On a "Nuclear Winter"

R. P. Turco, O. B. Toon, T. P. Ackerman, J. B. Pollack, and C. Sagan (TTAPS), in their article "Nuclear winter: Global consequences of multiple nuclear explosions" (23 Dec. 1983, p. 1283), predict long-lasting subfreezing temperatures over land areas after a nuclear war (the "nuclear winter"). Their article focuses on previously neglected atmospheric radiation consequences of smoke and soot from widespread conflagrations, but does not make it sufficiently clear that changes in their assumptions and a more complete treatment can yield quite different climate scenarios.

1) The temperature change of the land surface (assumed to have a small heat capacity) would be caused by a temporary imbalance between incoming and outgoing energy fluxes. Since these fluxes are quite small, a perturbation could have major effects on the extent and even the sign of the temperature change; for example, larger particulates would in general increase the infrared absorption, as would the complicated gaseous products of combustion (*1*). The greenhouse effect would be enhanced by clouds from the large-scale injection of water vapor into the atmosphere, both as a product of combustion and from vaporized moisture and water. Smoldering combustion after the nuclear exchange could provide an appreciable energy input; as little as 1 ounce burned per square foot per day could change the outcome drastically (*2*).

TTAPS assume a uniform distribution of the particles. But could the particles survive for appreciable times (that is, weeks) in the atmosphere (*3*)? A nonuniform distribution might not produce cooling: Dust loadings higher than the TTAPS value cannot increase the visible absorption much further, but would increase infrared opacity. Conversely, lower dust loadings would admit solar radiation to heat the surface.

2) The duration of the nuclear winter is ascribed by TTAPS to the long atmospheric residence time of the particles. Indeed, volcanic eruptions have at times injected particles into the stable stratosphere, where they survived for many seasons before falling out to the earth's surface. Three points could be made.

(i) The assumed nuclear scenario is important. For example, bombs with a yield of less than 1 megaton would not project dust into the stratosphere. Several low-yield bombs are more destructive than an equivalent high-yield bomb and may therefore be preferred by military planners. Also, air bursts cause wide destruction (and more fires and smoke) than ground bursts, but airbursts create less dust to project upward.

(ii) The stable stratosphere might be destroyed, thereby limiting the duration of any climate effect. TTAPS point to destruction of ozone as a way of removing the temperature inversion at stratospheric heights, and thereby the stability. This point needs further discussion.

(iii) The conflagrations that follow the nuclear blasts and last perhaps several days would not project much material into the stratosphere (*4*). This issue cannot be settled by a one-dimensional model, but requires a mesoscale approach. The outcome is likely to be large-scale vertical convection with both upward and downward air currents. In the resulting cumulonimbus generation, there would be thunderstorms, rainout, and cleansing of the atmosphere. These processes would sharply limit the lifetime of the particulates in the atmosphere.

While the TTAPS results are presented in a properly qualified form, it is evident from the following article by P. R. Ehrlich *et al.*, (23 Dec. 1983, p. 1293) that their results are being uncritically accepted by many. That is not to say that the long-term consequences of a nuclear exchange should be discounted. Even with the TTAPS predictions reversed, a hot earth surface could threaten the survival of animals and plants. But then again, the temperature change might be negligible and so would the biological consequences.

The same issue of *Science* contains a News and Comment briefing (p. 1308) about a joint American-Soviet scientific forum sponsored by the Nuclear Freeze Foundation on 8 December 1983 (*5*). This forum sharply criticized a study prepared by the Federal Emergency Management Agency (FEMA), which suggests that food supplies would still be available after a nuclear attack. The FEMA study was faulted not only for its conclusions, "but also for its *underlying attitudes*" (emphasis mine). Senator Edward Kennedy (D–Mass.) was quoted as saying: "This kind of thinking makes nuclear war more likely because it makes nuclear war seem more bearable."

This remark raises ethical problems. First, does prediction of a global holocaust make nuclear war less likely? And, second, should scientists therefore ignore scenarios which produce less severe global outcomes?

