Letters

NSF Policy Analysis

M. Granger Morgan argues (Editorial, 16 Dec., p. 1187) that the National Science Foundation "is not, and has never been, a good place for a federal policy analysis job shop." This seems to me to miss the point. Rather, the question is whether there is any other government organization that can perform top-level policy analysis in science and technology. Two trends have converged to make NSF perhaps the *only* place in the Executive Branch where such comprehensive policy analysis, with a "national" rather than an "agency" perspective, can be performed. One trend is the growing recognition by the Executive Office of the President, particularly the Office of Science and Technology Policy and the Office of Management and Budget, that such analyses can make important contributions to improving policy-making on crucial national issues, especially with White House unwillingness to give the Executive Office an in-house policy analysis staff. The other trend is to treat NSF, not as a true foundation that dispassionately distributes public funds in pursuit of scientific excellence, but rather as an agency of government, responsive to changing political priorities and to the objectives of the elected leadership. One might argue that this is an undesirable evolution, but one cannot deny its reality.

The question then is whether NSF can simultaneously (i) provide effective policy analysis for both the central elements of government and its own leadership; (ii) support the kind of development of policy analysis capability called for by Morgan; and (iii) carry out its traditional function of supporting basic research and science education. It is not obvious that all of this is possible, but if it is, the benefits would outweigh the undeniable risks of politicizing NSF, which in any case is a resilient organization that historically has resisted much stronger threats to its integrity.

NSF's Division of Policy Research and Analysis (PRA) is about to issue a program announcement that proposes just the kind of longer range effort in improving the theory and practice of policy analysis that Morgan calls for; several million dollars are likely to be devoted to this effort in fiscal year 1984. If PRA did not also have a major role as a performer of policy analysis for influential users, it is unlikely that this level of funding for research support would be available. Rather than call, as Morgan does, for a "drastic reorientation of PRA" before it is well embarked on its current direction, perhaps the community of science and technology policy analysis should become more familiar with PRA's plans, criticize them when criticism is deserved, and work with NSF to advance *all* of the objectives it is seeking.

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In remarks he made to the American Society of Biological Chemists in New Orleans in 1980, John Logsdon said, "NSF has been assigned a number of tasks appropriate for a central policy staff, tasks which probably ought to be carried out by the White House Office of Science and Technology Policy" (*1*). I share that view. He went on to point out, as he does in this letter, that "the current OSTP is unwilling, and probably unable, to carry out such tasks, and NSF gets them almost by default" (*1*). This correctly describes the situation, but I do not share Logsdon's current view that this arrangement is inevitable, appropriate, or acceptable.

Agencies like the Environmental Protection Agency have tried to combine long-term fundamental work with short-term applied activities and have not done very well. The short-term activities have generally taken over, or forced out, the long-term work. To be useful, long-term technically focused policy research need not be tightly tied to or directly responsive to the current agendas of specific policy-makers. Good policy-focused research (*2*), undertaken independently, on specific problems like acid rain, or methodological issues like the treatment of uncertainty, can substantially inform and shape future understanding, public discussion, and decision processes. While NSF-supported work of this kind should be undertaken with an awareness of political and institutional realities, current Executive Office and agency agendas should not be allowed to dominate the process of problem selection. Individual investigators should be able to independently set their research agendas: identifying, proposing, and justifying research on demonstrably important problems using the classic NSF vehicles of unsolicited proposals and peer review. By fostering a tradition of technically focused policy research that is both independent and long term, NSF could help to significantly enlighten and improve our processes for managing, governing, and regulating our technological society.

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References

1. J. M. Logsdon, "The National Science Foundation as an instrument of national policy," a speech delivered to the annual meeting of the American Society of Biological Chemists: Biophysical Society, New Orleans, 4 June 1980.
2. M. G. Morgan, *Science* **201**, 971 (1978).

Nuclear Test Yields

In replying to a briefing by R. Jeffrey Smith (News and Comment, 17 June, p. 1254) about a recent American Geophysical Union symposium on the verification of nuclear test bans, Ralph Alewine and Thomas Bache (Letters, 29 July, p. 418) make several statements that we, the coorganizers of the symposium, believe are misleading or incorrect. In an invited paper, Alewine and Bache presented the views of the U.S. Department of Defense on two issues: has the U.S.S.R. complied with the 150-kiloton limit set by the as yet unratified Threshold Test Ban Treaty (TTBT) of 1976, and can a comprehensive test ban treaty be verified with high reliability? Their conclusion that many Soviet tests since 1976 exceeded the 150-kiloton limit is at odds with that of many other speakers at the symposium and with the views of many members of a panel that debated that issue at the conclusion of the symposium. The seismologists at the symposium agreed that attenuation of short-period seismic P waves is much less for waves leaving the main Soviet test site in eastern Kazakh than it is for waves from the Nevada Test Site, the source of most U.S. calibration information on seismic magnitude and yield. The disputes were about the size of that bias and about the observed body-wave magnitude (m_b) values for specific explosions. Use over the past 15 to 20 years of uncalibrated m_b

magnitudes for Soviet explosions resulted in overestimates of these explosions by factors of 3 or so. The residual disagreements are minor compared to those of a decade ago, but they still provide the basis for our contrary assertions about probable Soviet behavior.

