

Deterrence in Everyday Life:
Some Notes on Directions for Research

Let's begin with a few quotes:

If, in the blunting-mission game, one side can make a surprise attack upon the other that destroys the latter's capability to make meaningful retaliation, then it makes sense to be trigger-happy with one's strategic air power. . . . This would be the situation of a gunfighter duel, Western frontier style. The one who leads on the draw and aims accurately achieves a good clean win. The other is dead. But if, on the other hand, the situation is such that neither side can hope to eliminate the retaliatory power of the other, the restraint that was suicidal in one situation becomes prudence, and it is trigger-happiness that is suicidal.

-Bernard Brodie, 1954¹

Suppose both the United States and the Soviet Union had the power to destroy each others' retaliatory forces and society, given the opportunity to administer the opening blow. The situation would then be something like the old-fashioned Western gun duel. It would be extraordinarily risky for one side not to attempt to destroy the other, or to delay doing so, since it not only can emerge unscathed by striking first but this is the sole way it can reasonably hope to emerge at all. Evidently such a situation is extremely unstable. On the other hand, if it is clear that the aggressor too will suffer catastrophic damage in the event of his aggression, he then has strong reason not to attack, even though he can administer great damage. A protected retaliatory capability has a stabilizing influence not only in deterring rational attack, but also in offering every inducement to both powers to reduce the chance of accidental war.

-Albert Wohlstetter, 1959²

The difference between a stable and an unstable balance is illustrated by another offensive weapon against which no good defense was ever devised. The "equalizer" of the Old West made it possible for either man to kill the other; it did not assure that both could kill each other. The advantage of first shot aggravated any incentive to shoot. As the survivor might put it afterwards, "He was about to kill me in self-defense, so I had to kill him in self-defense." Or, "he, thinking I was about to kill him in self-defense, was about to kill me in self-defense, so I had to kill him in self-defense." If everyone who was shot were assured of living long enough to shoot back, there would be no advantage in jumping the gun and little reason to fear that the other would try it.

-Thomas Schelling, 1959³

We all know that strategy in the nuclear age cannot be based on actual experience with nuclear war; strategic thought must therefore be hypothetical and largely abstract in nature. Metaphors and analogues to everyday life have consequently played a central role in our thinking about nuclear issues; they provide a handle on certain key problems, a way of making them intellectually

manageable. And no metaphor has had a more profound or pervasive effect than the one about two gunfighters in the Old West. Not only does it lie at the heart of the whole doctrine of strategic stability, but is frequently used even by the opponents of that theory. Thus General Daniel Graham (USAF ret.), head of High Frontier and a leading proponent of strategic defense, recently defended the Strategic Defense Initiative with the following argument:

Part of the argument [against SDI] . . . is that it would be de-stabilizing. But that assumes that we've got a stable situation today. And what we've got is an unstable situation that's getting more and more unstable as these offensive weapons become more and more accurate and deadly. We've got a situation that resembles the classic scene from a Western movie, you know. Two men are facing each other down--all offense--and each one looking for the other to make a move for that nuclear six-gun, and both of them knowing perfectly well that the man who gets in the first shot has got the best chance of survival. Now that's an unstable situation in my view. What would be a lot more stable, [would be] if these two guys were in foxholes, defending, glaring at each other.⁴

We are not concerned here, however, with the question of how metaphors structure thought, although this may be an important area in which cognitive psychology can throw light on political and strategic problems. Our point is different. It is simply that the metaphor itself provides an entree for further empirical study. One can actually study how much personal security there was in the Old West--or for that matter in any political circumscription (like the Walled City of Kowloon today⁵) where for whatever set of reasons the writ of civil authority does not run. If it turns out that people were not killing each other off at any great rate, or if the kind of violence that did exist fell into a clearly defined pattern, then this may have certain implications with regard to strategy as well. It may, for example, suggest certain questions, or lines along which our strategic thinking might move, that might otherwise not occur to us.