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References and Notes

1. The climate effect depends critically on the assumed sizes and optical properties of the particles. A change in these parameters affects the ratio of visible to infrared opacities. After all, the surface of Venus has a temperature of some 400°C and provides a convincing demonstration of a planetary greenhouse effect.
2. An assumed dry mass of 2.2 grams per square centimeter [P. J. Crutzen and J. W. Birks, *Ambio* **11**, 115 (1982)] would lead directly to ~2.5 grams per square centimeter of water vapor, doubling the normal H_2O content of the atmosphere. The combustion energy of $\sim 3 \times 10^4$ joules per square centimeter can vaporize another 10 grams per square centimeter of water. The combustion of 1 ounce of wood per square foot per day corresponds to an energy release rate of ~50 watts per square meter; the average solar insolation on a cloud-free low-latitude location is ~200 watts per square meter.
3. Achieving uniformity necessarily involves 3-dimensional motions. The condition of superstability assumed by TTAPS (implying no vertical motion) must lead to highly nonuniform accumulations of Lagrangian particles according to theoretical considerations [see, for example, H. Tennekes and J. L. Lumley, *A First Course in Turbulence* (MIT Press, Cambridge, Mass., 1972)]. But an approach to uniform distribution, that is, mixing, implies contact with the surface and therefore removal of the particles. An analogy might be the dry deposition of atmospheric acidity. The perfect atmospheric mixing of CO_2 is possible only because the surface does not act as a sink for CO_2.
4. Experience from World War II firestorms suggests that 5 kilometers may be a maximum height [D. Irving, *The Destruction of Dresden* (Kimber and Ballantyne, New York, 1963), p. 175].
5. Russian calculations apparently supporting those of TTAPS were reported at the Nuclear Freeze forum [V. V. Aleksandrov and G. L. Stechikov, *J. Comp. Math. Phys.* **14**, 140 (1983)], but the authors used the same radiation specifications as TTAPS. Further, their global three-dimensional model cannot capture the mesoscale effects which might be determining the particulate content and optical thickness of the atmosphere and therefore its radiation properties.
6. The main points of this letter were presented by me at an informal session convened in connection with a symposium on global environmental problems held in November 1983 in Chicago, Ill. Some 20 participants attended the session and contributed discussion, but did not necessarily endorse the conclusions.

The evaluations by TTAPS of climatic effects of nuclear war might be taken more seriously by Soviet leadership if based more nearly on Soviet descriptions of how cities burn after being blasted in a Soviet-type nuclear war and on more realistic assumptions regarding the extent of forest, brush, and grass fires likely to result from nuclear weapons targeted according to Soviet and U.S. strategic policies.

A comprehensive Russian civil defense manual (*1*) describes the burning of cities subjected to nuclear attack in detail that has been repeated for years in Soviet official publications:

> The zone of complete destruction is characterized by an overpressure exceeding 0.5 kg/cm^2 [~7 psi] in the blast wave front. In this zone, residential and industrial buildings are completely destroyed; fallout shelters and some of the blast shelters located near ground zero are also destroyed. . . . The streets are completely clogged due to the destruction of buildings. . . . Fires do not occur in zones of complete destruction; flames due to thermal radiation are prevented, because rubble is

scattered and covers the burning structures. As a result the rubble only smolders, and fires as such do not occur.

Implementation of the basic Soviet nuclear war strategy (2) would result in those areas of cities that contain the greater part of the flammable materials being reduced mostly to rubble that would smolder for days. Such smoldering rubble would inject smoke and gases into the atmosphere much less forcibly and to much lower altitudes than if the same material burned as parts of standing structures; and it is likely to produce less black, sooty smoke.

Soviet strategy does not include the targeting of cities to kill civilians (2). But since long runways and ports can be used for the refueling and rearming of returning bombers and submarines, and because important factories, administrative centers, and communication and transportation facilities are concentrated in city areas, Western scientists should give priority to developing realistic attack scenarios based on Soviet concepts of modern war and on the types of fires and the extent of fires that would be ignited by a Soviet nuclear attack. Furthermore, all concerned should take account of the fact that the Russian nuclear arsenal has more than twice the deliverable megatonnage of the American arsenal and contains many more large warheads; a single 1-megaton (MT) explosion at optimum burst height would reduce about 93.3 square kilometers [about 36 square miles (3)] to mostly smoldering rubble.

