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## The games grown ups play

The disciplinary formation of strategic studies, 1946-1960

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Alfred Whitehead once remarked that one got the essence of a culture not by those things which were said at the time but by those things which were *not* said, the underlying assumptions of the society, too obvious to be stated. (Gore Vidal, 'The Twelve Caesars')

## **Abstract**

This paper reconstructs the history of the discipline of strategic studies in the period 1946 to 1960. It argues that the process of formalisation and systematisation of the theory of nuclear deterrence through this period brought into being the discipline of strategic studies. The key elements of the theory of nuclear deterrence were the view that nuclear weapons were qualitatively different from conventional weapons, that "deterrence" was the sole purpose of nuclear weapons, and that in order to fulfil this purpose the weapons' retaliatory *capability* had to be invulnerable to enemy attack. The paper also argues that strategic studies was developed in emulation of economics, in particular, quantitative economics. The strategic theorists who fashioned strategic studies into a discipline by systematising and formalising deterrence theory were convinced that sound strategic analysis consisted in applying the quantitative methods of economics to the study of deterrence and the specification of its requirements. The paper argues that such a novel application of the quantitative methods of economics was only made possible for the strategic theorists by their use, adaptation and refinement of game theory and systems analysis. It also argues that systematisation and formalisation pushed the view that deterrence was the sole purpose of nuclear weapons in the direction of abstraction, rendering the discipline increasingly irrelevant to the practical concerns of American military planners and policy makers. For unlike the theorists, the planners and policy makers posited no sharp, qualitative difference between conventional and nuclear weapons, only a quantitative one of efficiency and effectiveness. They therefore did not accept the view that the sole purpose of nuclear weapons was "deterrence" as the theorists understood the term. Moreover, by the mid 1950s the military planners in particular had come to the conclusion that the only way to "deter" an enemy nuclear strike was to pre-empt it. The paper argues that the theorists' acknowledgement of the yawning gap between their conception of the purpose of nuclear weapons and that of the policy makers and planners was of paradigmatic importance for the development of the discipline in its formative years.

## **Introduction**

The discipline of strategic studies emerged in the United States during the early years of the Cold War in response to the perceived difficulties presented by the existence of atomic weapons in the context of the rapidly deteriorating American-Soviet relationship. Strategic studies was a creation of civilian academics who regarded atomic or nuclear weapons as being qualitatively different from so-called "conventional" weapons. American military strategists did not accept this view, instead taking it for granted that nuclear weapons were simply bigger and better versions of conventional high explosive and incendiary bombs, and assuming therefore that they could plan to use nuclear weapons just as they would any other kind of weapon. Traditional or established military training and methods were, then, in the view of the military strategists, just as relevant and useful in the nuclear age as they had been in the pre-nuclear age. Aghast at such obscurantism, the theorists were convinced that the military strategists were ill-equipped to develop a viable, but more importantly a *rational*, nuclear strategy. Indeed, for the theorists viability was a function of rationality.

Being very much men of their time, the civilian strategic theorists were in thrall to rationality and to science. The great faith they put in science and technology to solve the military and strategic problems facing the United States was largely a product of the experience of the Second World War in which scientific research and technological development had contributed significantly to the victory of the Allies over the Axis. The atomic bomb—and, to a lesser extent, the strategic bomber—was taken to be the single most impressive achievement in this regard, all the more so because ‘the bomb’ was an American invention. The strategic theorists believed that science and technology, if properly harnessed, could be just as decisive in the intensifying Cold War with the Soviet Union. However, they went beyond this then widely held belief and embraced the view that, precisely because nuclear weapons were qualitatively different from their conventional counterparts, new techniques to underpin and guide the formulation and application of strategic policy were required. In short for the strategic theorists, strategy had to be erected on a more rational and scientific foundation than had hitherto been the case.<sup>1</sup>

The premium which the civilian strategic theorists put on rationality derived from the assumption that the sole purpose of nuclear weapons was averting war, in particular, war with the USSR. It followed from this assumption that nuclear strategy consisted in the skilful *threatened* use, in fact the clever *non-use*, of nuclear weapons. The threatened or non-use of nuclear weapons to avert Soviet aggression was the nub of deterrence theory which lay at the core of the discipline of strategic studies. Indeed, it is argued in this paper that strategic studies *was* systematised and formalised deterrence theory. The process of systematisation and formalisation of the theory of nuclear deterrence, which began in the late 1940s but proceeded apace through the 1950s, involved the application of the methods of quantitative economics, by way of game theory and systems analysis, to strategic analysis. Strategic analysis focused on the formal and systematic specification of nuclear deterrence and its requirements.

Game theory, basically an abstract mathematical model, was a technique that its proponents and practitioners within the community of strategic theorists believed could be helpful essentially in improving the skills required for the effective non-use of nuclear weapons, namely, rationally making credible deterrent threats, tacit bargaining, and negotiating in crisis situations.<sup>2</sup> The “rationality” of game theory was exactly the same as that for classical economics. States were accordingly treated in game theory as rational, self-interested value or utility maximisers, just as individual buyers and sellers in product markets were treated in classical economic theory. The assumption that values or utilities were measurable and quantifiable, a fundamental assumption of neo-classical economics, was also fundamental to game theory. Only on the basis of this assumption could the “payoffs” or outcomes of the preferences of each side be calculated, the

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<sup>1</sup> Comments Ghamari-Tabrizi, “The most striking effect of the attainment of a scientifically-based strategic asset (namely, atomic and thermonuclear weapons) was the debasement (or at least supersession) of the personal wisdom of the senior officer rooted in combat experience, in favour of intuitions arising from repeated practice in laboratory-waged simulations of future war.” Sharon Ghamari-Tabrizi, ‘Simulating the Unthinkable: Gaming Future War in the 1950s and 1960s’, *Social Studies of Science*, vol. 30, no. 2 (April 2000), p. 164. It should be pointed out that it was the strategic theorists not the senior officers who reached the conclusion that the wisdom of the latter had been debased by the attainment of a scientifically-based strategic asset. She also overlooks the fact that in the view of the theorists the novelty and distinctiveness of nuclear weapons did not so much derive from their scientific basis (which was common to many contemporary weapons systems, including conventional ones) as their assumed or posited non-usability.

<sup>2</sup> Daniel Hausman and Michael McPherson observe that “Game theory is concerned with strategic interactions among individuals who are to some extent rational. An interaction among individuals is strategic if the choices of some individuals depend on the choices of others. Most game theory employs standard conceptions of rationality.” Because the situations in which individuals interact are so many and varied, “game theorists typically suppose that the only relevant properties of individual strategies are a) their causal consequences for the plays made by others and b) the utilities of the outcomes of different combinations of strategies—the ‘payoff structure’”. Daniel M. Hausman and Michael S. McPherson, ‘Taking Ethics Seriously: Economics and Contemporary Moral Philosophy’, *The Journal of Economic Literature*, vol. XXXI, no. 2 (June 1993), pp. 717-718

credibility of their threats assessed, and such a complementary configuring of the threats made by each side as to achieve a “balance of terror” or state of “mutual deterrence” ensured.<sup>3</sup>

While the assumption that states are rational, self-interested value maximisers provided the theoretical basis for the calculation of payoffs or outcomes, as already noted the quantitative methods of economics actually enabled these to be calculated. These same methods of quantification were also incorporated into systems analysis. They enabled the cost and effectiveness of competing nuclear weapons systems to be measured. In systems analysis as employed by the strategic theorists, cost and effectiveness were measured in terms of the ability of these weapons systems to serve the purpose of “deterrence”. This purpose would be served only if the weapons retained their retaliatory capability after enduring a surprise Soviet attack or first strike; in other words, if they were invulnerable to such an attack. Without this capability, the weapons would be unable to “deter” a Soviet attack. “Vulnerability” would, then, invite just the calamity for the United States that it was the sole purpose of its nuclear weapons to avert.

While the strategic theorists regarded their methods as greatly superior to, because much more rational and scientific than, the traditional methods of the uniformed military planners, it is argued in this paper that the theorists in fact exerted little or no influence over American military planning in the period 1946-1960. Thus, not only did the strategic theorists not invent American nuclear strategy, the increasing formalisation and systematisation of the theory of deterrence during the 1950s in fact made strategic studies more and more irrelevant to the concerns of the policy makers and planners. The gap became widest in the mid 1950s when the planners responsible for developing US nuclear strategy came to the view that planning for the pre-emptive use of nuclear weapons, and even pre-emption itself, was the only way to “deter” a Soviet first or surprise attack. It is curious, therefore, that the strategic theorists knew of and openly acknowledged the extent of the gap which divided their assumptions about the purpose and role of nuclear weapons from the assumptions of the policy makers and military planners. But it was precisely their belief in the superiority of their conception of the purpose of nuclear weapons and of the methods they employed in strategic analysis which led the theorists to acknowledge the existence and extent of “the gap”.

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<sup>3</sup> When the term “neo-classical economics” is used in this paper it refers, as is customary, to the re-orientation in economic thinking and analysis inspired by Alfred Marshall, Leon Walras and William Stanley Jevons (who was also one of the founders of econometrics), the three figures who instigated the so-called “marginal revolution” in economics of the 1870s. The central concept of neo-classical economics is “utility”. Comments Joan Robinson: “*Utility* is a concept of impregnable circularity; *utility* is the quality in commodities that makes us want to buy them, and the fact that individuals want to buy commodities shows that they have *utility*.” When consumers enter the marketplace they seek satisfactions or “utilities”. The total number of units of a commodity the consumer purchases effects the amount of satisfaction or “utility” that they obtain from any individual unit such that “With the addition of each unit, it could be expected that the increment in total satisfaction (i.e. the additional or marginal utility) would decline.” This is the law of diminishing marginal utility. Because the amount of satisfaction declined with the quantity of units of a commodity purchased, the “rational consumer” would “be prepared to pay less for the last unit than for the preceding ones and a reduction in price would be necessary to induce him to buy more.” For the neo-classicists, “the aim and purpose of economic life is to get as much of it [i.e., utility] as possible. And, set out in a diagram, it looks just like a measurable quantity.” When utility was installed as the central concept of economics, so did mathematics become predominant in economic analysis—a development which seemed to “promise a new dawn for economics as a truly scientific subject.” For Jevons, indeed, mathematics (and statistics) was the key to making Political Economy into an “exact Science”, as he put it. Joan Robinson, *Economic Philosophy*, Penguin Books, Harmondsworth, 1964 [C.A. Watts, 1962], pp. 48, 49, 65. For the neo-classical economists, economic investigation began with the “formulation of abstract models of the economy’s behaviour in which the frictions and untidiness of the real world were neglected”, a “*modus operandi*” which was perfectly suited “to the use of mathematics in economic analysis and particularly to the application of the differential calculus.” William J. Barber, *A History of Economic Thought*, Penguin Books, Harmondsworth, 1967, pp. 166 and 167. Some of these important issues in the development of the discipline of economics are taken up below, in the section dealing with the manner in which strategic studies fashioned itself in the likeness of economics.