There is, of course, nothing new to the point that one can learn something about strategic problems by thinking about analogues from everyday life. The use of such analogues was one of the most distinctive features of Thomas Schelling's style of analysis. What Schelling ultimately wanted to do

was to reach for a "general theory of strategy, one that would unify the structures of concepts like deterrence in widely different fields, and that could benefit from the similarities and analogies among different areas of conflict."⁶ "Stripped to its essentials," he wrote, "the notion of deterrence" appeared in many different contexts--in criminal law, in parent-child relations, on the road and in the underworld. This led to the important conclusion that "there are a number of other areas available for study that may yield insight into the one that motivates us, namely the international area." Developing a general theory of this sort was "almost sure to provide new insight" into the problem that one had started out with--the problem of international conflict in the nuclear age.⁷

But in thinking along these lines, Schelling relied mainly on common sense and personal experience; his analogues were a device for bringing general problems about the logic of conflict into focus, but they were not meant to serve primarily as a bridge to empirical analysis. In the early 1960s he was interested in experimental games⁸, and other people also tried to use games as a surrogate for direct historical experience. The term "synthetic history" was in fact in vogue for a time among the game people at Rand. But natural experience was never systematically mined for insight into strategic problems. We think it can be, we think it should be, and we want to discuss how one should go about doing it.

There is a very nice article--actually it was the text of a talk he gave at the Rand Graduate Institute--where Charles Hitch talks about how he made his reputation. For many years, he said, when he met other economists for the first time, he was often asked if he was the Hitch of "Hall and Hitch."

"Hall and Hitch" was an article entitled "Price Theory and Business Behavior." Economists since the time of Adam Smith through and including E.H. Chamberlin and Joan Robinson had simply assumed that businessmen set prices which maximized "profits," or "present value," or equated marginal cost and marginal revenue, completely ignoring the fact that, however much they may have wanted to, they didn't know how. In the typical case, selling in oligopolistic markets, they don't know the location or shape of their marginal revenue curve, so what do they

do?

Hall and I had the simple but unbelievably original idea of asking them. As I recall, we asked forty or fifty and tabulated the answers, which fell into a pattern that seemed to us to be significant. To my amazement, this obvious and elementary exercise in empiricism was assigned to students in economics departments and schools of business administration, which apparently lacked anything better, for decades.⁹

We would like to do the same kind of thing with strategy that Hitch did with economics, but what exactly are the questions that we would like the real world to help us answer? What would be our equivalent, our counterparts, to Hitch's question about how businessmen actually operate?

We think research should focus on the question of how people deal with risk and uncertainty. In the strategic literature, this basic issue is hardly dealt with adequately. Claims in this area are as a rule impressionistic and sometimes even contradictory in nature. There is no need to dwell on this point; we limit ourselves to a couple of quotations drawn from Secretary of Defense James Schlesinger's testimony before a Congressional committee in 1974. In the first quotation, Schlesinger was talking about how the United States had an "in-between deterrent" in Europe "in the sense that we have war plans that contemplate the employment of tactical nuclear weapons" there. Senator Muskie then commented that he had "mixed feelings about the credibility" of those forces, and the Secretary of Defense replied as follows:

Secretary SCHLESINGER. Once again, you are thinking of circumstances in which they would be employed with high confidence. Looking at it from the Soviet perspective, any possibility of their employment, whether it is 3, 4, 5 percent, has a deterrent effect. We do not have to have a 100 percent confidence on our part that we would actually employ them. As long as our opponents perceive there is some likelihood, even a low likelihood of employment, that will have a restraining influence.¹⁰

But just a little later Schlesinger was to take the very opposite line. Senator Javits was talking about a report on NATO which in effect argued, with respect to the nuclear deterrent, both tactical and strategic, "that if you are not ready to use it, forget it and that you have got to take the position that if you have to, you are ready to use it. That is what it is for. That is why it inhibits a Soviet effort to overrun Europe." And now