Top national leadership also is not likely to be motivated to take "nuclear winter" possibilities seriously if provided only with estimates such as those of TTAPS regarding nonurban "nuclear" wildfires. For example, note 55 of the TTAPS article states that a 1-MT explosion used against a nonurban target will ignite "fires over an area [of dry forest, brush, or grassland] of 500 km^2/MT—approximately the zone irradiated by 10 cal/cm^2. . . ." In fact, more than 90 percent of the weapons used to attack nonurban targets (primarily missile silos and command modules) would be detonated as extremely low airbursts or surface bursts. A 1-MT extremely low airburst or surface burst would irradiate an area of about 225 km^2 by 10 calories per square centimeter (cal/cm^2) or more—less than half the 500 km^2 stated by TTAPS. This can be calculated easily by using equation (7.96.4), table 7.101, and figure 7.98 in (3). Furthermore, in calculating the nonurban areas burned by a 4000-MT nonurban yield, TTAPS does not take into account the fact that both Soviet and U.S. policy is to target missile silos and other hard-point targets each with *at least* two warheads. Because present missile accuracies are remarkably good, pairs of 10 cal/cm^2 ignition areas would be essentially superimposed. When allowance is made for this overlap, one concludes that the TTAPS estimates of such ignition areas are high by an additional factor of at least 3.

TTAPS make a far worse overestimate of areas burned by nonurban wildfires by tacitly assuming that each 1-MT ignition area is separate from and does not overlap another ignition area. But by using unclassified maps of the six U.S. Minuteman missile fields (which contain a total of 950 missile silos and 95 missile control facilities, the destruction of each of which necessitates the use of an accurate surface burst or an extremely low airburst), we find that the total possible ignition area is about 110,000 km^2. With at least two warheads on each silo or other hard-point target, these 110,000 km^2 of Minuteman missile fields would be hit by at least 2090 warheads. Thus, if the TTAPS assumptions are corrected to take account of real world geography and if 2090 1-MT warheads are detonated as surface bursts or extremely low airbursts, then about 52 km^2/MT—not the TTAPS 500 km^2/MT—could possibly be ignited in and around the U.S. Minuteman missile fields, under the other TTAPS assumptions. These missile fields contain about 90 percent of the nonurban counterforce targets in the United States.

Regarding the calculation of the ignitable fuel available (grams per square centimeter) in nonurban areas where important military targets are located, TTAPS in their note 55 state:

> Because ≈50 percent of the land areas of the countries likely to be involved in a nuclear exchange are covered by forest and brush, which are flammable about 50 percent of the time, the 1000-MT ignition yield [out of the 4000 MT that in the TTAPS Baseline Exchange is not assigned to urban or industrial targets] follows statistically.

But the great majority of strategic military targets, mainly deployed missiles and bomber bases, are actually in northern areas of scrub grasslands, in farmlands where vegetation is flammable much less than 50 percent of the time, and in desert areas where there is very little to burn. Also, in consideration of the fact that more than two-thirds of the megatonnage detonated in this TTAPS scenario would be Soviet, and that typical Soviet warheads to be used against hard-point targets are several times as large as their typical American counterparts, the location of some Soviet strategic missiles in forested areas does not invalidate the conclusion that the TTAPS estimate of ignitable fuel capable of sustaining "nuclear" wildfires in the pertinent nonurban areas is far too high.

Thus it appears that the TTAPS estimate of the amount of smoke likely to be produced by nonurban wildfires ignited by the explosions is high by a factor of at least 10.

The TTAPS article does not mention by far the most extensive wildfires that would result from their 5000-MT baseline exchange—the huge forest fires that beginning several months to years after the exchange would roar through conifer forests killed by fallout radiation. Work by A. H. Sparrow (4) shows that the mean lethal exposure to kill the conifer species tested was 826 roentgens. Conifers are much more vulnerable to radiation than are deciduous trees. Lightning and survivors would set fire to these dead forests at different times during droughts in different regions.

