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What Indicators for Science, Technology and Innovation
Policies in the 21st Century?
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Keynote Address

John Marburger
Director, Office of Science and Technology Policy
Executive Office of the President

I thank the organizers of Blue Sky II for inviting me to speak this morning on a subject that is increasingly important for science and technology policy. As a keynote speaker, I understand it is my responsibility to say some obvious things about our subject as briefly as possible.

Policy *making*, while not simple, can be pursued in a straightforward way, with traditional phases of data gathering and analysis. Policy *implementation* is exceptionally difficult and not at all straightforward. Success in either depends critically on access to reliable and well-defined data. But high quality and clear definition, while necessary, are by no means sufficient to render data useful. The data also have to be relevant to the issues policy seeks to address and they have to be accompanied by a credible interpretive framework. These requirements, obvious though they may seem, are very difficult to satisfy. My remarks about these needs and difficulties are based on my own career as a scientist, administrator, and policy advisor, during which I have struggled with policy in nearly every stage of its complex life cycle from conception to death.

In my current role as science advisor to President Bush and Director of the U.S. Office of Science and Technology Policy I am acutely aware that actions of the U.S. government have global impact, and I am deeply motivated to help make those actions as rational as humanly possible. Rationality in action, from my scientist's viewpoint, entails linking actions to empirically validated hypotheses about the behavior of the phenomena whose course over time we wish to influence. I am assuming we have general goals in mind that we want to achieve, and those goals too must be established with some idea of how we can tell if we have met them, or at least are making progress toward them.

Facts do not speak for themselves. They are meaningful only in some explanatory context. *Physicists* are fortunate in having achieved consensus on a nearly comprehensive interpretive framework for the phenomena they observe. *Economists* can mostly understand each other – or at least many of them say they do – and they use a variety of conventional interpretive frameworks, expressed in their most concrete form through mathematical models with parameters that are estimated by matching to data. *Science policy makers* tend to rely on economic models and data when they exist, but also employ ad hoc surveys and opinions offered by panels of experts. *Science policy implementers* are usually government employees and elected officials whose information comes from a variety of sources of varying degrees of visibility, with advocacy groups on the high end and science policy technocrats somewhere near the bottom. I would like to change this. I would like to have science policy tools that are so credible that their products are embraced by the advocates as well as the technocrats. I do not expect tools that approach the credibility of Newton's laws or quantum mechanics, but I believe we can move the standards for science policy making and implementing closer to what already exists in the world of economic policy.

Not that all is rosy in that world. The Economist magazine published a critique of economic models in its July 15 issue this year, recounting infamous wide-of-the-mark model predictions and noting how results, particularly of the "computable general equilibrium" models, tend to mirror the preconceptions of the model-builders. But the report also noted how influential the models are. "Big models, which span all the markets in an economy," said the report, "can make policymakers think twice about the knock-on effects of their decisions." That is a salutary effect, even if the models are not perfect. The report quotes OECD Chief Economist Jean-Philippe Cotis as saying "orders of magnitude are useful tools of persuasion." I agree. Despite their shortcomings, economic models have raised awareness of the complexity of economic policy issues, and provided insights into the possible side-effects of policy. "All models," urged the Economist report, "should ultimately be seen as pedagogical devices, their calculations a means to the end of helping policymakers think through their decisions."

I am emphasizing models because they are essential for understanding correlations among different measurable quantities, or metrics. The time series of a single metric, of course, says nothing about cause and effect. Its shape – smooth or chaotic, increasing or decreasing – may get us thinking about what is going on, but otherwise it gives little insight. Statistically significant correlations among different metrics do provide clues to an underlying model, but do not necessarily indicate a causal relationship. It is a logical fallacy to regard one metric as "indicating" another just because their time series are correlated. Both metrics could be responding to a third unmeasured or unknown driving force. I am surely not telling this audience anything new, but this fallacy is routinely ignored by advocates, and may lead to bad policies.

For example, several years ago a colleague showed me the results of an unpublished study showing an amazingly strong correlation between U.S. federal spending on non-biomedical research and the number of bachelor's degrees awarded in the physical sciences, mathematics and engineering. No similar correlation seems to exist between research funding and degree production in the bio-medical sciences. What does this mean? Can I replace scholarship incentives with R&D spending to adjust the production rate of engineers? The authors speculated about possible extrinsic effects, but did not explore them. This correlation is so strong I would really like to know what is going on. I would like to know what causal factors drive engineering degree production – and indeed production of scientists in all fields – and I need a model relating production to its inputs. Despite the fact that no model exists to explain these data, my colleague, a respected and skeptical scientist (and not an author of the study nor a government employee), was distributing them widely in an advocacy effort to obtain more funding for physical science. For other reasons this is a reasonable policy just now for the U.S., but I was reluctant to use the data without deeper understanding of its significance.

In a more exasperating case of advocacy trumping analysis, advocates have seized upon the downward historical trend of federal support of research per unit of GDP (*i.e.* non-business R&D intensity) in our country as an argument for substantially increasing government research funding. Now OECD analyses have shown that *business* sector investment in research, which is more than twice the government amount, is strongly correlated with productivity. To quote OECD's Chief Economist again: "… growth regressions point to large effects of business R&D spending on productivity." In the U.S., the federal share of *non-business* R&D has steadily increased to all time highs both in absolute terms and as a percentage of the discretionary domestic budget (exceeded only by a spike during the Apollo program in the late 1960's). But the GDP has far outpaced both the discretionary budget and the federal R&D share, which accounts for the declining federal R&D intensity that advocates deplore. (The current ratio of U.S. federal R&D to GDP stands at about 0.7% which is the OECD average. The public-plus-private sum, or total R&D intensity, is fairly stable over time, and the U.S. value of 2.7% is exceeded among large economies only by Japan's 3.1%.) But it is the *business* R&D intensity that really counts here. A declining federal R&D intensity might even be viewed as an indicator of successful policies for encouraging business sector R&D. Perhaps the U.S. is spending as much on federal R&D as it needs to, perhaps more, perhaps less. Undoubtedly it could be spending it more wisely. I can find arguments to support various positions, but the salience of the underlying correlations is low, and very few people who serve on science advisory panels are even aware of them. Advocacy groups tend to ignore detailed statistical analyses, or interpret them to suit their causes.

Federally funded R&D does play an important role in what some have called the "ecology of innovation," and we have tried to understand that role so we can work toward an effective distribution of funds among different fields. This is a universal problem for science ministers in every country. We tend to copy from each other and then cite trends in other countries to support our decisions. I am sure many in this audience are aware of the complex and decentralized nature of government-sponsored research in the U.S., which presents huge challenges to rational distribution of resources. Overall science planning and policy-making is accomplished through a bewildering variety of advisory panels, interagency working groups, and Executive Branch policy processes, the most important of which is the annual budget process that synthesizes the proposal presented annually by the President to Congress. In Congress, multiple committees and subcommittees authorize and appropriate funds in an intense advocacy environment from which politics is rarely excluded. Organizing this potential chaos would be easier if we had "big models" of the sort economists use to intimidate their adversaries. More seriously, the entire process would benefit from the level of scholarly activity that exists today in economic policy. Nevertheless, the U.S. does manage to achieve consensus on a number of science policy principles, not the least important of which is the idea that government should fund high risk long lead time basic research and the private sector should fund lower risk short lead time applied research and development. Some of these principles are embedded in the current competitiveness initiative launched earlier this year by President Bush in his State of the Union Speech.

The *"American Competitiveness Initiative"* (ACI) is a multi-component proposal to strengthen long term U.S. economic strength. This proposal is highly visible and is currently receiving favorable attention by Congress. It is notable that the most expensive part of the Initiative is the Research and Experimentation Investment Tax Credit, a tax incentive for business R&D that Congress has tended to pass year by year, but which we would like to see authorized permanently. Given the empirically inferred sensitivity of economic output measures to business R&D intensity, this is a rational policy proposal in the sense I defined earlier in my remarks. The ACI also includes tuning of the federal share of R&D as well as important education, training, and immigration proposals. We believe all these actions will improve the climate for innovation and competitiveness, but they function at a level within the economy that is only very weakly probed by existing empirical studies. We have lots of data, and we have some correlations, but we do not have models that can serve even as "pedagogical devices" for policy formation. Without these the challenge of defending a coherent pattern of actions to improve the framework is daunting.

The ACI seeks to strengthen foundations for future economic performance. Unfortunately, in our era of dynamic change, the empirical correlations that inform the excellent OECD analyses of economic performance are not very useful to science policy makers as guides to the future. They are not models in the sense that they capture the microeconomic behaviors that lead to the trends and correlations we can discover in empirical data. Take, for example, the production of technically trained personnel in China. China is producing scientists, mathematicians, and engineers at a prodigious rate. As a scientist and an educator, I tend to approve of such intellectual proliferation. As a policy advisor, I have many questions about it. How long, for example, can we expect this growth rate to be sustained? Where will this burgeoning technical workforce find jobs? What will its effect be on the global technical workforce market? Is it launching a massive cycle of boom and bust in the global technology workforce? Historical trends and correlations do not help here. Nor, I am afraid, does simply asking the Chinese policy makers what they intend. They also need better tools to manage the extraordinary energy of their society. We need models – economists would call them microeconomic models – that simulate social behaviors and that feed into macroeconomic models that we can exercise to make intelligent guesses at what we might expect the future to bring and how we should prepare for it.

I am under no illusion that either the OECD or any other single organization will be able to produce such models in a single massive effort. But I do believe it is a realistic goal to build a new specialty within the social science community – complete with journals, annual conferences, academic degrees, and chaired professorships – that focus on the quantitative needs of science policy. The U.S. National Science Foundation has launched a program in "the social science of science policy" and important conferences are taking place where such issues are discussed. There are several reasons why this is a good time to encourage such ventures.

First, the dramatic influence of information technology on almost every aspect of daily life, from entertainment to global trade, has made it very clear that technical issues will be an important dimension of nearly all future economies. In this context, science and technology policy acquires an unprecedented significance. Post World War II science policy, at least in the United States, focused on Cold War issues until the late 1980's. The 1990's were a transition decade. Since the turn of the century all science policy eyes have been on technology-based innovation and how to sustain it. Studies of government science investment strategies have a long history, but the increased demand for economic effectiveness creates a dynamic in which new approaches to science policy studies will flourish.

Second, in the face of rapid global change, old correlations do not have predictive value. The technical workforce today is highly mobile, and information technology has not only dramatically altered the working conditions for technical labor, but has also transformed and even eradicated the functions of entire categories of technical personnel. Distributed manufacturing, supply chain management, and outsourcing of ancillary functions have undermined the usefulness of old taxonomies classifying work. The conduct of scientific research itself has been transformed, with extensive laboratory automation, internet communication and publication, and massive computational and data processing power. We simply must have better tools that do not rely on historical data series. They do not work anymore. Microeconomic reality has inundated macroeconomic tradition with a flood of new behaviors.

Third, the same rapidly advancing technologies that created these new conditions also bring new tools that are particularly empowering for the social sciences. Large databases and complex models are inherent in social science research. The vast articulation of internet applications makes possible the gathering of socio-economically relevant data with unprecedented speed and affordability, and access to massive inexpensive computing power makes it possible to process and visualize data in ways unimagined twenty years ago. New capabilities for direct visualization of large data sets in multiple dimensions may render traditional statistical methods obsolete. A growing community of scientists from many different fields are inventing data mining and data visualization techniques that I believe will transform traditional approaches to analysis and model-building. These new tools and opportunities can be an invigorating stimulus for all the social sciences, including the social science of science policy.

The themes of this "Blue Sky II" meeting bear directly on the issues that make my job difficult. On behalf of all science policy advisors everywhere, I commend and thank OECD and its committees and the sponsors of this week's conference for their good work. I look forward to learning more from you about how to improve the empirical basis for science policy.