“The gap” was therefore an extremely important factor in the development of systematised and formalised deterrence theory, particularly after 1949 when the two conflicting conceptions of the purpose and role of nuclear weapons began to move even further apart than they had been at the outset. Indeed, “the gap”, and the theorists’ awareness of it, was of paradigmatic importance and therefore needs to be given due regard in developing an adequate approach to writing the history of systematised and formalised deterrence theory.

### **Developing an approach to writing the history of systematised and formalised deterrence theory: some preliminary remarks**

A narrow disciplinary approach to the reconstruction of systematised and formalised deterrence theory, that is, strategic studies, is explicitly rejected in this paper. This is because such an approach focuses too heavily on the “inside” (prominent practitioners, texts, areas of research, and so on.) and thus loses sight of what is “outside” of the discipline, that is the wider academic, social and political context within which it developed. On the other hand, an approach which concentrates almost exclusively on the context of the discipline also has to be rejected because it is unable to account for the development of its “inside” and for how the “inside” and the “outside” of the discipline connected and interacted with one another. Rather the history of strategic studies has to be approached in a manner which explains how the events, processes and institutions on the “outside” of the discipline shaped the development of its “inside” and, in turn, how its “inside” apprehended and sought to explain the part of the wider context on which it focused. Two concepts from the history and sociology of science, “paradigm” and “*doxa*”, will be used here in developing such an approach to the history of strategic studies.<sup>4</sup>

Due account must also be taken of how sharply the theorists’ thinking about the proper purpose and role of nuclear weapons diverged from the planners’ (and policy makers’) thinking, for no adequate history of the discipline is possible without first doing so. As seen above, the planners and policy makers saw nuclear weapons as essentially bigger and better versions of conventional high explosive and incendiary bombs, and planned for their pre-emptive use on this basis. For the theorists in contrast, the essential difference between the two types of weapons was qualitative not quantitative, a difference which prescribed that “deterrence” as they understood it was the sole purpose of nuclear weapons. Moreover, this purpose could only be realised if the weapons retained their retaliatory capability even after an enemy first strike. These opposing views were at the core of two collections or compilations of quite contradictory assumptions, beliefs and ideas about nuclear weapons and their strategic purpose, which by the 1950s had hardened into, on the one hand, the “deterrence paradigm”<sup>5</sup> and, on the other, the “conceptual framework of pre-emption”. It needs to be pointed out here that, as will be seen more clearly below, “paradigm” and “conceptual framework” are identical in meaning in regard to their respective “disciplines”. However, the two different terms are used in order to enhance the clarity of the analysis and argument, that is, clearly to differentiate the theorists’ set of assumptions about nuclear weapons from that of the planners (and policy makers).

It would be wrong to conclude on the basis of the foregoing, however, that the strategic theorists, military planners and policy makers had nothing in common. Indeed, in reality they shared more

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<sup>4</sup> As is well known, Thomas Kuhn introduced, developed and deployed “paradigm” in *The Structure of Scientific Revolutions*, The University of Chicago Press, Chicago and London, 1962. Pierre Bourdieu introduced the notion of “*doxa*” in ‘The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason’, *Social Sciences Information*, 14(6), 1975, pp. 19-47

<sup>5</sup> The notion of “deterrence paradigm” was suggested by Roman Kolkowicz. While Kolkowicz’ use of the term is a veiled, perhaps even unintended, appeal to the authority of Kuhn, his elucidation of the concept has to be rejected for not only does he not look beneath the paradigm to the underlying *doxa*, he also discusses and analyses “deterrence” in the terms and rhetoric of the strategic theorists themselves. Moreover, he does not consider the paradigm in relation to the conceptual framework of the military planners and policy makers. See Kolkowicz, ‘Intellectuals and the Nuclear Deterrence System’ in Kolkowicz (ed.), *The Logic of Nuclear Terror*, Allen and Unwin, Boston, 1987, pp. 15-46.

than they disagreed on. For example, the transformation in the planners' concept of "deterrence" through the 1950s and the elevation in deterrence theory of the importance of "vulnerability" over the same period—each of which was symptomatic of the hardening of the two sets of assumptions and related beliefs about nuclear weapons—were both developments predicated on the view that the Soviet Union was planning a pre-emptive strike against the strategic nuclear forces of the United States and that it was capable of and intent on eliminating these forces in such an attack. Actually, the theorists, military planners and policy makers shared many assumptions and presuppositions that were even more fundamental than those contained in either the paradigm or conceptual framework. In this paper, this collection of more fundamental assumptions and presuppositions is referred to as the "*doxa*". The *doxa* contained assumptions and presuppositions, mostly undisclosed and unarticulated and, therefore, unchallenged, about the place and role in the world of the United States and the Soviet Union, and about why the former was morally, politically and in other ways superior to the latter. Importantly, the *doxa* gave legitimacy to both the paradigm and the conceptual framework without explicitly sanctioning either.

Before proceeding to unravelling the history of strategic studies, based on the approach foreshadowed above, a number of common misapprehensions and myths surrounding the academic strategists and the extent of their influence on the making of American nuclear strategy have to be revealed for what they are. These misapprehensions and myths together comprise what might be called the folklore of the Cold War and American nuclear strategy.

### **The folklore of the Cold War and American nuclear strategy**

According to academically respectable folklore, American nuclear strategy during the entire Cold War could be described and characterised in one word, "deterrence". "Deterrence" defined a strategy by which the United States sought to avert Soviet aggression, in particular a Soviet first or pre-emptive nuclear strike, by the threat of devastating retaliation. Defined thus, American nuclear strategy was a defensive, reactive and, in the end, restrained response to unremitting Soviet provocation and malevolence. Having little or no room for manoeuvre, and for the sake of their own country's and the West's survival, American leaders were compelled to resort to the threat of nuclear retaliation. Given that over the entire duration of the Cold War the Soviet Union did not launch a nuclear attack against the United States or its allies, the strategy of nuclear deterrence was in academic circles and elsewhere widely acclaimed a remarkable success, a tribute to American, and more generally Western, restraint, rationality and morality.

Academically respectable folklore would also have it that those largely responsible for devising the strategy of nuclear deterrence were a group of civilian strategic theorists employed by the RAND (Research and Development) Corporation, a "think tank" located in Santa Monica, California which was established by the United States Air Force in 1946.<sup>6</sup> While the folklore extends to the

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<sup>6</sup> General Curtis Le May, at the time Deputy Chief of Staff for Research and Development (and soon after the Commanding General of the Strategic Air Command), described the mission of Project RAND (as RAND was then called) thus: "Project RAND is a continuing program of scientific study and research on the broad subject of air warfare with the object of recommending to the Air Force preferred methods, techniques and instrumentalities for this purpose." cited in R.D. Specht, *RAND: A Personal View of its History*, P-1601, The RAND Corporation, Santa Monica, Calif., October 23, 1958, p. 2 Harold Linstone observes that it was the realisation that "Planning for weapon systems on the basis of military officers' experience was now no longer possible"—a realisation which grew out of the experience of World War II—which led to the creation of the RAND Corporation as a think tank to assist in military planning. He also notes that at RAND, "Economics joined mathematics and engineering as a basic discipline. The initial tasks concerned nuclear weapon strategy; they were perceived to be well-structured problems and quickly resulted in great success and prestige for RAND. Game theory, linear programming, inventory theory, cost-effectiveness analysis and the planning-programming-budgeting concept were developed there in the 1950s. The term *systems analysis* came to be used as a label for RAND's activities." Harold A. Linstone, 'Breaking out of the Systems Quandary' in Kenyon B. De Greene (ed.), *A Systems-Based Approach to Policymaking*, Kluwer Academic Publishers, Boston/Dordrecht/London, 1993, p. 277. In this paper, "systems analysis" is used to refer to a

acknowledgement that the theorists were not so much concerned with the practicalities of “deterrence”, it does attribute the RAND strategic theorists with establishing the theoretical underpinnings and constructing the conceptual framework of the strategy. These theorists, seized of the vast divide separating conventional from nuclear weapons but deeply concerned about the pernicious intentions of the Soviet Union, conceived of a strategy which sought through the threatened or non use of nuclear weapons to avert war by “detering” Soviet aggression.

Obviously, the RAND strategic theorists would not have had the opportunity to take the lead in developing the guiding principles of American nuclear strategy had they not had an inordinate level of influence over the policy makers and military planners who were formally responsible for devising American nuclear weapons policy and strategy. An important part of the folklore surrounding the theory and strategy of “deterrence” and the role of the strategic theorists, therefore, is the belief that the policy makers and planners shared with the theorists the view that nuclear weapons were qualitatively different from their “conventional” counterparts. Like the theorists, then, the policy makers and planners accepted that nuclear weapons should play a central role in United States military strategy for dealing with the Soviet Union, but that the role of nuclear weapons was to avert or “deter” Soviet aggression. According to the folklore, for the theorists, as for the policy makers and planners, this role for nuclear weapons was performed in the act of threatening to retaliate with them to a Soviet attack, not in actually using them. Threatening to retaliate with nuclear weapons was an act of non-use.

Implicit in the folklore is the view that the invention of nuclear strategy fell to the strategic theorists because they were actually prepared to think about the “unthinkable” and, more importantly, how it might be avoided. Unlike the strategic theorists, the policy makers and planners evidently were unable to deal rationally and scientifically with such grave matters, and therefore simply became the consumers of the ideas about the proper role and use of nuclear weapons supplied to them by the theorists. These ideas were constitutive of American nuclear strategy. That is to say, they guided the policy makers and planners in the choices they made in regard to numbers and yields of weapons, types of bombers, basing modes for the bombers, locations of bases, and so on. In guiding the policy makers and planners, the strategic theorists ensured that American nuclear weapons would increasingly serve their proper purpose which was, of course, “deterrence”. “Deterrence” was the theoretical hub of strategic studies, an academic discipline, more specifically a policy science, which was created and developed by the RAND strategic theorists.<sup>7</sup>

In the folklore, strategic studies was the discipline created by the RAND theorists’ and in which their thinking about the purpose and role of nuclear weapons, and nuclear strategy itself, increasingly became scientifically rigorous—even while it remained essentially abstract and speculative.<sup>8</sup> Indeed, according to the folklore, strategic studies emulated economics which it was

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particular technique of the strategic theorists (cost-effectiveness analysis), not to refer to the entire range of the techniques developed at RAND as Linstone does. .

<sup>7</sup> Ronald Brunner observes that “The term ‘policy sciences’ can be traced back at least to 1943 when [Harold D.] Lasswell crystallized the concept from many sources, and Lasswell and [Myres S.] McDougal published ‘Legal Education and Public Policy: Professional Training in the Public Interest’.” Brunner also notes that “‘The policy sciences’ appropriately refers to the problem-oriented, contextual, and multi-method approach to human dignity developed by Lasswell, McDougal, and their collaborators, and not to other parts of the policy movement that diverged radically from that approach or emerged separately. Other distinctive parts of the policy movement include, for example, ‘policy analysis’ (economics), ‘policy studies’ (political science), ‘public affairs’ (philosophy), and ‘socioeconomics’ (sociology), which often differ significantly in basic assumptions, aims, concepts, methods, and outcomes, as well as explicitness and integration.” Ronald D. Brunner, ‘A milestone in the policy sciences [book review essay of Harold D. Lasswell and Myres S. McDougal, *Jurisprudence for a Free Society: Studies in Law, Science and Policy* (2 Vols, 1992)], *Policy Sciences*, pp. 45 and (note 1) 65-66. On Brunner’s taxonomy, and given its parasitic dependence on economics, strategic studies would belong to the policy analysis part of the wider policy movement.