If the dominant American leaders come to believe that nuclear war surely will result in catastrophic "nuclear winter," but the top Soviet leaders do not, the impact on stability and on the hope of continuing peace will be disastrous. Albert Wohlstetter (5) states: "If your adversary understands that you believe a nuclear reply would be suicidal, he may count on your being unwilling to reply, even if you say you will."

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5. A. Wohlstetter, *Commentary* 76, 16 (December 1983).

Singer's comments on our "nuclear winter" article (hereafter TTAPS) have already been extensively aired and answered in other forums (*1*, *2*). Many of his points are either inadequately formulated or simply mistaken. For example, he incorrectly infers that the surface energy budget in a nuclear winter would be delicately balanced. In fact, there would be a large decrease in the downward solar flux, which could not readily be compensated by other energy sources; this effect has been illuminated by detailed energy balance calculations in our article and elsewhere (*3*). Moreover, no plausible greenhouse warming is likely to alter the sign of the temperature change in this case (*3*). Singer's proposal that smoldering debris might provide enough heat to warm the earth in a nuclear winter is easily dismissed (*2*), unless he is assuming that most of the planet might be set afire. And what about the smoke? His fuel combustion rate of 1 ounce per square foot per day over a (TTAPS) fire area of 500,000 square kilometers would generate an additional 100 million metric tons of acrid smoke per week (corresponding to the burning of about 0.2 gram per cubic centimeter per week).

Singer's comments on stratospheric dust reflect a common misinterpretation of our results. The severity and duration of nuclear winter effects are related primarily to the tropospheric soot burden, not the stratospheric dust burden (*4*). Our calculations, and others, further suggest that the upper troposphere might develop a stable stratification—like the stratosphere—due to solar heating of the uppermost soot layers (*5*). Such a response would tend to increase the soot particle lifetimes, not decrease them, as Singer speculates. Recent studies of the heights of smoke plumes and the optical-infrared properties of a smoky atmosphere are consistent with the predictions of severe nuclear winter effects (*6-8*). Moreover, large-scale smoke plumes are observed to be stable over great distances (*8, 9*) and tend to dissipate water clouds, not stimulate their formation (*10*).

Singer's closing comments are obscure and unrelated to our published work. Scientists are not ignoring scenarios that produce less severe global outcomes; indeed, prior to the studies by Crutzen and Birks (*11*) and TTAPS, all types of scenarios had been examined without recognition of the disastrous contingency of a global nuclear winter.

Kearny, in criticizing our article, relies heavily on two Soviet documents: one on civil defense that was published more than a decade ago; the other, a public statement on Soviet nuclear strategy of doubtful applicability. Many Western strategists believe that the Soviets would avoid purposeful saturation nuclear bombing of cities (*12*). The Soviet preoccupation with defense against high overpressures apparently stems from their fear of "countervalue" (city) attacks, a policy that has been publicly repudiated by the Western Allies in recent years. Thus, Kearny's basic hypothesis that cities would be completely and systematically reduced to rubble is not in concert with the professed nuclear strategy of either superpower. It is much more likely that the cities would be damaged and burned in attacks against nearby military and industrial targets (*12*).

This was a fundamental assumption in the TTAPS baseline case, although direct targeting of cities was also considered. Moreover, in the baseline calculation, only about 10 percent of the total smoke emission originated from city centers where the buildup of rubble might be significant. [The severe restrictions on city-center smoke emissions in the baseline case were later relaxed in the 100-megaton (MT) city attack scenario, which also produced a nuclear winter.]

Kearny's contention that bombed cities would not burn, but only smolder for days, is unfounded. A careful reading of the Soviet literature reveals a continuing concern with mass fires in all types of cities, although less concern with *firestorms* in areas of ferro-concrete construction (*13*). After the atomic bombings at Hiroshima and Nagasaki, the rubble burned vigorously (*14*). Even the Federal Emergency Management Agency—optimistic by most standards in its forecasts of nuclear damage—predicted, in the Five Cities modeling study, that two-thirds of all the buildings in Detroit would burn within 24 hours after a single 5-MT detonation over the city (*15*). Most recent analyses of fire damage in cities recognize high burning efficiencies in areas devastated by nuclear explosions (*16*). Only fuels covered by deep compacted rubble might not burn, but even this buried material is likely to pyrolyze and the vapors burn. Moreover, the total area subject to fire ignition by a 1-MT airburst is at least four to five times the area that is heavily damaged at 10 pounds per square inch overpressures or greater (*17*).