Schlesinger agreed: "You have touched upon the heart of the question, that in order to deter you must have a threat that you are prepared to implement, and that your opponent must perceive that you are prepared to implement that threat."¹¹ A mere possibility that it might be used would evidently not do much good. And the official Defense Department commentary that Schlesinger had prepared at Javits's request stressed that "Any potential aggressor must see in the realities of our force structure, our planning and our resolve a high degree of certainty that they will elicit a response from us that they would wish to avoid."¹²

Is it possible, however, to be more rigorous, more precise and less impressionistic? How are firmer conclusions even possible? If analysis in this area is in the final analysis to be anything more than a projection of one's own basic assumptions and prejudices, it is vital to recognize that the fundamental problem derives from the need to strike a balance between conflicting claims. One might assume that "deterrence" is the overriding goal; in that case, the assumption may be implicit that if deterrence is good, then more deterrence is better, and we should therefore try to get as much of it as we can. This line of argument is incomplete because it does not take into account the fact that there may be a price--and not just an economic price--for increasing the level of deterrence, and that decisions about military policy should take this kind of trade-off into account. Our basic concern should therefore be with the terms of the trade-off: what if we have to pay a big price--for example, a big increase in the level of risk that certain circumstances would generate--to get only a small increase in the level of deterrence? Simply recognizing that is this a basic problem can be extremely useful, but can we hope for anything more than that? Is there anything concrete that we can say about this particular problem?

Not too long ago, one of us argued (in a paper on the no first use question¹³) that the principle of diminishing marginal utility threw some light

on this issue. Think of "deterrence" (or, more broadly, "political effect") as a function of the risk of escalation. Then one could take it for granted (for a given set of circumstances) that the greater the risk of escalation, the greater the political effect. But what about the curve that represents this function? The marginal effect of increasing the level of risk would presumably diminish: going from a zero to a ten percent probability of escalation would have a greater marginal deterrent effect than a shift from ten to twenty percent; an increase from twenty to thirty percent would have a still smaller net effect. The curve, in other words, flattens out.

One of the nice things about approaching the problem in this way is that it helps you get a handle on what these nuclear debates are about. For example, consider Figures (1) and (2):



Figure (1)

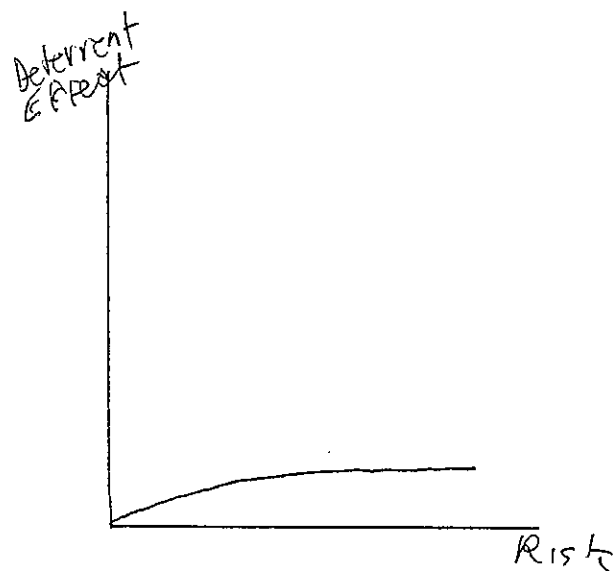


Figure (2)

Political positions can be associated with different kinds of curves. Figure (1) corresponds to the position that nuclear deterrence is relatively cheap and easy to achieve: a small risk can have a great deterrent effect, but increasing the level of risk has a very limited payoff. Figure (2) represents

the position held by those people who argue both that the risk of nuclear war is great and that nuclear weapons have a very limited political effect, that they are good for nothing more than deterring their use by others. The fact that adherents of these two positions sometimes co-author articles might obscure the point that these are radically different positions, but graphic representation is a good way of bringing these distinctions into focus.

