Letters

"Nuclear Winter" Models

Discussions abound over what would be unleashed on our planet during a "nuclear winter" (Articles, 23 Dec. 1983, p. 1283 and p. 1293; Editorial, 24 Feb. 1984, p. 775; News and Comment, 6 July 1984, p. 30; Letters, 25 Jan. 1985, p. 356; News and Comment, 15 Mar., p. 1320; Research News, 12 Apr., p. 163). The thought of such an event is indeed serious from the standpoint not only of the human tragedy but the effect on atmospheric-oceanic balance.

In this same realm it would seem prudent and appear within the capability of our great technological and modeling expertise to carry the "nuclear winter" simulation one step further. McCracken and Luther (*1*) have shown that smaller amounts of aerosols—typical of Mother Nature's injections from volcanic sources such as El Chichón, Agung, or Krakatau—cause shifts in atmospheric circulation patterns high into the troposphere and even into the stratosphere. Would a "doughnut-like ring" of soot in the atmosphere over the Northern Hemisphere amplify such alterations? Such a shift could have major impacts on oceanic circulation with redirected heat distributions from these currents. Might it be possible that results from such modeling work would reveal effects, in addition to those already described so vividly, that would be more devastating than those attributed to the recent record El Niño?

Our study of the past 100 years of record shows statistically that warm oceanic conditions along the Pacific coast of South America are enhanced after injections into the stratosphere from sulfur-rich volcanoes located between 20°N and 20°S (*2*). We have found that for the 2 years after strong eruptions (70 events) sea-surface temperatures (SST) responded positively 86 percent of the time, whereas, for those years without major eruptions (34 events), only 65 percent gave positive SST indications. These initial results suggest a "nuclear winter" might perturb atmosphere-ocean interactions such that certain upwelling cold currents (for example, those off the west coast of the Americas) would cease and the affected areas would be anomalously warm, as in intensified El Niño–like conditions. These modifications together with Arctic-like weather over the continents would enhance storminess through promotion of strong meridional circulations in the atmosphere.

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Verification of Nuclear Testing

Although I have no serious disagreements with the recent article by W. J. Hannon (18 Jan., p. 251) when it is considered within the conditions posed, I have profound disagreements with the appropriateness of those conditions and, therefore, with his conclusions. This letter is a brief account of where I think his assumed conditions are in error, what conditions are correct, and the resultant impact on his stated conclusions.

Fundamentally, Hannon follows many of the same procedures I followed in 1976 (*1*) and in 1982 (*2*) when I calculated network capabilities. In those articles, I assumed, as Hannon does today, that the seismic network is designed to detect signals around a frequency of 1 hertz. However, in my 1976 article, I specifically excluded from consideration, for reasons that I explained (but that Hannon deems unacceptable), (i) great areas of thin-bedded salt in central U.S.S.R. as sites for decoupling; and (ii) the granitic areas of the U.S.S.R. as sites for decoupling at greater than a factor of 10. Hannon, however, allots high decoupling potential to all salt deposits and to all granitic terrains, thus reaching results that are less optimistic than those I reached.

A point that has been widely made and discussed among American seismologists during the past year is that the initial condition of both my 1976 analysis and of Hannon's analysis, which assumes detection at around 1 hertz, leads to the calculation of the comparatively low capabilities found by both of us. This condition is known to be out-of-date and in gross error if maximum monitoring capability is desired.

New data allow a drastic change in the initial condition of network analyses. These include the realizations that (i) explosions generate much higher high-frequency amplitudes than do earthquakes; (ii) these high-frequency signals are effectively transmitted for many hundreds of kilometers in terrains such as those in most of the U.S.S.R; and (iii) high-frequency noise levels are always very low at many sites within the U.S.S.R. (known by analogy with sites in Scandinavia and North America). If these recent understandings are incorporated quantitatively into the network analyses, with the network consisting of 25 simple internal stations and 15 stations external to the U.S.S.R. and detection assumed at around 30 hertz, one can predict that, even with 200-fold decoupling (not 60- to 70-fold as Hannon assumes) at all salt and granitic sites within the U.S.S.R, multistation detection of fully decoupled 1-kiloton explosions would be achieved with high probability. This result, when combined with other empirical and theoretical seismological considerations, leads to further conclusions: (i) simple nonarray stations are all that is required, not arrays, as suggested by Hannon; (ii) signal-to-noise ratios would be so high that identification at or very near this threshold would be possible, negating Hannon's conclusion about many unidentified small events of interest; (iii) the relative character of explosion and earthquake signals, as well as the distribution of Soviet seismic activity, would make the analytical load of the monitors easily manageable; (iv) there would be no problem from seasonal or aperiodic changes in microseismic levels, thus removing a proposed opportunity for potential cheating; (v) the problem of hiding explosion signals in those of earthquakes, whether near or distant, disappears; and (vi) the concept of cavity decoupling at yields of 1 kiloton or greater becomes passé because the resultant signals would be detected and identified as explosions.

Therefore, subsequent to deployment within and surrounding the U.S.S.R. of a network capable of detecting frequencies of 30 hertz or so, any fully decoupled test at or near 1 kiloton would be detected and identified with high confidence.

