of the outcome $\tau_i < \tau_j$ is the same on each repetition," is not in general met except on a local scale. This is apparent in cases, where lithostratigraphic units are time-transgressive or biologic components migrate (both cases being well known). In such cases the τ_{iK} or τ_{jK} will clearly be functions of a locality index K, and in general $P_{ijK} = Pr(\tau_i < \tau_j | K)$ will vary with K. The authors dismiss this problem on the basis that condition (iii) "is well approximated on a regional basis." That, of course, depends on how one defines a region and what one considers a good approximation.

The situations with conditions (ii) and (iii) will be apparent to most experienced stratigraphers because of all too familiar aspects of the facies problem. Condition (i) is more deceptive because of a common misunderstanding of the implications of stochastic independence. It must be defined here by what appears to be a roundabout approach. Let the relationship between two events i and j be represented by a random variable r such that if $\tau_i < \tau_j$ at locality K, then r_K will be defined to be 1, and if $\tau_i > \tau_j$ at locality K, then r_K will be defined to be 0 (*2*). It is clear that if any one observation involving i and j be considered alone, the expected value of r, denoted $E(r_K)$, is just the P_{ij} of the authors at locality K. Let us now denote two arbitrarily chosen observations by $K = 1$ and $K = 2$. From the definition of stochastic independence, condition (i) is met if and only if $E(r_2 | r_1) = E(r_2)$—read as "the expected value of r_2, given a value of r_1, is the same as the unconditional expectation of r_2." If this is true, then it will not matter in the least where the observations are made. Any experienced stratigrapher will agree that the same ordering is more likely if two observations are made in one highway cut than if they are made a thousand miles apart—this very point is apparent in the introductory comments of Southam *et al.* This being true, then by definition the observations are not stochastically independent. We should say in this case that propinquity induces a correlation between r_1 and r_2. Indeed, it is just when we have observations sufficiently removed that the correlation becomes negligible for most i, j pairs that we say we have crossed a facies boundary.

In summary, there is much to be said for the formulation proposed by Southam *et al.* The matrix elements are, however, conditional on geographic or facies conditions of the observations, and the confidence intervals that they calculate are appropriately conditional as well. Recognition of this reality necessitates some modification of their concept, but also opens the way for the application of some very powerful multivariate techniques for studying the interrelationships between the random variables that were used here.

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References and Notes

1. J. R. Southam, W. W. Hay, T. R. Worsley, *Science* **188**, 357 (1975).
2. The random variable r is related to two of the authors' variables. The x in equation 2 of the original report is

$$x = \sum_{K=1}^{N} r_{ijK}$$

and the matrix element a_{ij} is

$$a_{ij} = \frac{n_{ij}}{N_{ij}} = \frac{x}{N} = r_{ij}$$

the average of the observed r's. Although it was not necessary in the authors' presentation or here, the r's or x's should bear appropriate subscripts.

30 April 1975

Strategic Arms Debate

If *Science* is about to enter the strategic arms debate, it should do so more carefully: the article by Tsipis (*1*) contains numerous errors, both conceptual and mathematical.

The discussion of nuclear effects on humans is concerned almost exclusively with 1-megaton weapons and hence addresses only thermal effects. In fact, because of the different variations of effects with distance, radiation predominates as a prompt kill mechanism at very low yields and prompt thermal effects are important only at very large yields. Overpressure may be the dominant prompt mechanism at intermediate yields, depending upon the level of protection utilized (*2*). The statement that "by far the most lethal effect of a nuclear weapon is the thermal radiation it releases" is an overgeneralization, even when applied to urban areas where the prompt thermal radiation is augmented by fires and even firestorms. Moreover, a countervalue attack against an industrial area is not identical with a counterpopulation attack and is not achieved "more efficiently, by scattering several small weapons even at random over the area." Only the killing of people can be done with reasonable efficiency this way.