<sup>8</sup> For a reasonably clear expression of that part of the folklore of “deterrence” which deals specifically with strategic studies see Hedley Bull, ‘Strategic Studies and Its Critics’, *World Politics*,

believed had developed in such a way as to allow economic policy making to be rationalised and to give policy makers greater control over economic variables. By emulating economics, it was thought that strategic studies would similarly rationalise defence policy making and nuclear strategy formulation, and allow policy makers a much greater degree of control over the strategic environment than they had hitherto been able to exercise. The primary methods of analysis employed by the strategic theorists, namely game theory and systems analysis, were in effect vehicles for the application of the quantitative methods of economics to strategic analysis.

According to the folklore, the traditional methods of the military profession had, with the advent of nuclear weapons, become obsolete and accordingly been usurped by the rational, scientific methods of the strategic theorists. It was the lack of rationality and scientific rigour in the traditional methods they employed which accounted for the inability of the planners to think sensibly about nuclear weapons, relegating them in the folklore to the rank of consumers of strategic ideas. The source of this irrationality was their failure to draw a qualitative distinction between nuclear and conventional weapons, a failure which blinded them to the merits of the theorists' view of the proper purpose and role of nuclear weapons. Unfortunately for the theorists, their own much vaunted rationality, and the supposed irrationality of their military counterparts, did not translate into relevance in the making of American nuclear strategy.

In the following section, the two conceptions of "deterrence", one belonging to the theorists the other to the planners (and policymakers), are explicated. The two conflicting conceptions hardened during the 1950s in response to the same strategic innovations, namely the successful testing and deployment of thermonuclear weapons and the development of truly intercontinental strategic bombing aircraft, first by the United States and later by the Soviet Union. These innovations only served to widen "the gap" between strategic theory and strategic practice and, therefore, to heighten the theorists' irrelevance.

### **The two concepts of "deterrence", and the notion of "vulnerability"**

In an apparent, and very early, vindication of the strategic theorists' nascent ideas, a national policy of "deterrence" was approved by the US National Security Council (NSC) in November 1948.<sup>9</sup> This policy made possession of the atomic bomb the centrepiece of American strategy. However, this was an apparent and not an actual vindication because there were even at this early stage two conflicting conceptions or notions of "deterrence" in circulation in the American strategic community, one which was the intellectual property of the strategic theorists and the other of the planners and, by association, the policy makers. The folklore fails to account for this fact and, as a result, the widespread belief or presumption that the strategic theorists invented American nuclear strategy, which is central to the folklore's integrity and persuasiveness, is able to stand virtually without challenge or qualification.

The national policy of "deterrence" approved by the NSC in 1948 was, not surprisingly, an unambiguous endorsement of the policy makers' and planners' concept, not the theorists'. In what, then, did the policy makers' and planners' concept consist, and how did it differ from the

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vol. 20, no. 4 (July 1968), pp. 593-605. A very worthwhile critique of strategic thought, and more broadly the folklore, is Joseph E. Schwartz, 'Strategic Thought: Methodology and Reality' in Charles McCoy and John Playford (eds.), *Apolitical Politics: A Critique of Behavioralism*, Thomas Y. Crowell, New York, 1967, pp. 55-74. Also very worthwhile is P.M.S. Blackett, 'Critique of Some Contemporary Defence Thinking', *Encounter*, April 1961, pp. 9-17. Blackett deals in particular with Albert Wohlstetter's 'Delicate Balance of Terror' which is analysed at some length below. A more recent work which draws on and perpetuates the folklore of the Cold War and American nuclear strategy is Paul N. Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America*, Cambridge MA, USA and London, England, The MIT Press, 1997. For a critical review article of Edwards' *Closed World* see Mark Rix, 'Opening the Closed World of the Cold War and American Nuclear Strategy', *Prometheus*, vol. 17, no. 2, 1999, pp. 211-224.

<sup>9</sup> NSC 20/4, 'U.S. Objectives with Respect to the USSR to Counter Soviet Threats to U.S. Security', November 23, 1948, reprinted in Thomas H. Etzold and John Lewis Gaddis (eds.), *Containment: Documents on American Policy and Strategy, 1945-1950*, Columbia University Press, New York, 1978

theorists'? In 1948, prior to the first successful Soviet atomic test which was conducted the following year, for the policy makers and planners "deterrence" was at best an unintended by-product—intimidation—of the planning for the early use of nuclear weapons in a war with the Soviet Union. The policy makers and planners at this point regarded nuclear weapons as a counterweight to an alleged Soviet superiority in conventional weapons (infantry, tanks, etc.) in Europe. After the Soviet atomic test in 1949, "deterrence" for the policy makers became the by-product of planning for the pre-emptive use of nuclear weapons (basically, "pre-emption" meant getting in the first blow, delivering a first strike). By the mid-1950s both the United States and the Soviet Union had equipped themselves with thermonuclear weapons (hydrogen bombs) and intercontinental strategic bombers. At this time, "deterrence" for the military planners and, by association the policy makers, became squarely equated with pre-emption. That is, by the mid-1950s the military planners were firmly of the view that, in order to "deter" a Soviet first, pre-emptive or surprise attack, it had to be pre-empted. In other words for the planners, planning an American first strike was the only way of "detering" a Soviet first strike. This was a considerable distance indeed from the theorists' conception of "deterrence"—as the credible (and rational) threat of devastating retaliation. It was precisely because the policy makers and planners did not regard nuclear weapons as a distinct class or category of weapons the sole purpose of which was "deterrence" as understood by the theorists, that they were able to plan to use nuclear weapons in war with the Soviet Union and do so in a pre-emptive manner, that is, in a way designed to forestall anticipated Soviet aggression.

In the mid-1950s, and in responding to exactly the same strategic developments as caused the planners to transform their own concept of "deterrence", the strategic theorists elevated the notion and problem of "vulnerability" to a standing in the theory of nuclear deterrence which very nearly put it on a par with "deterrence" itself. These developments served only to vindicate and strengthen the theorists' belief that the sole and proper purpose for nuclear weapons was "deterrence" as they understood it. "Vulnerability" expressed the view that, because the strategic nuclear weapons of the United States would be destroyed in a Soviet pre-emptive strike, they were unable to "deter" such an attack. Hence, only if they retained their retaliatory *capability* even in the event of such an attack would they be able to "deter" it. This was the point in time, then, when the two concepts of "deterrence", nuclear strategic theory and nuclear strategic practice, moved farthest apart.

In the following section, the story of formalised and systematised deterrence theory over the first fifteen years of its development will be recounted, from the perspective of the strategic theorists. Recounting the story from this perspective will serve to highlight how the theory dealt with the major strategic innovations over this period and how it sought to rationalise the gap between strategic theory and strategic practice which these innovations only served to widen. This will in turn set the scene for setting out more systematically the approach adopted to writing the history of strategic studies based on the concepts of "paradigm" and "*doxa*", which was introduced and briefly explained above.

### **The first fifteen years in the development of systematised and formalised deterrence theory at RAND: the major identities and their contributions**

The view that "deterrence" was the sole purpose of nuclear weapons was originally advanced by Bernard Brodie in 1946.<sup>10</sup> He contended that, in the atomic age, the chief purpose of the "military establishment" was no longer to fight and win wars, rather it was to avert them. This was the founding moment for the theory of nuclear deterrence. Even at this early stage Brodie realised that the retaliatory force would only be able to avert or deter an enemy attack if it were not vulnerable to such an attack. The force therefore had to be protected, in other words, made invulnerable. He thought that putting the retaliatory force underground in reinforced shelters might be one way of protecting the bombers from a surprise enemy attack. In reaching the conclusion that the retaliatory force had to be protected from attack, Brodie had reasoned that protecting the force's retaliatory *capability* would not only prevent or "deter" the enemy's use of atomic weapons, it would also obviate the need for the United States to use them. His conception of "deterrence" was

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<sup>10</sup> Brodie, 'Implications for Military Policy' in Brodie (ed.), *The Absolute Weapon: Atomic Power and World Order*, Harcourt, Brace & Co., New York, 1946, pp. 76-81.

in effect, therefore, a prescription for the non-use of atomic weapons. Brodie's ideas on the purpose of nuclear weapons and how that purpose could be fulfilled were the nub of deterrence theory which endured through the 1950s and beyond, the theory which was systematised and formalised at RAND during that decade.

It was on a fairly abstract plane that Brodie first offered his analysis of the problem of "deterrence" and the specification of its requirements. For example, even though he anticipated that before too long the Soviet Union would develop nuclear weapons, the first Soviet atomic test was still some years off. After the Soviet test (1949), the development of the concept, and theory, took place in the context of the Soviet Union's development of a deliverable nuclear weapon and the building of its arsenal, the rapid and massive growth in size of the American nuclear arsenal and Air Force (particularly the Strategic Air Command (SAC)), and the acquisition of thermonuclear weapons and intercontinental bombers, first by the United States and then by the Soviet Union.

Through the late 1940s and 1950s the theory of nuclear deterrence thus, on the one hand, became less abstract as it was applied to actually existing weapons and weapons "delivery systems" and as the Soviet Union emerged first as a nuclear and then a thermonuclear power. On the other hand, however, the theory grew more and more abstract as its formalisation and systematisation continued apace; that is, as game theory and systems analysis were increasingly used in the analysis of "deterrence" and its requirements. One sure indication of the increasing abstractness of the theory was that it moved further and further from the actual concerns, intentions and decisions of the policy makers and military planners. The further widening of "the gap" separating strategic theory from strategic practice, a function of the abstractness entailed in the use of systems analysis and game theory, was consciously pursued by the strategic theorists. The process of systematising and formalising deterrence theory took place in the RAND Corporation, which as noted was a so-called "think tank" established by the United States Air Force in 1946. The establishment of the RAND Economics Division in 1947 effectively initiated the process of systematising and formalising deterrence theory. This process would accelerate through the 1950s not only in response to the strategic and technological developments just outlined but also as a result of the increasing application of systems analysis and game theory to the analysis of these developments.