Moreover, looking at the problem in this way helps clarify what the question is. If the argument is that nuclear deterrence is weak, is this an argument about the shape of the curve or about the particular point in the curve we happen to be on? This point leads the way to some nice arguments. For example, there is the common argument that the threat of first use is not credible and that we should therefore go to a policy of no first use; but if first use is not credible, the risk of escalation is low, and we are therefore at the low end of the scale. But it hardly follows that because credibility is weak, it should be weakened still further. The principle of diminishing marginal utility implies that it is precisely at this end of the scale that we are likely to get the biggest return for marginal increases in the level of risk, and where we pay proportionately the biggest price for small cuts in the level of risk.

But arguments of this sort are still essentially qualitative in character, and there is a good deal of room for analysis that is empirical, and in fact quantitative in nature. In fact this type of approach really paves the way for empirical analysis of a special sort. How do we know, for example, that the principle of diminishing marginal utility can be applied directly to these problems? Perhaps there is a certain threshold effect, where below a certain point, risk can be dismissed as virtually negligible. In that case, our curve might have the shape depicted in Figure (3):

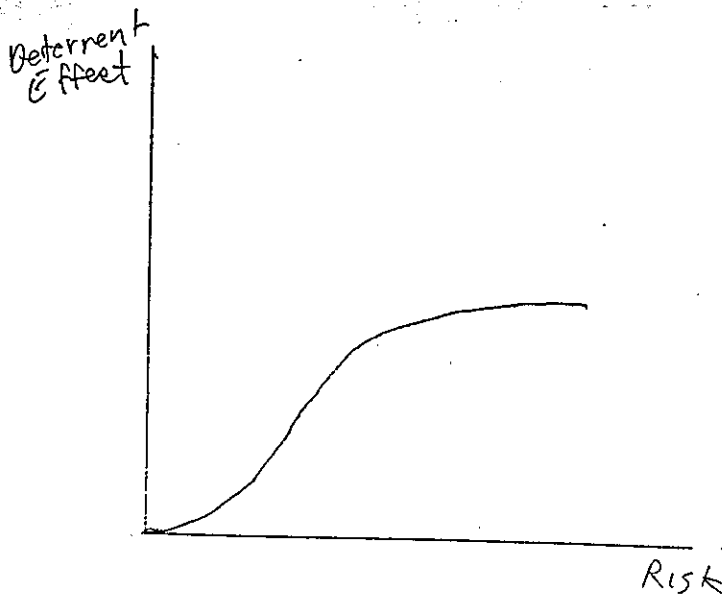


Figure (3)

Or suppose that we looked at the problem from the other side. Our variable, that is, would no longer be the probability that matters would escalate, but rather the probability that the conflict could be kept under control. Then how would the principle of diminishing marginal utility apply? The greater the chance that the lid could be kept on things, the more political behavior would be affected, but the marginal effect would diminish. Going from a zero chance of control to a ten percent chance might have a great political impact, much greater perhaps than going from forty percent to fifty percent. But then what would the curve look like? Since (for these purposes) control and escalation are reciprocal concepts, what this means is that the political effect is greatest at the top of the scale, where the risk of escalation is highest, so the curve might have the shape depicted in Figure (4):

Deterrent
Effect

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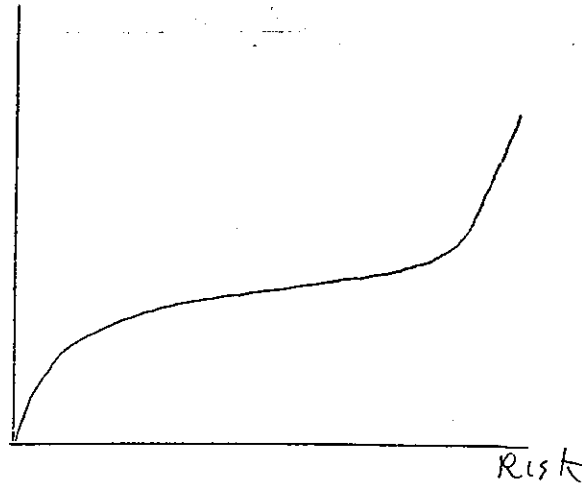