A specialized definition is also used for counterforce. Counterforce implies the destruction not only of missiles inside reinforced concrete silos, as Tsipis defines it, but of missiles in silos regardless of their construction, missiles on soft pads, and missiles in warehouses, as well as of any other military force—bombers, submarines and ships in port, weapons in storage, and army units. However, since the article is concerned only with the specialized case, only that is considered below.

First, however, two points should be made about pindown and interference. The "carefully timed arrival and detonation of reentry vehicles overhead" is not a practical way to facilitate the use of bombers against silos, since the number of reentry vehicles required to do this, while waiting several hours for bomber arrival, is astronomical. If interference does negate the efficacy of all but the first reentry vehicle reaching the silo, then only the most lethal reentry vehicles should be counted in considering the countersilo potential of a force.

As for the "calculus of destruction," it is unnecessary to attempt to precisely fit a curve to the general overpressure-distance (Δp-r) relationship, since the departure from a power of Δp is of significance only at values of Δp lower than those of interest here. Moreover, equation 1 in (*1*) does not include height-of-burst effects and hence is inappropriate at low values of Δp. At high values it is approximated by its leading term, and equation 7 can be replaced by

$$r = \frac{Y^{1/3}}{0.408(\Delta p)^{1/3}}$$

where Y is weapon yield in megatons, r is distance in nautical miles, and Δp is overpressure in pounds per square inch. [Better fits, valid at high overpressures, can be achieved with other values of the constant, such as 0.43, or better still with other values of both the constant and power of Δp, such as $0.25\,(\Delta p)^{0.425}$.]

The hardness (H) is described by a simple "cookie cutter" model such that the silo is expected to survive if $\Delta p \leq H$, but be destroyed if $\Delta p > H$.

Equation 8 in (*1*), relating the probability (P_s) that a silo will survive detonation of a reentry vehicle to the hardness of the silo and the accuracy of the reentry vehicle, is wrong. The correct expression is

$$P_s = 0.5^{(r_o/CEP)^2}$$

where r_o is the distance at which $\Delta p = H$ and CEP is the circular error probable of the reentry vehicle; the free variable r_s is irrelevant. Note that it is 0.5, not e, which is raised to the power. An alternative exponential formulation which parallels equation 8 in (*1*) is

$$P_s = e^{(\ln 0.5)\,(r_o/CEP)^2}$$
$$= \exp\,[-0.693\,(r_o/CEP)^2]$$

Equations 9, 11, 12b, 14, 15, 20, and 21 perpetuate the error. A correct form of equation 14, which utilizes the simplified expression for r, is

$$P_k(n) = 1 - \exp\left[\frac{-0.693\, Y^{2/3} n}{0.166\, H^{2/3} (CEP)^2}\right] = 1 - \exp\left[\frac{-4.17\, Kn}{H^{2/3}}\right]$$

where $P_k(n)$ is the probability of destroying a silo with n warheads and K is the lethality of a reentry vehicle to a silo [equation 13 in (*1*)].

In the paragraphs which follow equation 21 throw weight and absolute numbers of missiles are dismissed as "not directly related" to the efficacy of a nuclear arsenal. Only a narrow construction of the modifier "directly" admits of such an interpretation. But such an interpretation would also lead to dismissing yield and accuracy as not being directly related; these only enter indirectly through the quantity K, yet K is described as one of the most sensitive performance characteristics. Another characteristic considered most sensitive is the number of reentry vehicles (n) per missile, even though absolute numbers of missiles are described as not directly related.

In fact, because there is a minimum weight which a reentry vehicle must have in order to contain a critical mass of fissionable material, and because for large reentry vehicles yield is essentially directly proportional to vehicle weight, the ratio of $Y^{2/3}$ to weight (that is, the so-called equivalent yield to weight ratio) is small for both very small and very large reentry vehicles and is a maximum at intermediate yields. For real reentry vehicles and warheads, this maximum is very broad and is almost constant for yields from a fraction of a megaton to several megatons. Thus, over the range of reentry vehicle sizes of interest, payload is a direct measure of equivalent yield, and for a given accuracy it is a direct measure of KN, which Tsipis describes as a direct measure of ability to destroy the opponent's missiles in their silos. Consequently, instead of being indirect relevance, total throw weight is of critical relevance.