In order to identify and account for the events, problems and issues involved in the process of systematisation and formalisation of the theory of nuclear deterrence beyond Brodie's founding contribution, the major works in the theory published during the 1950s by three key RAND strategic theorists during the 1950s will be briefly surveyed. These are Albert Wohlstetter, 'The Delicate Balance of Terror' (1959); Thomas Schelling, *The Strategy of Conflict* (1960); and Bernard Brodie, *Strategy in the Missile Age* (1959).<sup>11</sup> However, before turning to these works, a corrective note on standard histories of the theory of nuclear deterrence must first be entered. It is commonplace in historical accounts of deterrence theory and strategic studies arbitrarily to divide the first fifteen or so years of their development into a very brief first wave, 1945-1946, in which Brodie was the dominant figure, and a second wave or "Golden Age", stretching roughly from 1954 to 1965, during which strategic theorists such as Albert Wohlstetter, Thomas Schelling and others came to prominence. The trouble with this periodisation of the history is that Brodie was also very prominent during the so-called second wave or Golden Age. Indeed as already noted, the growing importance of "vulnerability" in deterrence theory was a vindication of Brodie's earliest ideas, and the systematisation and formalisation of deterrence theory which occurred in the second wave was in fact a systematisation and formalisation of these very same ideas. Such a periodisation is therefore rejected here.<sup>12</sup>

<sup>11</sup> In commenting on developments in strategic theory through the 1950s, Colin Gray observes that, in response to the experience of "limited war" in Korea and the development of the hydrogen, thermonuclear or "super" bomb, "analysts at and connected with the Rand Corporation in Santa Monica, California, launched a public, *and classified*, strategy debate that was to sustain itself until the mid-1960s." Colin S. Gray, *Strategic Studies: A Critical Assessment*, Greenwood Press, Westport, Conn., 1982, p. 15.

<sup>12</sup> In addition to the two "waves" of development already mentioned, a number of authors have also suggested a "third wave" that spans the period from 1971 to 1981. For more on the wave theory of development see, e.g., Gray, *Strategic Studies*, pp. 15-23 (Gray is one who speaks of a "third wave"); Barry Buzan, *An Introduction to Strategic Studies: Military Technology and International*

Wohlstetter's 'The Delicate Balance of Terror' is extremely important in the development of systematised and formalised deterrence theory because it was in this article that the concept of "deterrence", which Brodie had introduced in 1946, was shown to have remained relevant into the late 1950s when a much altered strategic situation obtained.<sup>13</sup> Even though Brodie's founding contribution was not directly acknowledged in 'The Delicate Balance of Terror', Wohlstetter demonstrated that overcoming "vulnerability" was the lynch-pin of a strategy of deterrence as conceived by the theorists. Indeed, the 'Balance of Terror' amounted to little more than an oft-repeated warning of the vulnerability of the American retaliatory or second-strike force, the consequential likelihood of a successful, "warningless" Soviet first strike, and the difficulties for the United States in achieving through the 1960s anything more than a delicate and dangerously fragile strategic balance.

Wohlstetter was undoubtedly motivated by the growing importance in official war plans of what SAC and the Air Force called the BRAVO or blunting mission (a pre-emptive, counterforce strike which would employ both thermonuclear and nuclear weapons) to advance the argument in the 'Balance of Terror' article that it was mistaken to believe that "deterrence" consisted in nothing more than a matching of the enemy's ability to strike first. Rather, insisted Wohlstetter, "To deter an attack means being able to strike second" or, in other words, having the "capability to strike second".<sup>14</sup> At bottom, the problem for Wohlstetter as with the other theorists was that the planners did not differentiate clearly enough between conventional and nuclear weapons. The planners therefore took it for granted that their skills, the traditional skills of the military profession, had retained their relevance and usefulness into the nuclear, and thermonuclear, age. This was diametrically opposite to what the theorists thought. On this score, Thomas Schelling no doubt spoke for all the theorists when he contended that deterrence involved the "non-use" of military force, and, therefore, required skills markedly different from those possessed by members of the military profession who were chiefly concerned with the *application* of actual force.<sup>15</sup>

Beyond the foregoing, Wohlstetter's article was also important because it summarised two classified reports that had been prepared for the Strategic Air Command earlier in the 1950s by a team of RAND analysts led by Wohlstetter himself.<sup>16</sup> In the two studies, Wohlstetter and his cohorts had shown that, based on the assumption that SAC's bombers comprised a deterrent force, SAC's overseas bases (and the aircraft stationed at them) were vulnerable to a surprise Soviet attack and therefore did not have the capability to deter such an attack. Thus, at precisely the time that the military planners at SAC (and the Joint Chiefs of Staff) were equating "deterrence" with pre-emption, and taking action in accordance with this equation, Wohlstetter was warning that they did not have the capability to deter a Soviet pre-emptive strike.

The first report in particular demonstrated that systems analysis could be profitably employed in strategic analysis. This classified report, *The Selection and Use of Strategic Air Bases* (R-266), reported the findings of the study that had been conducted by Wohlstetter and his team into alternative basing systems for SAC's bombers (including the then-programmed system). The study considered

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*Relations*, MacMillan Press (in association with the International Institute for Strategic Studies), Basingstoke and London, 1987, pp. 143-160 (Buzan is another "third waver"); Roman Kolkowicz, *op. cit.*, pp. 21-32 (Kolkowicz focuses on the "Golden Age"). See also Michael Howard, 'The Classical Strategists' in Howard, *Studies in War and Peace*, Maurice, Temple, Smith, London, 1970, especially pp. 160-175 and Howard, 'The Strategic Approach to International Relations' in Howard, *The Causes of Wars and other Essays*, Counterpoint (Unwin Paperbacks), London, 1984 [Maurice, Temple, Smith, 1983], especially pp. 43-44.

<sup>13</sup> Albert Wohlstetter, 'The Delicate Balance of Terror', *Foreign Affairs*, vol. 37 (January 1959), pp. 211-234

<sup>14</sup> *Ibid.*, pp. 212 and 213.

<sup>15</sup> Thomas Schelling, *The Strategy of Conflict*, Oxford University Press, London, Oxford and New York, 1963 (reprinted 1973) [Harvard University Press, 1960], p. 9

<sup>16</sup> The first of these reports is R-266 (the number which it was assigned at RAND), *The Selection and Use of Strategic Air Bases* (RAND Corporation, April 1954) and, the second, R-290, *Protecting US Power to Strike Back in the 1950's and 1960's* [RAND Corporation, September 1, 1956].

the comparative costs of the different basing systems against the vulnerability of each to a surprise Soviet attack. The retaliatory capability of the bombers which comprised the strategic striking force hinged on the vulnerability of the bases at which they were located. Thus, the effectiveness of each basing system was judged in terms of its vulnerability for, of course, the capacity of nuclear weapons to deter an enemy nuclear attack was inversely proportional to their vulnerability. The important point here is that Wohlstetter's first study helped to establish the view among the strategic theorists at RAND that good strategic analysis consisted in the use of the quantitative methods of economics.<sup>17</sup> Systems analysis was a vehicle for the direct application to strategic analysis of the mathematical and statistical methods of neo-classical economics and econometrics. Quantitative strategic analysis systematically excluded non-quantifiable variables. Indeed, for the strategic theorists at RAND, general nuclear war between the United States and the Soviet Union came to be regarded as a rather abstract type of conflict the analysis of which could exclude non-quantifiable, and therefore largely irrelevant, variables such as political, cultural and historical factors. The exclusion of such factors from strategic analysis was a function of regarding nuclear weapons as being qualitatively different from their conventional counterparts.

Wohlstetter's use of systems analysis to analyse the problem of "vulnerability", and to determine the most cost-effective system of bases for the strategic bombers, was indirectly an application of game theory to this problem for it at once reinforced and structured the thinking behind Wohlstetter's bases study. At bottom, the bases study, and Wohlstetter's subsequent works, applied a simple-minded line of reasoning to the problem: the closer we are to them, the closer they are to us with the *doxa* adding that they were therefore more likely to hit us first than we were to hit them.<sup>18</sup>

The two reports recommended a number of steps that should be taken by SAC to protect the retaliatory capability of the bombers (and, therefore, their capacity for "deterrence" in the strategic theorists' sense). One of the recommendations of Wohlstetter's second report, *Protecting US Power to Strike Back in the 1950s and 1960s* (R-290), was that the bombers of the Strategic Air Command be placed underground in hardened shelters to protect them from a Soviet nuclear attack thus ensuring the maintenance of their retaliatory capability. It was noted above that this was a recommendation that Brodie had made in 1946.

To understand what Wohlstetter hoped to achieve by publishing the 'Balance of Terror' article in *Foreign Affairs*, it has to be set against the background of the transformation in the planners' concept of "deterrence", from planning for the early use of nuclear weapons through planning for their pre-emptive use to pre-emption itself. The article also has to be considered in light of the reception which R-266, *The Selection and use Strategic Air Bases* and R-290, *Protecting US Power to Strike Back* met with in the policy and war planning agencies. The shrill tones of the article, its constant reiteration of the same message, and its appeal to a wider audience than had read R-266 and R-290 reflect the difficulties faced by Wohlstetter in getting the recommendations and proposals he had made in those studies accepted by SAC and Air Force planners. They also indicate his frustration with the intransigence of the Eisenhower Administration in particular regarding SAC vulnerability—the Administration he thought simply did not take his warnings seriously enough and therefore largely ignored his recommendations, very much thought Wohlstetter to the peril of the security of the United States.

Bernard Brodie, writing in the last article he was to publish before his death, illuminates further what Wohlstetter hoped to achieve by going public. According to Brodie, Wohlstetter's article was the "first sharp public reminder" that the United States had an "important vulnerability problem".<sup>19</sup> Brodie also claims that the article was "precipitated by Wohlstetter's frustration with the Air Force [by which Brodie means SAC]." This frustration was the result of the Air Force's

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<sup>17</sup> A similar point has been made by Fred Kaplan, *The Wizards of Armageddon*, Touchstone (Simon & Schuster), New York, 1983, pp. 109-110. Kaplan also points out that the so-called "hard data" cited in the report were in fact compiled by Air Force Intelligence.