The pertinent data and analyses supporting these conclusions (*3*), when combined with nonseismological considerations relative to a new test ban treaty, suggest that the proper course is to negotiate a low-threshold treaty, with the threshold set at 1 kiloton, all permitted lower-yield tests restricted to a specified site, and no decoupling permitted.

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2. L. R. Sykes and J. F. Evernden, *Sci. Am.* **247**, 47 (October 1982).
3. J. F. Evernden, in preparation.

Although Evernden may disagree with the conditions stated in my article, his proposed network for monitoring a low-yield test ban treaty (LYTTBT) contains almost as many in-country stations (25) with greatly improved signal-to-noise ratios (SNR) as the networks that I described for monitoring a comprehensive test ban treaty (CTBT). Since the number and quality of in-country stations are important issues, and since Evernden had previously stated (*1*) that such treaties could be adequately monitored with 15 less capable in-country stations, I am pleased to see this similarity in our current monitoring requirements. It is not surprising, as high-frequency stations and arrays are complementary ways of achieving improved SNR. Keeping this current similarity in mind and noting that we agree that high-frequency monitoring is potentially very useful (we do not agree that it is proved), it is worthwhile to examine some of the assertions in his letter.

The distribution of media suitable for decoupling nuclear explosions in the Soviet Union is a critical parameter in determining seismic monitoring requirements. The fact that such material is widely distributed in the Soviet Union is strongly supported by available studies and is not a matter of personal belief, as Evernden's letter implies. Examination of the legend of the map cited in reference 27 of my article and the explicit discussion of the distribution of thick salt, diapirs and domes in the Soviet Union given in Fryklund's work (*2*) leave little doubt about this, even when thinly bedded regions are excluded. Furthermore, examination of Evernden's own work (*3*) shows that his reservations about decoupling in granite were stated in the context of 10-kiloton explosions, not the lower yields (with smaller, more easily constructed cavities) that we both seek to monitor with the improved SNR capabilities. Thus, the condition that cavity decoupling opportunities in the Soviet Union must be considered to be widespread is a completely appropriate and, in fact, necessary condition for the accurate evaluation of monitoring requirements in the Soviet Union.

High-frequency seismic signals have been observed for some source–recording-site combinations, and low noise sites have been found in Lajitas, Texas, and Fennoscandia. Despite our agreement about the existence and importance of these observations, it is in this area that Evernden and I have our greatest differences. These differences stem from his treatment of the unproved, extrapolated properties of high-frequency monitoring as if they were proved facts, applicable without qualification, to sites throughout the Soviet Union. In fact, evidence about the capabilities of high-frequency monitoring for the detection and discrimination of small events is limited and sometimes contradictory. Much of the support for the potential value is based on extrapolations from measurements made at limited sites at frequencies less than 30 hertz. Furthermore, the sources considered are typically larger than those of interest for monitoring and have different spectral content.

A few examples may serve to illustrate this point. The statements about differences between decoupled explosions and small earthquakes are not as well defined as Evernden's statements imply. For example, the work of Glenn *et al.* (*4*) indicates that nonspherical cavities can introduce azimuthal variations in the spectral content of the signals radiated by decoupled explosions. These variations reduce the amplitudes and make the explosion look more earthquake-like in some directions. These changes could affect both detection and discrimination at key stations. Finally, U.S. experience indicates that small earthquakes (which are of concern in the present context, but about which we know little in the Soviet Union) are probably widespread in regions of monitoring interest (*5*). Various source theories give different high-frequency properties for small earthquakes (*6*).

The effects of the source, the path, and the receiver sites on the high-frequency SNR's have not been sorted out in a way that allows meaningful selection of the recording sites. Low-noise sites have been located, but the high-frequency SNR properties have not been fully explored. The relationships among the site and path geology, the noise sources, and recording at depth are matters of current research. At the Lajitas, Texas, site there is an indication that the low noise is also accompanied by a reduction in the signal (*7*) and this reduction has been observed at other locations for conventional seismic frequencies. Finally, the variability of the SNR at stations within networks operating at conventional seismic frequencies raises significant questions about our understanding of these phenomena.

These and other uncertainties are the basis for the note of cautious optimism expressed in my *Science* article and its predecessor (*8*). Also, I have treated high frequencies as an addition to the use of arrays, so that the potential gains appear much less dramatic. The uncertainties raise significant issues which must be resolved before one can responsibly advocate a CTBT or a LYTTBT on the basis of a "technological fix" with high-frequency monitoring. Among the costs of current advocacy positions must be counted the fact that they divert attention from important military and political decisions. Such decisions must be made if there is to be a long-term, equitable test ban treaty which contributes to stability even in the face of inevitable false alarms and external tensions. Some of these decisions are discussed in my original article.

My original article and this letter do not address a LYTTBT. The monitoring requirements of such a treaty deserve far more rigorous examination (for example, a discussion of the effect of the probable uncertainty in the yield estimation in terms of the military significance of violations, calibration of the yield estimation procedures, and so forth) than either Evernden or I address here.

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