The foregoing properly describes the relationship of K to throw weight. A larger warhead will have a greater value of K; miniaturization of components merely affects the proportionality factor. Thus, the statement that "the size of the missile becomes largely irrelevant," in its context, is incorrect. The size of individual missiles is, in fact, largely irrelevant (*3*), but only because total force payload is the key measure. Thus, comparisons of counterforce capabilities on the basis of throw weight are far from "simplistic and irrelevant," but in fact are most fundamental, as long as accuracy is properly included. The issue then becomes one of what accuracy is to be used in determining the proportionality factor, and for what era that accuracy obtains.

As for the conclusions, I do not believe that there exists reliable open literature (*4*) on Soviet missile performance, but I do know that many of the numbers given for U.S. systems are incorrect. More plausible numbers for current systems are cited by Congressman R. Leggett (D–Calif.) (*5*). Apart from this, and from the undesirability of comparing 1975 U.S. figures with 1974 Soviet figures, as in table 1 in (*1*), the conclusions are confused by the apparently floating definition of K. It can variously be inferred to be the K of a reentry vehicle, the value of K needed by a single reentry vehicle to achieve some probability of kill, and the total value of K needed by a series of reentry vehicles to achieve, cumulatively, some probability of kill. Subscripts would alleviate this problem. In addition, the expected value is wrongly used interchangeably with the confidence level. A probability of kill of 97 (or 40) percent for missiles in silos does not mean that "the probability of destroying them all" is 97 (or 40) percent. It means that the expected fraction of silos destroyed is 97 (or 40) percent.

The conclusions are also clouded by flat statements about silo hardness for which no justification is offered, and for most of which I believe that none can be. This being so, it is almost gratuitous to note that if killing 97 percent of Soviet silos requires a total lethality of 40,000, and if the United States has available a total lethality of 22,000, then the P_k for Soviet silos is 85 percent, not the 40 percent stated, although the probability of "destroying them all in their silos" is negligibly small.

Finally, because K is a strong function of accuracy, small misestimates of CEP severely distort the conclusions. Further, the way the lethality needed to destroy S silos (KS) enters exponentially into the expression for P_k, makes conclusions very sensitive to the value of P_k chosen; the value of KS is needed for a 90 percent kill probability is only two-thirds of that needed for a 97 percent kill probability.

The quantity K is used in an effort to include in a single parameter the essential measures of force effectiveness, but it is based on peak overpressure alone. Thus it is inadequate at low yields, because damage depends on total impulse rather than simply on peak overpressure (this fact can be perceived by considering the effectiveness of the high instantaneous pressures achieved in shock tubes in destroying massive structures). As a result of this, the K factors for sub-megaton warheads are diluted as follows (*6*), in the hardness range 300 to 1000 psi: for yields (in megatons) of 1.00, 0.20, 0.16, and 0.05, the respective dilution factors would be 1.0, 0.88, 0.84, and 0.73.

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References and Notes

1. K. Tsipis, *Science* **187**, 393 (1975).
2. An adequate discussion is available in S. Glasstone, Ed., *The Effects of Nuclear Weapons* (Government Printing Office, Washington, D.C., 1962).
3. Size of individual missiles does matter in the context of survivability and hence stability. A total force payload divided into many small missiles is more stable in a crisis than a force with the same payload divided into few large missiles.
4. One reviewer's comment implied that someone with access to classified literature could select correctly from the great variety of open estimates. In my view, such informed selection classifies the open estimate. I do not wish to comment on the adequacy of classified estimates.
5. R. Leggett, *Armed Forces J. Int.* **112** (No. 6), 30 (February 1975).
6. D. C. Kephart, "Damage probability computer for point targets with P and Q vulnerability numbers" (Report R-1380-PR, Rand Corporation, Santa Monica, Calif., 1974).