<sup>18</sup> *Ibid.*, pp. 91 and 92

<sup>19</sup> Bernard Brodie, 'The Development of Nuclear Strategy', *International Security*, vol. II, no. 4 (Spring 1978), pp. 67-68

rejection of the RAND study group's preferred basing option for SAC bombers. In the article Wohlstetter not only vented his frustrations with the Air Force, but also sought to raise the general level of alarm about the perilous vulnerability of SAC's bombers. This he did by deliberately downplaying the likely effectiveness of the "popular remedies" for curing vulnerability which he had listed such as the improved alertness (readiness) of the bomber force, dispersal of the bombers to several different locations, the sheltering of the bombers, or improved air defences at SAC bases—several of which were his own suggestions. Brodie observes that Wohlstetter "had been leading for over a year a large research project at RAND which had been trying to find the best means of protecting our bomber aircraft against surprise attack". After rejecting the option of "airborne alert" and the "idea of striking at the enemy before he gets off the ground" (pre-emption), the project group had "decided that the most cost-effective defense of bombers against surprise attack was a slightly-below-ground concrete shelter for each aircraft." Brodie points out that while the Air Force favoured the "airborne alert", this was their second choice after "striking at the enemy before he gets off the ground".<sup>20</sup> It should be noted here that, even if it was their second choice, the airborne alert would have allowed the Air Force an each way bet; on the one hand, it would have left the bombers less vulnerable to attack, but on the other, made possible a quicker pre-emptive strike. More to the point, however, the military planners in the Air Force actually planned for a pre-emptive strike, and had instituted "ground alert" for about one third of the strategic force—suggesting they did not believe the bombers to be nearly as vulnerable to attack as the theorists warned.<sup>21</sup>

Given that it understood "deterrence" as pre-emption, the Air Force's response to Wohlstetter's recommendation was, not surprisingly, swift and furious. It "vehemently rejected" Wohlstetter's solution "invoking slogans which identified concrete [shelters] with the Maginot Line and with excessive *defense-mindedness*." In Brodie's view, Air Force intransigence forced Wohlstetter to go directly to the public "which by its response showed itself both surprised and alarmed at the situation he depicted—the more so as his elegant use of the facts and figures at his fingertips lent persuasiveness to his message."<sup>22</sup>

Perhaps in a veiled allusion to the limp and unreceptive reaction of the Air Force to Wohlstetter's confidential studies, Brodie had much earlier in *Strategy in the Missile Age* referred to the "professionally-induced aversion" which the "military people who design war plans" had "to being charged with being '*defensive-minded*'". According to Brodie, these same people also had an aversion "to basing any kind of plan on the assumption that the outbreak of hostilities will find us off our guard or otherwise discommoded."<sup>23</sup> It is no mere coincidence or accident that Brodie's

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<sup>20</sup> *Ibid.*, p. 68. The failure of the Air Force to protect the bombers, which remained the case at least up until the 1970s, not only indicated that it was interested in pre-emption, not "deterrence" and retaliation. It also suggested that it did not take seriously the possibility of a "surprise attack". Brodie comments that "I do support fully the belief in the Air Force position that some kind of political warning [of an attack] will always be available. Attack out of the blue, which is to say without a condition of crisis, is one of those worst-case fantasies that we have to cope with as a starting point for our security planning, but there are very good reasons why it has never happened historically, at least in modern times, and for comparable reasons I regard it as so improbable for a nuclear age as to approach virtual certainty that it will not happen, which is to say that it is not a possibility worth spending much money on." *Ibid.*, pp. 68-69

<sup>21</sup> In the late 1950s, SAC was pushing forward with plans to put about a quarter of the B-52s on airborne alert. While the JCS "approved the airborne alert in principle", it "recommended that it be put into operation only when the President considered such special precautions necessary." The prohibitive cost of continuous airborne operations was the main reason for this recommendation. David Alan Rosenberg, "The Origins of Overkill: Nuclear Weapons and American Strategy, 1945-1960", *International Security*, vol. 7, no. 4 (Spring 1983), p. 49

<sup>22</sup> Brodie, 'Development of Nuclear Strategy, *op. cit.*, p. 68; emphasis added.

<sup>23</sup> Brodie, *Strategy in the Missile Age*, The RAND Corporation, Princeton University Press, Princeton and Oxford University Press, London, 1959, pp. 245 and 246; emphasis added. Brodie pointedly remarks that being 'defensive-minded' is in practice "construed as a frame of mind that seeks to take seriously into account the possibility that the enemy, rather than ourselves might seize and hold the initiative during the crucial early phases of the war." *Ibid.*, p. 246

comments on the aversions suffered by the “military people” appeared immediately following his admission that the “preparations and concepts” for the use of the strategic air force were designed more for pre-emption than retaliation.<sup>24</sup>

It was through Wohlstetter’s classified and published works that “vulnerability” moved from the periphery, where it had been in Brodie’s earliest work, to centre stage in the theory of nuclear deterrence. Indeed, ‘The Delicate Balance of Terror’ was extremely well received by Brodie and Schelling.<sup>25</sup> To begin with, both Brodie and Schelling accepted Wohlstetter’s contention that the “balance of terror” was delicate. They also made it clear they were convinced by Wohlstetter’s arguments that: “deterrence” was becoming increasingly difficult for the United States to achieve; the problem of “vulnerability” was too readily dismissed as unimportant by too many people who should have known better; and, it was seriously mistaken to believe that “deterrence” was simply a matter of matching the Soviet Union’s ability to strike first. Brodie and Schelling shared with Wohlstetter the view that the problem of “vulnerability” was the most urgent and pressing strategic issue, for they likewise reasoned that the ability of nuclear weapons to “deter” a Soviet attack, which was their sole purpose, would be effaced if they were vulnerable to attack.

Brodie was acutely conscious of the vast difference between the theorists’ and the military planners’ notions of “deterrence”, that is, of their divergent understandings of the purpose of nuclear weapons. In *Strategy and the Missile Age* he attempted to make the theorists’ conception more consistent with the planners’, an attempt motivated by a desire to show that “the gap” which separated the theory from the practice of “deterrence” was not as unbridgeable as it first appeared to be. Bridging the gap would mean that the theorists need not any longer be left out on the nuclear weapons policy limb where they had been since 1946 when he first published his recommendations on what the proper purpose of nuclear weapons should be. Brodie thought that the gap could be bridged by conflating the two conceptions of “deterrence”, a feat he performed in demonstrating that “deterrence” by protection of a massive retaliatory capability and “deterrence” by massive pre-emption could be achieved by one and the same strategic striking force armed with thermonuclear weapons.<sup>26</sup> Importantly, however, in attempting to bring the two conceptions of “deterrence” closer together Brodie did not cease to believe in the soundness, correctness and superiority of his own conception. Indeed, through the 1950s he became more and more convinced of this. For his part, Schelling in the *Strategy of Conflict* was not at all prepared to come to terms with the military planners, nor to make any concessions to them as Brodie had done. This can be traced back to his belief, apparently more unshakeable than Brodie’s, that, on the one hand, the theorists’ concept of “deterrence” rested on genuinely scientific foundations and, on the other, that the traditional skills of the military profession had become obsolete in the nuclear era, primarily concerned as the profession was with the *application* of force.

In *The Strategy of Conflict* (1960).<sup>27</sup> Schelling attempted to demonstrate that game theory could be profitably employed (by theorists, planners and policy makers) in the analysis of strategic problems such as “deterrence” (as understood by the strategic theorists), and tactical nuclear weapons and limited war. He contended that, because game theory regarded states in just the same way as it regarded individual buyers and sellers in product markets, it could be just as useful in studying the behaviour of states, especially in situations of international conflict where the antagonists had both divergent and common interests. Schelling argued that the nuclear stand off between the United States and the Soviet Union was an almost perfect example of this type of conflict. Beyond their enmity and deep-seated animosities, he assumed that the two sides shared a

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<sup>24</sup> *Ibid.*, p. 245

<sup>25</sup> For Brodie’s comments on Wohlstetter’s arguments and analysis in the ‘Balance of Terror’ see *Strategy in the Missile Age*, pp. 282-283. Schelling’s assessment can be found in *The Strategy of Conflict*, pp. 233-234

<sup>26</sup> Brodie, *Strategy in the Missile Age*, *op. cit.*, p. 283

<sup>27</sup> Lawrence Freedman describes Schelling as the “exemplary formal strategist” for whom the “exemplary methodology” was game theory. Lawrence Freedman, *The Evolution of Nuclear Strategy*, Macmillan, London & Basingstoke, 1981, p. 181. Colin Gray observes that Schelling encouraged his readers to believe that “strategy could be played as a rational interstate game.” *op. cit.*, p. 136.

powerful interest in avoiding the destruction that would ensue from an all-out nuclear war. Thus, according to Schelling, game theory could be useful in the analysis of “deterrence”, especially in specifying the requirements of “mutual deterrence”.<sup>28</sup>

Tactical nuclear weapons—which NATO first deployed in Europe in 1952—presented deterrence theory with a thorny and difficult problem. The nub of this problem was that tactical nuclear weapons seemed much more like conventional armaments than their larger “strategic” counterparts, either nuclear or thermonuclear. In other words, the difference between tactical nuclear weapons and conventional weapons was evidently a quantitative rather than a qualitative one. For this reason they appeared, at least at first sight, to be much more useable than strategic nuclear weapons, especially in so-called “limited wars”.

Thus for deterrence theory the problem with limited war was exactly the same as that with tactical nuclear weapons: both seemed to undermine the posited qualitative distinction between nuclear and conventional weapons making the former apparently useable. If useable, nuclear weapons would cease to have the capacity to serve their fundamental purpose which was to avert or deter nuclear war between the United States and the Soviet Union. As both Brodie and Schelling argued, this problem could perhaps be overcome if the “vast watershed of difference” (Brodie’s phrase) between the use and non-use of nuclear weapons was preserved and reinforced. As Schelling saw it, this depended on demonstrating that it was still possible, despite considerable difficulties, to differentiate sharply between conventional weapons and nuclear weapons. As for Brodie, he was more worried that there was no clear distinction between tactical nuclear weapons and strategic nuclear weapons which had serious implications for any attempt to keep war limited. If the maintenance of this distinction could play no part in keeping war limited, then that between use and non-use would have to serve this purpose. These points will become clearer below.

Schelling’s analysis of limited war was an attempt to fit both it and tactical nuclear weapons into the framework of game theory by demonstrating how the theory could help in ensuring that the difference between the use and non-use of nuclear weapons, as with that between nuclear and conventional weapons, could be maintained and reinforced. This was difficult, because Schelling admitted that the distinction between tactical nuclear weapons and conventional weapons had become “blurred” which meant that the vast difference between the use and non-use of nuclear weapons was similarly hazy. Essentially for Schelling, the use of tactical nuclear weapons in a limited war would establish a dangerous precedent for their use *and* the use of larger nuclear weapons by either side in both limited and general wars in the future. As Schelling saw it, once the nuclear threshold was crossed there would be no way back. Having crossed the threshold, any attempt to limit use to “tactical nuclears”, or to keep limited wars from developing into total or general nuclear war, would thus be totally and irretrievably in vain. The only solution was to preserve the distinction between use and non-use and between nuclear and conventional weapons, even if as Schelling conceded the latter distinction was becoming a largely symbolic or psychological one.<sup>29</sup>

Similarly, Brodie wanted to show that limited war and tactical nuclear weapons did not undermine the theory of “deterrence” he had founded but could be incorporated into it, even if this involved difficulties similar to the sort encountered by Schelling. While there remained an intrinsic or qualitative difference between nuclear and conventional weapons—here his analysis differed from Schelling’s—unfortunately, admitted Brodie, this could no longer be said of tactical and strategic nuclear or thermonuclear weapons.<sup>30</sup> Thus neither side to a conflict could know whether the other side was exercising restraint by restricting itself to the use of tactical weapons, rendering utterly futile and self-defeating any attempt to keep war limited in this way. Under these circumstances, there would be no incentive for either side to confine itself to the use of tactical nuclear weapons. As with Schelling, the solution for Brodie was obvious: a clear and rigid

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<sup>28</sup> Schelling, *op. cit.*, pp. 232-234 and 6-8

<sup>29</sup> *Ibid.*, pp. 75-79 and pp. 256-265

<sup>30</sup> According to Brodie, there was no military reason for tactical nuclear weapons to be of smaller yields than strategic weapons and, moreover, thermonuclear weapons could be used tactically. Brodie, *Strategy in the Missile Age, op. cit.*, pp. 325-326

distinction had to be kept between the use and non-use of nuclear weapons. Only by preserving this fundamental distinction would the proper purpose of nuclear weapons itself be preserved.<sup>31</sup>

This completes the survey of the contributions which Brodie, Wohlstetter and Schelling each made to the development of deterrence theory over the period 1946 to 1960. Their contributions all served to reinforce the view that “deterrence” was the only proper purpose for nuclear weapons. Indeed, by directly taking up the practical and theoretical challenges that such innovations as tactical and thermo- nuclear weapons presented to the tenability of this view they confirmed the problematic which the challenges together constituted as the core of the discipline of strategic studies. The use of game theory and systems analysis in taking up the challenges—of vulnerability, and of preserving the distinction between nuclear and thermonuclear weapons, nuclear and conventional weapons, and use and non-use—validated and bestowed scientific respectability on the discipline.