Figure (4)

Well, what is it to be? Much of our thinking is based on implicit assumptions about the shape of these relations, so one of the merits of this kind of approach is that it helps us draw them out and make them explicit. But our basic point is that there is a wealth of material to be drawn from a study of everyday life that can shed a good deal of light on these and other strategic issues.

For example, what can be learned from the regulation of automobile parking? What relation is there between compliance with parking regulations--the "deterrence" of illegal parking--and the certainty that one might get a ticket or be towed away? How does this vary with the intensity with which the regulations are enforced, whether it is done erratically (for example, by the police, who might be able to do it only when real crime was slack) or systematically (by meter maids), the kind of consequences that non-payment of a ticket might have, the severity of the penalty, and so on? One could study debt collection from the same point of view, whether done by private collection agencies or by a government office like the IRS. (IRS audit strategy would be a particularly interesting area to focus on.)

The worlds of crime, especially petty crime, and commerce, in fact, look like a veritable gold mine just waiting to be exploited. For example, consider the policies for the prevention of shoplifting in department stores. Should

the guards be uniformed, or is it better to have floorwalkers in mufti who blend in with the shoppers? It would be fascinating to see how uncertainty affects behavior, and whether the results from these studies fall into any kind of pattern. If they do, this might tell us something basic about the issues of strategy that we are most concerned with.

A study of the psychology of risk might in fact give us some insight into a broader set of political problems. Consider, for example, the problem of explaining the origins of the 1958-1962 Berlin Crisis. Was Soviet policy in this area essentially reactive in nature, deriving from a whole set of anxieties about West Germany, and especially about West German acquisition of nuclear weapons? Or were they practicing a more aggressive policy that ultimately aimed at driving the United States out of Europe? In the absence of hard evidence, one is forced to rely on certain assumptions about behavior, and it would help if we had something more to go on than just common sense. For example, is there anything of a general nature that can be said about the emotional weight of anxieties as opposed to more positive aspirations? What carries more weight, the fear of a loss or the hope of an equivalent gain? If as a general rule the former significantly outweighs the latter, does this lead to a presumption that behavior is mainly reactive in nature?

If there is an asymmetry of this sort, what might it imply about nuclear deterrence? It can help answer one of the perplexing problems about whether nuclear weapons contribute to international stability. The fear of nuclear war might restrain behavior. But one's opponent is also afraid, and one might therefore be tempted to take advantage of those fears. The nuclear fear might be exploited--it might serve as a license for more aggressive behavior. Why do these two effects not simply cancel each other out? Why should it be that the former may be presumed to outweigh the latter?

Imagine that someone who hates mice comes across a mouse eating some food in his pantry. The mouse sees him and runs away, but the man also draws

back. The situation is stable--there is no violent confrontation--because visceral fears dominate on both sides. But assume that the mouse was intelligent. He might dawdle, counting on the man's fears to make him withdraw. The man might then calculate that he would have to restrain his fear and move forward to scare the mouse away. The factor of intelligence thus tends to cancel out, at least to some degree, those visceral anxieties which had been a stabilizing force. When we think therefore about the psychology of risk, we want to look especially at the "animal" sources of human behavior--at the kind of thing embedded in the "hard wiring."