10 March 1975

The conclusion of Tsipis (*1*) that neither the United States nor the Soviet Union currently has the ability to destroy the other's strategic missile silos may be mildly comforting until one examines potential dynamics of the problem not touched on in the article. Leaving unchanged all other data on missile numbers, yields, silo strengths, and so forth, suppose one assumes it possible that in some years from the time represented by Tsipis' data the Soviet Union could achieve, with all its warheads, the weighted average of the CEP's (CEP = circular error probable) attributed to the U.S. missiles: 0.29 nautical miles. Then the total "countersilo kill capacity" (KN) for the Soviet forces in Tsipis' table 1 would become 54,065, which is virtually the same as the KS value of 54,170 given in his table 3 as the requirement to destroy all current U.S. silos with probability $P_k = .90$. With this accuracy, the lethality (K value) of a single Soviet-SS-9 warhead becomes 81.58, compared with the quoted figure of 45 needed to destroy a 300-psi silo with $P_k = .97$, so that from table 4 the 288 SS-9's theoretically could then destroy at least 288 of any of the silos with $P_k \geq .90$, rather than only 45 300-psi silos. Of course, the improved accuracy may be achieved in new missiles, such as the four or five the Soviets are now developing, rather than by improving the SS-9.

It would be possible to carry such speculations further, using only the numbers given in (*1*) plus the ceiling of 2400 missiles on each side including 1320 equipped with MIRV's that was agreed on in November 1974 at Vladivostok, to show that neither side can take much comfort from calculations comparing current or past situations and capabilities only. If either side can improve its force to match the best values of critical parameters shown by the other (yield, *CEP*, reentry vehicles per missile, and silo strength), then either side potentially might develop a counterforce capability against the other's fixed launchers. Of course, it can be argued that such a capability against fixed launchers only is not a credible counterforce capability, since the movable or mobile launchers would remain a formidable countervalue force.

It is problems such as these that have made the public arguments about the "calculus" of the arms race appear so slippery and that make formulations such as Tsipis' appear too simple to provide useful guidance. The issues are associated with the credibility of various strategic options and with the physical characteristics and performance of weapons in being and in development; the analytical formulation itself does not offer much latitude for debate.

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1. K. Tsipis, *Science* **187**, 393 (1975).

19 February 1975

Walsh states that Congressman Leggett's figures on the accuracy of U.S. missiles are more plausible than those I cited (*1*). However, the two sets of figures are identical. He states that the pindown effect that would permit the use of bombers would require an astronomical number of reentry vehicles. In the article I emphasize the importance of the electromagnetic pulse (EMP) as an effect that could temporarily disable a missile in an otherwise intact silo and therefore make it a putative bomber target. A crippling EMP can be generated with a single large explosion beyond the atmosphere over a missile farm (*2*). That would not be astronomical; merely exoatmospheric.

In one of several examples of the use of unpublished information, Walsh offers the expression

$$r = \frac{Y^{1/3}}{0.408\,(\Delta p)^{1/3}}$$

instead of equation 1 in (*1*). This is an empirical formula which is only a one-term approximation of the full expression in my equation. Furthermore, Walsh's formula had not appeared in public at the time of the writing of (*1*), and I considered it sound scientific and editorial practice to begin with a formula that had already seen publication (*2, 3*).

The statement that equation 8 in (*1*) is incorrect is simply false. First, this equation is what Walsh refers to as an alternative expression. Secondly, as the following derivation shows, equation 8 gives results that are at most a few percent different from (more conservative than) those obtained by Walsh's formula. For a normal distribution in r, quite generally,

$$P(r) = Ce^{-r^2/2\sigma^2}$$

Therefore

$$1 = C\int_0^{2\pi} d\theta \int_0^{\infty} r dr e^{-r^2/2\sigma^2}$$

or

$$1 = 2\pi C\int_0^{\infty} \frac{dx}{2}\, e^{-x/2\sigma^2}$$

or $1 = 2\pi\sigma^2 C$, which determines C, so

$$P(r) = \frac{1}{2\pi\sigma^2}\, e^{-r^2/2\sigma^2}$$

Now, for a lethality radius r_s

$$P_k = \frac{1}{2\pi\sigma^2} \int_0^{r_s} r dr \int_0^{2\pi} d\theta\, e^{-r^2/2\sigma^2}$$