The following section considers the problems frequently confronted in writing histories of disciplines in the social, policy or applied social sciences, and the specific problems confronted in reconstructing the history of strategic studies. It also shows how in employing the concepts of “paradigm” and “*doxa*” these problems can be overcome. As briefly noted above, the use of these concepts in reconstructing the history of strategic studies makes possible the development of an approach which enables the content and context of the discipline each to be given the attention it deserves. It thus avoids concentrating on one at the expense of the other, a common pitfall of disciplinary history. It also enables the interaction between the “inside” and “outside” of the discipline to be explained and understood. The approach adopted in this paper, therefore, is able to show how the “inside” of the discipline was shaped by what was “outside” and how the “inside” apprehended and sought to explain what was “outside”. It will thus enable the contributions which Wohlstetter, Schelling and Brodie each made to the development of systematised and formalised deterrence theory to be explained and understood, not only in light of “the gap” which separated strategic theory from strategic practice, but also in terms of the common assumptions and beliefs that lay beneath the theory and the practice of nuclear deterrence.

### **The history and sociology of science and the development of systematised and formalised deterrence theory**

The problems confronted in writing the history of strategic studies are very similar to those which are encountered in constructing the histories of other social sciences, whether these be of the policy-oriented (applied) kind or the more theoretically inclined. Prominent amongst these is the difficulty in deciding on what the focus of the history should be. If the wider social and political context is the emphasis, then what happens on the “inside” of the discipline can be obscured. On the other hand, if the history focuses too narrowly on what goes on within the discipline, insufficient attention is paid to what is taking place on its “outside” with the result that no understanding is reached of how the discipline connects and interacts with the groups, institutions and belief systems that constitute its wider context.

This issue is taken up by Hamilton Cravens who is particularly concerned with the writing of the history of the social sciences in the United States. Cravens observes that “Arguably the term *social science* is a misnomer.” He suggests that “The social sciences could be rechristened the *social technologies*” since “Most have had a heavily applied orientation”, adding that “For the most part they have been invented and developed in response to social and public policy concerns.”<sup>32</sup> Given that the social sciences have been concerned with American “society, economy, polity and culture” and have “played an enormous role in science, society, and culture for much of American history” the “traditional disciplinary focus” of historical inquiry needs to be supplemented by the incorporation of “broader perspectives”. This is not to deny that the various social sciences have been “sciences, disciplines, and professions” but it is to insist that “much more has been involved”

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<sup>31</sup> *Ibid.*, pp. 323 and 326

<sup>32</sup> Hamilton Cravens, ‘History of the Social Sciences’, *Osiris*, 2nd series, 1985, 1, p. 185.

than a narrow disciplinary focus might reveal.<sup>33</sup> Even more helpful than a “broad definition” of what disciplines have been among the social sciences “would be many more works than currently exist that transcend disciplinary boundaries and focus on larger problems in science, society, and culture.”<sup>34</sup> While there is no doubt, says Craven, that in the field of the history of the social sciences the “issues of disciplinary history” are important, nevertheless they are “secondary to larger areas of inquiry, such as the invention and use of knowledge in society and culture and the constituencies and merchandisers of such knowledge.”<sup>35</sup>

Strategic studies was typical of the social sciences in the United States. It developed in response to the concerns of public policy, in spite of its “abstractness” had a heavily applied orientation (even if that orientation did not register as a major influence on policy) and by popularising the notion of “deterrence”, played a significant role in American society and culture of the Cold War—although this role was not quite the one sought by the theorists. Craven is certainly correct to emphasise the importance of these factors for histories of disciplines in the social sciences. However, the point that is missed by him is that what he calls “larger areas of inquiry” are just as much issues of disciplinary history as are the narrower concerns of a “traditional disciplinary focus”.

The concerns of traditional disciplinary history include “founding figures”, “fundamental innovations”, the emergence of canonical texts, and so on. This sort of history seeks answers to such questions as: “When and how does a specialised discipline come coherently together?” and “Does the process mark a cumulative development of pre-existing elements, or else mark a definite break with what went before?” In short, traditional disciplinary history focuses on the “inside” of a discipline. It is concerned with the formation of a discipline’s identity and with specifying the combination of “subject-matter, methods, techniques and theories” peculiar to it and which therefore marks it off from others.<sup>36</sup>

Traditional disciplinary history has been supplemented by an approach in which disciplines are regarded as social structures. This is a sociological approach which seeks to identify, describe and explain characteristics of a discipline such as the “distribution of research interests and characteristic differences in work organization, publication practices, and competition through the field.”<sup>37</sup> While this approach presents what is essentially a “snapshot” of the state of a discipline at any one time, its historical counterpart seeks to provide an account of the development of these characteristics over a period of time. What is common to the traditional approach to disciplinary history and an approach which regards a discipline as either a static or developing social structure is their micro-level analysis, the disregard for the “outside” or wider social and political context of a discipline, that is, Craven’s “larger areas of inquiry”. But Craven’s approach is equally unsatisfactory, for he is not interested in the “inside” of a discipline. But what is a discipline without founding figures, canonical texts, research interests, publications, and competition and collaboration among its practitioners?

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<sup>33</sup> *Ibid.*, p. 183. Craven complains that “Works on anthropology, psychology, and sociology so dominate the field [of the history of the social sciences] that it is difficult to find work on such staple social sciences as economics, geography, and political science, not to mention such lesser-known applied social sciences as home economics, urban planning, industrial engineering, agricultural economics, child development, industrial engineering, rural sociology, public administration, or agricultural engineering.” *Ibid.* It should also be said that strategic studies *qua* applied social science or policy science has not as yet received the attention of historians and sociologists of the social sciences it deserves.

<sup>34</sup> *Ibid.*, p. 185.

<sup>35</sup> *Ibid.*, p. 186.

<sup>36</sup> J.R.R. Christie, ‘The Development of the Historiography of Science’ in R.C. Olby, G.N. Cantor, J.R.R. Christie and M.J.S. Hodge (eds.), *Companion to the History of Modern Science*, Routledge, London and New York, 1990, pp. 11 and 12.

<sup>37</sup> Stuart S. Blume and Ruth Sinclair, ‘Aspects of the Structure of a Scientific Discipline’ in Richard Whitley (ed.), *Social Processes of Scientific Development*, Routledge and Kegan Paul, London and Boston, 1974, p. 224

Neither the traditional approach nor an approach which regards disciplines as social structures can account for how the inside of a discipline, and a discipline as such, actually connects and interacts with the wider social and political context. Moreover, they do not explain how events, institutions and processes in the wider context bring about the emergence and formation of a discipline and help to mould its internal characteristics and their ongoing development. By using and articulating the notions of “paradigm” and “*doxa*” in combination, it becomes possible to understand how strategic studies connected and interacted with events, groups and institutions in its wider social and political context and to explain how these factors brought about the emergence of systematised and formalised deterrence theory and its subsequent development.

The deterrence paradigm of the strategic theorists stood between the *doxa* and the theory of nuclear deterrence, just as the policy makers’ and military planners’ conceptual framework of pre-emption stood between the *doxa* and the nuclear strategy which they formulated. “Stood between” is a shorthand locution which is to be understood as meaning that the paradigm was at the intersection of the theorists’ beliefs and assumptions about nuclear weapons and their proper purpose and the cultural assumptions and presuppositions of the *doxa*. The conceptual framework was at a similar point of intersection for the policy makers and planners. The cultural assumptions of the *doxa* were common to the theorists, policy makers and planners. But what, beyond that which has already been said, is meant by “paradigm” and “*doxa*”?

While in the first edition of *The Structure of Scientific Revolutions* Kuhn tended unhelpfully to use “paradigm” in quite a number of different ways<sup>38</sup>, for the purposes of this paper it is probably best to try to follow Kuhn’s lead in the ‘Postscript’ to the second edition and to think of “paradigm” primarily in two different senses. In the first sense, “it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given [scientific] community” and in the second “it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science.”<sup>39</sup> The first sense of the term is much nearer that which will be employed here than the latter. This is because, it can help to explain the importance of the shared conceptual, analytical and broadly ideological commitments of the strategic theorists. However, as will become clearer in the following section, the influence of the strategic theorists’ ideological commitments on the emergence and development of strategic studies can only be fully understood with reference to the *doxa*.

In the ‘Postscript’, Kuhn pointed out that, in his original text a paradigm or set of paradigms was treated as the object that accounted for the “relative fullness” of the “professional communication” and the “relative unanimity” of the “professional judgments” of a scientific community.<sup>40</sup> This, as he noted in ‘Reflections on my Critics’, was Masterman’s sociological sense of ‘paradigm’.<sup>41</sup> However, he now thought that it was “inappropriate” to use ‘paradigm’ in this way. He also thought that ‘theory’ was not an appropriate substitute. Kuhn did acknowledge that the scientists who belonged to a “particular community” would probably say that it was a theory or set of theories which enabled them not only to communicate effectively with each other but also to reach a consensus in their professional judgements. This notwithstanding, according to Kuhn the then current use of the term ‘theory’ in the philosophy of science connoted a “structure

<sup>38</sup> Margaret Masterman found not less than twenty-one senses of “paradigm” used in *The Structure of Scientific Revolutions*, although she groups them under three categories—“metaphysical paradigms” or “metaparadigms”, “sociological paradigms” and “artefact” or “construct paradigms”. See her ‘The Nature of a Paradigm’ in Imre Lakatos and Alan Musgrave (eds.), *Criticism and the Growth of Knowledge*, Cambridge University Press, London and New York, 1978 (first published 1970), pp. 61-66. See also in the same volume Kuhn, ‘Reflections on My Critics’, especially p. 271. Kuhn corroborates Masterman’s findings and endorses her categorisation.

<sup>39</sup> Kuhn, *Structure of Scientific Revolutions, op. cit.*, p. 175. Briefly, the enterprise of “normal science” can be considered as “an attempt to force nature into the preformed and relatively inflexible box that the paradigm supplies.” *Ibid.*, p. 24.

<sup>40</sup> *Ibid.*, p. 182.