The TTAPS calculations for wildfires are also questioned by Kearny, although his alternative treatment is inadequate. For example, he incorrectly states that 90 percent of all the bursts in a nuclear exchange would occur on or very near the surface (note that such a strategy would greatly increase the dust lofting in many of the TTAPS scenarios). Kearny ignores barrages against air bases, mobile missiles, radar installations, and strategic industries, all of which would involve predominantly airbursts. Similarly, tactical nuclear explosions, which are particularly efficient at igniting fires (*18*), would occur mainly in the air. Kearny also does not acknowledge that multiple bursts over forests and other wildlands could increase both the probability of ignition and the fraction of fuels consumed and could project the smoke to much higher altitudes than would otherwise be the case. Finally, it is not realistic to assume that all missiles would be precisely on target and, hence, that ignition zones would be essentially superimposed.

Table 1 provides a more thorough estimate of the potential area of forest fires in a "counterforce" nuclear exchange (*19*). The case described employs less than one-third of the world's strategic, theater, and tactical nuclear arsenals. The estimated forest fire area is one-half that of the TTAPS baseline case, but ignores fire spread and neglects other fuels (such as brush, grass, and structures) in the ignition zones. The smaller burn-off area would be partially made up by the additional fuel consumed in regions subject to intense multiple blast and thermal pulse environments, which can shatter, dry, and burn vegetation effectively (*20*). Our projections of nuclear wildfire areas (TTAPS and Table 1) are actually much smaller than many of those forwarded by the U.S. Forest Service, which range up to several million square kilometers (*21*).

Focusing, as Kearny has, on the missile silo complexes of the United States and the Soviet Union, we estimate that, in the summer half-year, these hold about 1200 tg [1 teragram (tg) = 10^{12} grams] of biomass, consisting of trees (1000 tg) and brush, grass, grain crops, and organic debris (200 tg). The bulk weights follow from the areas in Table 1 and average accessible biomass burdens of 2 grams per square centimeter in forests and 0.1 gram per square centimeter elsewhere (*22*). The missile silos are concentrated in relatively small areas; systematic attacks against them could expend up to 2500 MT (or more). The average energy release over the silo complexes would be ~1000 calories per square centimeter; the sudden deposition of ~5 to 10 calories per square centimeter is sufficient to ignite a variety of wildland fuels (*17*). Exposure to extreme nuclear conditions could incinerate 35 percent of the forest biomass and 100 percent of the brush, grass, and crop biomass, converting 4 percent to smoke (*23*) and releasing up to 22 tg of smoke from this source alone (the total wildfire

Table 1. Wildland ignition areas in a nuclear exchange.

Targets or exchange elements	Megatons detonated	Total area affected (km^2)*	Area forested (%)†	Area subject to multiburst (%)	Forest burn-off area (km^2)‡
U.S. missile silos (1000)	1,500	100,000	10	100	10,000
U.S.S.R. missile silos (1400)	1,000	150,000	27	100	40,000
Strategic military bases (100)	200	50,000	40	100	20,000
Other military bases (200)	400	100,000	40	100	40,000
Bomber barrage	400	200,000	40	0	40,000
Theater-tactical	500	500,000	40	0	100,000
Total	4,000	1,100,000	35§	36‖	250,000

*For silos, the areas of the fields; for military bases, 2 MT and 500 square kilometers per base; for bomber barrages, 500 square kilometers per 1-MT airburst; for tactical warheads, 1 square kilometer per kiloton. †Estimates were obtained by comparing target locations with geographical surveys of vegetation types and distributions (*27*). About 80 percent of the U.S. military bases (other than missile silo fields) are in partially forested regions. The average areal forest coverage within 15 kilometers of each such base is taken to be 50 percent to account for local land use. Nonurban barrage attacks and theater nuclear detonations are assumed to occur over or near forests 40 percent of the time. ‡In multiburst nuclear environments, the forest ignition probability is taken to be 100 percent, otherwise 50 percent, for summer conditions (*27*). §Average. ‖Average.