At the cognitive level as well, there is a mode of thinking about these issues that has the same kind of stabilizing effect. We may try hard to present our own behavior as immutable, as though it were like a force of nature, and thus put the burden of choice on the other side. But in more operational terms, we cannot help but assume that the real asymmetry runs in exactly the opposite direction. We think of ourselves as having some control over the situation--the very act of thinking about policy leads us to dwell on those areas over which we exercise choice, so they inevitably loom large in our minds--while more or less impersonal forces are seen as dominating what goes on elsewhere. This tends to load the dice in favor of more conservative and more prudent tactics: if we are making the choices and the burden of keeping things under control is essentially on us, we have to behave cautiously. The more we view the rest of the world as a kind of force of nature, the less we will be tempted to try to exploit fears: it is impossible to terrify an earthquake or a volcano.

It is hard to know exactly how all these issues can be brought into focus, but the best place to start is with the large literature dealing with issues of risk, expectation and uncertainty. Scholars from many disciplines--economics, psychology, ecology, criminology, business--have produced work in this field. To give a small taste of this literature, let us cite just a few

titles:

Kenneth MacCrimmon and Donald Wehrung, Taking Risks: The Management of Uncertainty (1986) ("based on 12 years of research with more than 500 top-level executives")

Maurice Allais, "Le comportement de l'homme rationnel devant le risque: critique des postulats et axiomes de l'ecole americaine," Econometrica (Oct. 1953)

Kenneth Arrow, Essays in the Theory of Risk-Bearing (1974)

Hans Binswanger, "Attitudes toward Risk: Theoretical Implications of an Experiment in Rural India," Economic Journal (Dec. 1981)

Daniel Kahneman and Amos Tversky, "Prospect Theory: An Analysis of Decision under Risk," Econometrica (March 1979)

R. Duncan Luce and Patrick Suppes, "Preference, Utility and Subjective Probability," in R.D. Luce et al, eds., Handbook of Mathematical Psychology, vol. III (1965)

Leslie Real, "Uncertainty and Pollinator-Plant Interactions: The Foraging Behavior of Bees and Wasps on Artificial Flowers," Ecology (Feb. 1981)

And finally one we cannot resist citing:

John R. Krebs et al, "Tests of Optimal Sampling by Foraging Great Tits," Nature (September 1978)

It might be worthwhile for someone to spend a few months foraging through this literature, to see whether it has anything that relates to the kind of questions we are interested in. If so, it might be a good idea to simply talk with some of the people who have been working on issues of risk and uncertainty in these different fields to see if they feel they have any special insight into the problems of concern to us.

Notes

1. Bernard Brodie, "Unlimited Weapons and Limited War," The Reporter, Nov. 18, 1954, p. 18.
2. Albert Wohlstetter, "The Delicate Balance of Terror," Foreign Affairs (January 1959), p. 230.
3. Thomas Schelling, "Surprise Attack and Disarmament," Bulletin of the Atomic Scientists (Dec. 1959), p. 414.
4. See the fascinating article by Simon Head in the Far Eastern Economic Review, published about twelve years ago.
5. Transcribed from a videotape of "The Knife Edge of Deterrence," a program shown on PBS in late 1985.
6. Thomas Schelling, "The Role of Theory in the Study of Conflict," Rand RM-2515 (Jan. 1960), p. iii.
7. Ibid., pp. 11-16. The quotes are taken from pp. 12, 16, and 11 respectively.
8. Thomas Schelling, "Experimental Games and Bargaining Theory," World Politics, vol. 14:1 (1961).
9. Charles J. Hitch, "Research Institutes and University Scholars."

Rand Graduate Institute Commencement Addresses, 1974-1983 (1983), pp. 26-27.

10. 93rd Congress, Second Session. United States Senate, Committee on Foreign Relations. Subcommittee on Arms Control, International Law and Organization. Briefing on Counterforce Attacks (Washington, 1975), p. 35. Emphasis added.

11. Ibid., p. 44. Emphasis added.

12. Ibid., p. 46.

13. Marc Trachtenberg. "The Question of No First Use." To be published this summer in Orbis.