<sup>41</sup> Kuhn, ‘Reflections on My Critics’, *op. cit.*, p. 271.

far more limited in nature and scope than the one required here.”<sup>42</sup> ‘Theory’ was thus ruled out. With ‘paradigm’ and ‘theory’ both rejected as inadequate, another term was required to denote what it was that the members of a scientific community shared, what formed them into a scientific community. Kuhn suggested for this purpose “disciplinary matrix”, and there were a number of reasons for this: “‘disciplinary’, because it is common to the practitioners of a specified discipline; ‘matrix’ because it consists of ordered elements which require individual specification”. He observed that the “objects of group commitment” which he had formerly described as “paradigms, parts of paradigms, or paradigmatic” were all components of the disciplinary matrix and were no longer to be considered all of a piece as they had been in his original text. The constituent elements of a disciplinary matrix included “symbolic generalizations”, “metaphysical paradigms” or the “metaphysical parts of paradigms”, “values”, and “exemplars”.<sup>43</sup>

With regard to symbolic generalizations, Kuhn had in mind, amongst other things, such expressions as “elements combine in constant proportion by weight” or “for every action there is an equal and opposite reaction”.<sup>44</sup> According to Kuhn, the general acceptance of such expressions by the members of a scientific community gave them points on which they could focus and “attach the powerful techniques of logical and mathematical manipulation in their puzzle-solving activity.”<sup>45</sup> The interactions and interrelations of the entities studied by the social and policy sciences cannot be so readily expressed in such succinct, symbolic generalisations. Economics is a possible exception to this general rule, but the connection between the categories of economic theory and economic “realities” is at best uncertain. Nevertheless, because strategic studies was in effect a vehicle for the application of the assumptions of classical economic theory and the quantitative techniques of neo-classical economic theory to the analysis of nuclear strategy (*via* systems analysis and game theory), the relationship between strategic studies and the discipline of economics will be considered more fully below.

As for exemplars, Kuhn described these as “concrete problem-solutions that students encounter from the start of their scientific education, whether in laboratories, on examinations, or at the ends of chapters in science texts.”<sup>46</sup> They also include “technical problem-solutions” found in the academic journals of a discipline that its practitioners consult and contribute to after their formal education has been completed. Again with the possible exception of economics, there are no obvious analogues or counterparts of exemplars in the social and policy sciences. Furthermore, canonical texts, standard training regimes based on them, and accredited schools for the training of novices or students are phenomena and institutions that characterise mature disciplinary fields. If this is so, then strategic studies was only in its infancy (or early childhood) even at the end of the 1950s, for it lacked all of these things.<sup>47</sup>

Exemplars are important for Kuhn, for he regarded them, or the differences among sets of them, as being the basis of the “community fine-structure of science”. He observed that, while all physics students begin their training by learning the same exemplars, in later years “the symbolic generalizations they share are increasingly illustrated by different exemplars.” In other words, as they receive more training so do physics students become more specialised. The “cover-all” exemplars of the early years of training do not satisfy the requirements of the more advanced, increasingly specialized courses undertaken by students in the later years.<sup>48</sup>

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<sup>42</sup> Kuhn, *Structure*, *op. cit.*, p. 182.

<sup>43</sup> *Ibid.*

<sup>44</sup> Kuhn, *op. cit.*, p. 183.

<sup>45</sup> *Ibid.*

<sup>46</sup> *Ibid.*, p. 187.

<sup>47</sup> Writing in 1982, Colin Gray points out that “Strategic studies continues to lack a truly secure place in American university curricula.” Gray, *op. cit.*, p. ix. A self-professed “university teacher” (in Britain and Canada) and “think-tank analyst” (in the United States), Gray insists that he “has been opposed to the creation of departments of strategic studies in universities for a long time.” *Ibid.*, note 30 p. 13; p. 165.

<sup>48</sup> Kuhn, *Structure of Scientific Revolutions*, *op. cit.*, p. 187.

It is the lack of obvious symbolic generalisations and exemplars in strategic studies—even if perhaps Wohlstetter's R-266 came close to being or playing the role of a Kuhnian exemplar—which suggests that the basis of the community fine-structure of the discipline must be sought in the values and metaphysical parts of paradigms shared by the strategic theorists. This is so even though economics, which strategic studies emulated, was a mature discipline possessing its own symbolic generalisations and exemplars, what Kuhn describes as concrete or technical problem-solutions.

Values are an important object of group commitment. However, they are generally not the exclusive preserve of one community of scientists, but are shared among different communities. On this score, Kuhn commented that values “do much to provide a sense of community to natural scientists as a whole.”<sup>49</sup> Values are important at all times but particularly so, according to Kuhn, when a scientific community is in crisis and its members have to decide between ways of practising the discipline that are not compatible with each other. Accordingly, his discussion of values highlights the role they play when the members of a scientific community have to decide between competing theories. In these circumstances, simplicity, self-consistency, plausibility, and compatibility with other theories in use at the time are some of the values or criteria by which theories might be judged.<sup>50</sup> More to the point as far as strategic studies is concerned, Kuhn also notes that in the natural sciences the “most deeply held values concern predictions: they should be accurate; quantitative predictions are preferable to qualitative ones; whatever the margin of permissible error, it should be consistently satisfied in a given field; and so on.”<sup>51</sup>

As seen above, in the formative years of strategic studies if one value came to be deeply held among the strategic theorists it was that quantitative strategic analysis was preferable to qualitative strategic analysis, and especially preferable to the use of the irrational and unscientific methods favoured by the military profession. It has also been noted that Wohlstetter's study of the selection and use of strategic air bases did much to establish the conviction among the strategic theorists at RAND that sound strategic analysis involved the use of the quantitative methods of economics. These methods were used both to measure the cost and effectiveness of competing weapons systems in terms of their vulnerability to, and therefore their retaliatory capability after, a “warningless” Soviet attack (systems analysis) and to measure and quantify the utilities of the opposing sides in order to calculate the payoffs of their respective preferences with a view to achieving a stable “balance of terror” or state of “mutual deterrence” (game theory).

When he spoke of “metaphysical parts” (“metaphysical paradigms”) Kuhn had in mind “shared commitments” to beliefs in particular models, either of an ontological or heuristic sort. As ever, the illustrations of this point were drawn from the natural and physical sciences. Ontological models include “heat is the kinetic energy of the constituent parts of bodies” and “perceptible phenomena” are due to “matter and force, or to fields”. Ontological models, then, say something about the entities and forces that make up the world and how forces cause entities to interact. Kuhn cited as examples of models of the heuristic variety: “the electric circuit may be regarded as a steady-state hydrodynamic system; the molecules of a gas behave like tiny elastic billiard balls in random motion.”<sup>52</sup> Such models are heuristic because they aid in investigation and research by giving a clue as to how objects or phenomena are to be apprehended, comprehended and explained. Whether of the ontological or heuristic variety, models provide a scientific community with acceptable “analogies and metaphors”. The models assist a scientific community in deciding between acceptable and unacceptable explanations and “puzzle-solutions” and help the community in determining the “roster of unsolved puzzles and in the evaluation of the importance of each.”<sup>53</sup>

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<sup>49</sup> *Ibid.*, p. 184.

<sup>50</sup> *Ibid.*, p. 185.

<sup>51</sup> *Ibid.*

<sup>52</sup> *Ibid.*, p. 184.

<sup>53</sup> *Ibid.*

Of the four components of the disciplinary matrix discussed by Kuhn—symbolic generalizations, exemplars, metaphysical parts, and values—it is the latter two which are of most interest and relevance in an analysis of the origins and development of strategic studies. This is because, as has been seen above, strategic studies in its infancy had not produced its own symbolic generalizations and exemplars. They therefore were not among the immediate objects of group commitment and identity among the members of the community of strategic theorists during these early years. It follows, then, that group commitment must have developed as the result of something like common values, metaphysical considerations and the like. However, while metaphysical beliefs or assumptions may ultimately bind together the practitioners of the physical and natural sciences, it is difficult to see how in the social and policy sciences, and especially in a discipline like strategic studies, purely *metaphysical* beliefs or assumptions or commitments could have played a part in fostering group commitment. Given that strategic studies came into being as the Cold War intensified and the nuclear arms race was getting into top gear, it may be more profitable to search for ideological beliefs and assumptions to account for the emergence and development of the community of strategic theorists in the United States during the late 1940s and through the 1950s. It is certainly true that Kuhn identified important sources of group identity and commitment in the natural and physical sciences. But in order to get to the sources of the strategic theorists' group identity and commitment, Kuhn's discussion of shared commitments to beliefs has to be extended beyond the binding powers of ontological and heuristic models. This is where Pierre Bourdieu's notion of "*doxa*" can be extremely useful.

Bourdieu, in his own inimitable fashion, introduces "*doxa*" in the course of his discussion of the "collective belief" in science. Science, says Bourdieu, has no basis other than this collective belief "which is produced and presupposed by the very operation of the scientific field."<sup>54</sup> Whether or not it is possible to speak of the *doxa* of the scientific field taken as a whole as Bourdieu appears to suggest, the notion can be used to account for the collective belief of the members of a particular scientific field. This is certainly true of strategic studies. The *doxa* refers to the "aggregate of presuppositions" which the members of a particular scientific field take to be "self-evident and outside the area of argument because they constitute the tacit condition of argument".<sup>55</sup> Put more simply, the *doxa* is made up of "unthought assumptions".<sup>56</sup> It is the source of collective belief within the scientific field. Orthodox and heterodox positions within the field are defined in terms of the *doxa*. Furthermore, Bourdieu asserts that the *doxa*:

...is constitutive of the very functioning of the field, and...bears on the totality of what is admitted by the mere fact of belonging to the field, and on the totality of what is set beyond discussion by the mere fact that the agents accept the issues at stake in the argument, i.e. the consensus on the objects of dissensus, the common interests underlying conflicts of interests, all the undiscussed and unthought areas tacitly kept outside the limits of the struggle.<sup>57</sup>

Bourdieu's idea that the *doxa* is made up of unthought assumptions and presuppositions which are regarded as being self-evident and outside the area of argument, is a very suggestive one. It clearly complements and extends Kuhn's notion of the "metaphysical parts" of the disciplinary matrix. For in accounting for group commitment it goes beyond shared belief in ontological and heuristic models. It might even be said that belief in these models is possible only because the *doxa* is already in place.<sup>58</sup>

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<sup>54</sup> Bourdieu, *op. cit.*, p. 34.

<sup>55</sup> *Ibid.*

<sup>56</sup> *Ibid.*, p. 37.

<sup>57</sup> *Ibid.*, p. 34.

<sup>58</sup> In this regard, even Kuhn had to acknowledge that, in the early stages of a science's development, "Only very occasionally, as in the cases of statics, dynamics, and geometrical optics, do facts collected with so little guidance from pre-established theory speak with sufficient clarity to permit the emergence of a first paradigm." He also pointed out that "No natural history [that is, a collection of facts compiled without the guidance of pre-existing theory, such as, for example, the "Baconian 'histories' of heat, color, wind, mining, and so on"] can be interpreted in the absence of

Strategic studies is particularly illustrative of this point. It came into existence as the Cold War was intensifying and as the urgency and anxiety surrounding the atomic bomb and its uses in this conflict were heightening accordingly. In these circumstances, belief in abstract theoretical and mathematical models could hardly have served as the basis of group identity and commitment. Something much more ideologically and emotionally forceful was required. It was in these areas that the *doxa* of the deterrence paradigm was strongest. However, as already noted, the *doxa* was not the exclusive preserve or possession of the strategic theorists, it was also the preserve of the policy makers and military planners. Indeed, its assumptions and presuppositions was constitutive of the society and culture to which the powerful business, political, military and academic elites which dominated American society all belonged.