Alewine and Bache presented yield estimates at the symposium (which they cite in their letter) derived from preliminary estimates of magnitudes by the U.S. Geological Survey. To our knowledge, they have never used and would never use such uncorrected data in any classified discussion of yield. The USGS magnitudes are rounded to the nearest 0.1 m_b unit (equivalent to a 1.25 to 1.5 factor of uncertainty in yield estimate at yields of 150 kilotons) and are not corrected for differing station distributions and thus different station magnitudes. We have recalculated magnitudes for all of the larger Soviet explosions in eastern Kazakh from 1976 to 1982 for those effects. Seven explosions since June 1979 have magnitudes very close to and statistically indistinguishable from 6.20. After recalculation, none of the magnitudes are as large as 6.3, as in the original USGS estimates. If a value of m_b bias of 0.4 is used rather than the 0.3 used by Alewine and Bache and magnitudes are not rounded to the nearest 0.1 unit, the yields of the nine explosions they cite as being of greater yield than 150 kilotons either drop below that value or are very close to it. There certainly are none that approach the value of 315 kilotons given by them at the AGU symposium for one of these explosions.

Alewine and Bache assert that an $m_b \geqq 6.2$ in all U.S. experience is associated with yields of 600 to 800 kilotons or greater. While this statement does indeed apply to hard rock explosions at the Nevada Test Site, it is not a correct statement of total U.S. experience. The United States exploded three devices on Amchitka Island in the Aleutians. The *observed* m_b data for these three events, when compared with data from the Nevada Test Site explosions, predicts a yield of 150 kilotons or slightly greater to be associated with an m_b of 6.2. Other geophysical data confirm that the Aleutian Islands area is characterized by low attenuation, as suggested by the observed m_b-versus-yield relationship. Also, the observed m_b value for the SALMON explosion in salt in Mississippi indicates that a 150-kiloton explosion in the eastern United States in hard rock would be expected to have an m_b value of about 6.2.

We are convinced that careful calibration using P waves provides estimates of yields of the large post-1976 Soviet explosions (near 150 kilotons) consistent with estimates obtained using other wave types. We believe that the assertion that has been made about alleged Soviet cheating on the TTBT, either in leaks to the press or in published documents by the U.S. government (*1*), flows from an incorrect calibration of seismic data. When one gets down to seismological details and away from seismological rhetoric, the disagreement in m_b bias is 0.1 m_b unit, not several tenths.

Finally, Alewine and Bache state the demonstrable truth that magnitudes of Soviet tests at the Kazakh test site have increased by about 0.3 since negotiation of the TTBT. Given that fact, they imply that Soviet failure to test to the limit for the first years of the TTBT suggests Soviet misbehavior and is thus an added cause for concern. Jack Anderson, in his syndicated column of 10 August 1982, presented the interpretation that the Soviets were testing to the 150-kiloton limit from the start of the TTBT and that their later conduct was based upon penetration of U.S. security and their immediate exploitation of this penetration (knowing the United States had adopted a presumably fallacious m_b bias for Kazakh) by raising all yields to our new m_b threshold for Kazakh. Why the Soviets would so obviously and for such little gain display the major fact of their penetration of U.S. security is not explained. Also, it is not pointed out that only after this increase in yield did m_b values (surface wave magnitudes) of the largest Soviet explosions at Kazakh finally reach those observed at all other test sites in the world for yields of 150 kilotons.

We suggest a scenario controlling Soviet conduct that seems much more realistic. First, Soviet conduct before the TTBT was to make essentially no large weapon tests (above 50 to 75 kilotons) in eastern Kazakh, but rather at Novaya Zemlya. Why did they do this? For the same reason that the United States conducted its multimegaton tests on Amchitka—to prevent excessive ground-shaking in nearby cities (for the United States, in Las Vegas and Reno; for the U.S.S.R., in Semipalatinsk). A fact well known to seismologists but possibly not so well known to intelligence experts is the grossly different attenuation of horizontally traveling short-period waves that occurs in different parts of the continents. At a range of about 150 kilometers (the approximate distance of Las Vegas from high-yield tests in Nevada and of Semipalatinsk from the Kazakh test site), the differing attenuations in Nevada and the eastern United States would result in a 75-kiloton explosion in the eastern United States causing about the same level of ground motion as would a megaton explosion in Nevada. Several geophysical criteria indicate that attenuation characteristics in Kazakh are similar to those in the eastern United States. Therefore, we suggest that pre-TTBT behavior of the U.S.S.R. was to accept in Semipalatinsk about the same level of explosion-induced ground motion that the United States was willing to accept in Las Vegas.

Another fact that seems apparent from observed m_b values for Soviet explosions at Novaya Zemlya is that few had yields as low as 150 kilotons. One would conclude from the pattern of observed m_b's that this yield was an unimportant reference point for them. So when they entered the TTBT, they then had to make a decision about whether to maintain the Novaya Zemlya test facility for yields of 75 to 150 kilotons only, or to essentially shut it down while accepting somewhat higher ground motions in Semipalatinsk. The magnitudes of the largest Soviet explosions in a given time increased in at least three steps from 1976 to 1979 and have remained constant since. One could interpret this gradual increase in yields at Kazakh as a deliberate and careful evaluation of the acceptability of the higher values of ground motion resulting from higher yield tests. In fact, it would have been surprising if the Soviets had immediately started testing at the 150-kiloton limit. We cannot, of course, guarantee that this is the actual rationale explaining Soviet behavior, but it certainly makes more sense than that suggested by Alewine and Bache.

Soviet violations or possible violations of the TTBT have been cited as one of several examples of the U.S.S.R.'s not living up to various arms control agreements. This may be a propitious moment for the U.S. Congress to seek an independent review of scientific issues related to the threshold and comprehensive treaties.

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1. Department of State, *Security and Arms Control: The Search for a More Stable Peace* (Government Printing Office, Washington, D.C., 1983).