$$= \frac{1}{2\sigma^2} \int_0^{r_s^2} dx\, e^{-x/2\sigma^2}$$

$$= e^{-x/2\sigma^2}\Big|_0^{r_s^2} = 1 - e^{-r_s^2/2\sigma^2}$$

Now to introduce *CEP* one must modify this since, because of the definition of *CEP*,

$$0.5 = \int_0^{CEP} r dr \int_0^{2\pi} d\theta\ \frac{e^{-r^2/2\sigma^2}}{2\pi\sigma^2}$$

$$0.5 = 1 - e^{-(CEP)^2/2\sigma^2}$$

and

$$\ln 0.5 = -(CEP)^2/2\sigma^2$$

therefore

$$2\sigma^2 = \frac{-(CEP)^2}{(\ln 0.5)} = 1.44\,(CEP)^2$$

In equation 14 in (*1*) $2\sigma^2$ is replaced by $2(CEP)^2$, which varies numerically by less than 5 percent from Walsh's formula for relevant values of K and H. So many simplifying assumptions are implicit in both our formulas that the uncertainty in P_k is much larger than 5 percent (*4*); therefore, I consider my approximation a valid one and certainly not an error.

Walsh states that "payload is a direct measure of equivalent yield." This can hardly be the case since the actual weight of the nuclear charge, which is proportional to the yield of the weapon, is a fraction of the payload carried by a missile. For "MIRVed" missiles such as Minuteman and Poseidon, the payload consists, in addition to the nuclear charge, of the arming and fusing mechanism of the warhead, the thermal shield of the reentry vehicle, the inertial platform that guides the individual warheads, the "bus" that carries them, the motors that change the velocity of the bus before each reentry vehicle ejection, the fuel for these motors, the ejecting mechanisms for the reentry vehicles, and the computer that controls the entire system. Thus, depending on the technological capabilities of a nation, the nuclear charge may be either a large fraction of the total payload or a small one. The Soviet reentry vehicles, for example, are reputed to have solid rather than ablative heat shields to protect them during reentry. If this is true, these heat shields must be massive, and it must take a considerable portion of the missile's throw weight capacity to lift them into trajectory. Therefore throw weight, or equivalent payload, is not a direct measure of the yield of the warhead carried by the missile.

As to the use of the lethality parameter K, I believe it is a convenient parameter to characterize warheads with. It displays asymptotic behavior for high values of P_k, but so do other measures of warhead countersilo effectiveness, such as yield and accuracy, since this is a characteristic of the exponential nature of the relationship and not of K.

Finally, although I too have Kephart's calculations (Walsh's reference 6), I could not calculate the dilution factors for K because the PVN and QVN codes that permit use of Kephart's tables are classified and therefore unavailable to me. Even so, these dilution factors may be useful in attempting to arrive at a cardinal value for P_k for a given silo, but for the type of ordinal indication of kill probabilities intended in my analysis I would consider use of them misleading, since making a predictive calculation to an accuracy of several significant figures does not always mean that you can actually measure or know the respective physical quantity to such a high degree of precision.

Deitchman is correct in his calculation that, if left unimpeded, the Soviet Union, trailing only a few years behind the United States, will acquire a credible countersilo capability against Minuteman silos. It is precisely in order to facilitate such calculations that I offered the analytical formulation. Such well-founded concern about the future strategic capabilities of several nations makes an informed public debate on the direction U.S. defense efforts should take essential and timely.

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1. K. Tsipis, *Science* **187**, 393 (1975).
2. *Air Force Manual for Design and Analysis of Hardened Structures* (Air Force Weapons Laboratory, Kirtland Air Force Base, N.M., 1974).
3. H. Brode, *Annu. Rev. Nucl. Sci.* **18**, 153 (1968).
4. *Mathematical Background and Programming Aids for the Physical Vulnerability System for Nuclear Weapons* (Defense Intelligence Agency, Washington, D.C. 1974), p. 35.

29 September 1975