### **The deterrence paradigm, the conceptual framework of pre-emption, and the *doxa***

It is a relatively easy undertaking to assemble the constituent elements of the deterrence paradigm, for they have all been identified above. The foundational constituent element of the deterrence paradigm was an assumption that contained two inextricably linked components: the belief that nuclear weapons were qualitatively different from other types of weapons, even the so-called “conventional” high explosive and incendiary bombs of the kind that had been used to such devastating effect by the US Army Air Forces in World War II; closely related to this was the belief that the sole purpose of nuclear weapons was “deterrence, that their role, as Brodie had stipulated, was to avert general war, “conventional” or nuclear, between the United States and the Soviet Union: It followed logically from the second of these founding beliefs that the deterrent purpose of nuclear weapons could only be fulfilled if their retaliatory *capability* were protected from destruction in an enemy pre-emptive strike. If vulnerable, this capability would be completely effaced. The protection of the retaliatory capability of nuclear weapons would obviate their retaliatory use or, indeed, any use of nuclear weapons by either the United States or the Soviet Union. Thus, “deterrence” and “vulnerability” became twin concepts in the deterrence paradigm. As the paradigm matured in the 1950s, strategic stability came to be equated with a “balance of terror” (Wohlstetter’s concept) or “mutual deterrence” (Schelling’s).

Game theory also developed into a central element of the deterrence paradigm. Ever since Brodie had first laid his ideas on the military and strategic implications of nuclear weapons down on paper, a tendency to view nuclear strategy, and nuclear war itself, as a high-stakes, tit-for-tat game of threat and counter-threat had developed among the strategic theorists. Game theory, especially in the hands of Schelling, formalised this view and made it much more of an abstraction. The view that sound strategic analysis consisted in the use of game theory and systems analysis, and the quantitative methods of economics they incorporated, was another constitutive element of the paradigm. As already seen, the work of Wohlstetter was instrumental in establishing this as a constituent element of the deterrence paradigm.

Unlike the theorists, the planners and policy makers saw no qualitative distinction between nuclear and conventional weapons, for them there was only a quantitative difference of efficiency and effectiveness. As noted above, the planners in particular came to the view that the only way to “deter” a Soviet pre-emptive strike was to pre-empt it, that is, to get in the first blow. This drew on one important element of the modern American military tradition, the conviction that the United States should never lose the initiative to the enemy and, accordingly, that it should always strive to get in the first blow. The conceptual framework of pre-emption drew on other important elements of this tradition, elements which were embodied in the American plans for nuclear war with the Soviet Union. These were the belief in the military efficacy of overwhelming force; the conviction that in war the strategic objective of the United States was to bring about the wholesale destruction of the enemy’s armed forces and society; and the view that war should be waged until the enemy submitted to unconditional surrender. These assumptions and beliefs were based on the confident expectation, grounded in the experience of the American Civil War and the First and

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at least some implicit body of intertwined theoretical and methodological belief that permits selection, evaluation, and criticism. If that body of belief is not already implicit in the collections of facts—in which case more than ‘mere facts’ are at hand—it *must be externally supplied, perhaps by a current metaphysic, by another science, or by personal and historical accident.*” Kuhn, *Structure, op. cit.*, pp. 16-17; emphasis added.

Second World Wars, that the enormous productive capacity of the United States would enable it to wage war with such overwhelming force that it would be able to annihilate any enemy.<sup>59</sup>

The *doxa* was a collection of more fundamental ideological assumptions and presuppositions than were contained either in the paradigm or conceptual framework. The assumptions and presuppositions of the *doxa* were mostly unthought and largely unarticulated and therefore beyond argument or critical scrutiny. These were assumptions and so on not only about the place and role of the United States in the world, but also about the place and role of the Soviet Union and the reasons for the US-Soviet enmity. The *doxa* portrayed the United States as the hero, and the Soviet Union as the villain, of the post War world. According to the *doxa*, moreover, the history of the United States had uniquely prepared it for leadership of the capitalist world-economy and the free world, just as the history of the Soviet Union had set it on the course of totalitarianism, expansionism and tyranny.

The *doxa* is not for all its pervasiveness and influence resistant to straightforward exposition even though straightforward expositions of it are few in number.<sup>60</sup> It is made up of a relatively few, simple beliefs and assumptions. The Soviet Union's inexorably aggressive and expansionist propensities were believed to be the product of the avowedly Marxist and Leninist ideology of its leadership, the totalitarian nature of its state and socio-political structure, and the legacy of repression and tyranny left by the tsars. There were also believed to be longstanding and irresistible geopolitical imperatives acting on the Russian state which, when combined with these other factors, led irresistibly in the direction of external aggression and a compulsive expansionism. Basically, according to this view of the Soviet Union, the deep suspicions and pathological hostility which the leaders of the Soviet Communist Party manifested toward any states and nations that did not willingly submit to the Soviet credo forced them to seek to dominate and rigidly control their external environment in much the same way as they brutally repressed dissent at home.<sup>61</sup> The assumption that a state's internal structure and behaviour determined the tenor of its actions in the "outside" world was a central element of the *doxa*. As was the case with the Soviet Union, the external behaviour of which had internal sources and

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<sup>59</sup> For more on the modern American military tradition and "the American way of war" see, for example, Russell F. Weigley: *The American Way of War: A History of United States Military Strategy and Policy*, Macmillan Publishing Co., New York, Collier Macmillan Publishers, New York, 1973, 'American Strategy from its Beginnings through the First World War' in Peter Paret (ed.), *Makers of Modern Strategy: From Machiavelli to the Nuclear Age*, Clarendon Press, Oxford, 1986, pp. 408-443, *History of the United States Army* (enlarged edn.), Indiana University Press, Bloomington, 1984 and, 'The Legacy of World War II for American Conventional Military Strategy: Should we Escape It?' in Warren F. Kimball (ed.), *America Unbound: World War II and the Making of a Superpower*, St Martin's Press, New York, 1992, pp. 73-93; Warren F. Kimball, 'U.S. Economic Strategy in World War II: Wartime Goals, Peacetime Plans' in Kimball (ed.), *America Unbound*, pp. 139-157; James M. McPherson, 'Lincoln and the Strategy of Unconditional Surrender' in James M. McPherson, *Abraham Lincoln and the Second American Revolution*, Oxford University Press, New York and Oxford, 1991, pp. 65-91; Michael Howard, 'The Forgotten Dimensions of Strategy' in Michael Howard, *The Causes of Wars and other Essays*, Counterpoint (Unwin Paperbacks), London 1983 [*Foreign Affairs*, Summer 1979]; Maurice Matloff, 'Allied Strategy in Europe, 1939-1945' in Peter Paret (ed.), *Makers of Modern Strategy*, pp. 677-702; Michael S. Sherry, *The Rise of American Air Power: The Creation of Armageddon*, Yale University Press, New Haven and London, 1987; David MacIsaac, *Strategic Bombing in World War Two: The Story of the United States Strategic Bombing Survey*, Garland, New York, 1976

<sup>60</sup> Indeed, there is no single, authoritative source to consult for details of the *doxa*. However, arguably NSC 68 (a joint State/Defense Department document titled 'United States Objectives and Programs for National Security', April 1950) is the most comprehensive, authoritative and influential enunciation of the mature *doxa* ever produced.

<sup>61</sup> Perhaps the clearest and most authoritative expression of that side of the *doxa* dealing with the Soviet Union is to be found in "Mr X" [George Kennan], 'The Sources of Soviet Conduct', *Foreign Affairs*, XXV, no. 4 (July, 1947), pp. 566-82. Kennan was the chief architect of the policy of containment and did much to inculcate in American policy making and military planning circles the view that a state's internal structure governed its external behaviour.

causes, so also with the United States. Hence, the nature of American political, social and economic institutions, of American society and people, supposedly determined the objectives and *modus operandi* of American foreign policy. Aggressiveness and expansionism were not the American way. This introduces the other side of the metaphysics or *doxa* of the deterrence paradigm and conceptual framework of pre-emption.

The United States was everything that the Soviet Union was not. To begin with, the United States did not look upon its external environment as something that had to be controlled and dominated. Democratic political institutions and respect for the rights of the individual at home were believed to be a moderating and restraining influence on the behaviour of the American state abroad. There were believed to be no geopolitical imperatives driving the United States to expansion and conquest. American history had prepared it well for its post War role of leader of the “free world”. After all, the United States had fought a revolutionary war against an old European colonial power in order to assert and establish its independence, endured a sanguinary civil war to abolish the institution of slavery, and participated in two world wars in order to crush German imperialism, European fascism and Japanese militarism.<sup>62</sup> Just as the American state and government were supposed to promote and robustly defend individual rights and democratic institutions, practices and freedoms for the American citizen, so the United States would it was believed support and vigorously defend the right to independence and self-determination of nations and peoples, unable to defend themselves, that were facing conquest, tyranny and despotism from external aggressors or straining under the yoke of colonial rule. Finally, the doctrine of free enterprise was believed or supposed to have unencumbered sway within the United States.

The American state sought not to interfere with the privacy and freedom of manoeuvre of capitalist enterprises, intervening in the economy merely to establish and maintain the conditions that were propitious for free enterprise. Similarly, in the outside world, the US government was supposed to be the primary sponsor of unfettered international trade and an open capitalist world-economy. The United States was also believed to be the main opponent of protectionism and economic nationalism, which, it was thought, had threatened the existence of the world-economy in the inter-War period and led to the rise of totalitarian states. An important corollary of this belief or supposition was the conviction that an open and unrestricted world economy was the surest guarantee of international peace. In effect, little if any distinction was drawn between freedom and free enterprise. There was, then, in the *doxa* of the deterrence paradigm a neat symmetry between the political and economic conditions that obtained within the United States and the sort of world which it sought to create and nurture outside. Beyond the frequent admissions that the Soviet Union was the enemy and aggressor, however, most of the other elements of the *doxa* of the paradigm remained hidden, undisclosed and beyond the critical or even detached scrutiny of the strategic theorists. Indeed, this is the very idea that the word “*doxa*” is meant to convey. The *doxa* is made up of unquestioned and unquestionable assumptions.

While the *doxa* remained almost wholly beneath the surface, it was the *sine qua non* of the theory of nuclear deterrence.<sup>63</sup> The whole idea that “deterrence” was the sole purpose of nuclear weapons

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<sup>62</sup> The doctrine or idea of Manifest Destiny, which first appeared in the mid-Nineteenth Century, was an embryonic version of the *doxa*. Briefly, the idea of Manifest Destiny held that the economic success and superiority of the United States derived from and indeed proved the moral and racial superiority of its white, