical research; National Science Foundation for soft-money grants to applicants in physics, biology, and in the more metric branches of such social sciences as psychology, mathematical statistics, and econometrics; and NASA. It should be stressed, however, that the three of us were

in no sense movers and shakers: Yankee Vannevar Bush was not one to be swayed by ribbon clerks’ syllogisms or dreams. Causation went the other way; we scribes adjusted toward what might become feasible.

Our own views, in retrospect, were less than perfect. We were a bit fearsome that non-university laboratories might grow stale and non-innovative in the absence of university teachers and students; NIH and RAND think tanks proved us to have been overly skeptical.

A reader may say the nation got much that was needed because it was all an obvious “lay-down hand.” Yes, maybe. But let me mention that my longtime Harvard friend, Willard Van Orman Quine, arguably one of the three greatest logicians of the twentieth century, wrote in *Daedalus* in 1974 that, to paraphrase, all those dollars of federal aid to science and scholarship had (net!) a *negative* effect on the advancement of science! Go figure. Though both MacLaurin and I were whelped in Schumpeter’s entrepreneurial innovation workshop at Harvard, we underestimated the burgeoning of Silicon Valley and venture-capital innovational productivity centers.

Deductive logic cannot prove or disprove policy propositions. Speaking for myself, I am glad that I was drafted for a couple of months for duties on this new frontier, where my specialized training and aptitudes could be useful. As I sum up in memory those months devoted to postwar scientific institutions, I must suspect that my per-hour contribution to the good society was accidentally near to my lifetime maximal dogooding.

Maybe through my many writings and advising to Congress, the Federal Reserve, presidents, and voters over the years I have been a useful citizen. Those end-of-war weeks with Jack and Rob delayed only a little my writing *Foundations of Economic Analysis* (1947), a sem-

inal treatise that changed economics and won a Nobel Prize for me. Nor, fortunately, did they abort my planned career program to alter postwar introductory textbooks. After half-a-century and a score of revisions, Samuelson’s *ECONOMICS* still survives as one of the best sellers (now especially to a million Chinese readers).

Summing up, ideologies do play a role in evolving scientific development. I dare to hope that a science with both a libertarian Milton Friedman and an eclectic centrist Paul Samuelson is all the better for its diversity. ■

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