smoke emission from *all* of the targeting in the TTAPS baseline scenario was about 80 tg, while the counterforce exchange in Table 1 implies a total wildfire emission of up to 73 tg). In a typical silo field of ~15,000 square kilometers, attacked over a period of 1 hour or less, the average energy release rate, including the heat of combustion, could exceed 10^8 megawatts—potentially enough power to inject the nuclear-generated smoke and dust into the upper troposphere (*24*).

Nevertheless, even if wildfire smoke emissions were completely disregarded in the TTAPS calculations—an unrealistic assumption—the predicted climatic effects would not be significantly altered. The sensitivity studies reported in our article demonstrate that the smoke from urban-industrial fires is sufficient to cause a nuclear winter. On the other hand, it now seems possible that major climatic anomalies could be triggered by a pure counterforce nuclear attack limited *only* to the missile silo fields because of the amounts and heights of the combined smoke and dust injections. This critical point should be refined through more detailed assays of wildland fuels and analyses of multiburst nuclear effects on soils and vegetation.

Kearny recognizes one effect that makes the outcome of a nuclear war more severe, namely, the eventual burning of forests killed by prompt radioactive fallout (*25*). Indeed, the eventual fate of the world's forests could be much worse. Frosts and hard freezes in subtropical and tropical regions would destroy plant life there indiscriminately. Even hardy northern temperate and boreal forests could be extensively damaged by sudden freezing during the summer season. Human survivors, seeking energy and materials to replace oil, gas, concrete, and steel, would intensively harvest the forests. With chemical-dependent food production at northern midlatitudes all but eliminated, slash and burn agricultural practices in other parts of the world—practices already of concern to ecologists—would accelerate. Forests weakened by widespread fallout, air pollution, climatic stress, and ultraviolet radiation would fall victim to pathogens and insect pests. All of these factors imply unprecedented rates of deforestation and biomass burning in the years immediately following a nuclear war. Such events could in turn stimulate continuing climatic disturbances.

Kearny mentions the political and strategic implications of the TTAPS findings and speculates unconvincingly on their significance. A more thorough and quite different discussion of the implications of nuclear winter for policy and doctrine has been published by Sagan (*26*). The seriousness attached to the nuclear winter problem by officials in the United States and the Soviet Union should be judged by their recent actions. In the United States there has been continuous discussion of nuclear winter in the military, policy, and intelligence communities, as well as substantial press and television coverage. Scientific briefings have been given to the director of the Arms Control and Disarmament Agency, Kenneth Adelman, and to other officials of comparable rank. Several million dollars have already been allocated for nuclear winter research, and an Interagency Working Group on Nuclear Winter has been organized—at the urging of George Keyworth, White House director of the Office of Science and Technology Policy—under the aegis of the National Oceanic and Atmospheric Administration, to plan a $50-million, 5-year national research program. In the Soviet Union both American and Soviet research results have been widely communicated by the Soviet electronic and print media. At a meeting in the Vatican in early 1984, one of us (C.S.) was informed by Y. P. Velikhov, vice president of the Soviet Academy of Sciences, that he had given detailed briefings on nuclear winter to Soviet Foreign Minister Gromyko and then to Defense Minister Ustinov and that the Soviet Academy of Sciences has committed several million rubles for research in this area over the next 2 years. Many other nations have recently exhibited signs of growing interest in the nuclear winter problem.

Even if the TTAPS baseline estimates of smoke and dust emissions were high by a factor of 3 to 6, the implied climatological effects could still be very severe. Indeed, the probability of a nuclear winter could be significantly lower than is indicated by the available data, and the risk of a nuclear winter would still be unacceptable because the costs are so high (*25*, *26*). We reemphasize, however, that, on the basis of current knowledge, nuclear winter would seem to be a likely outcome of nuclear war.

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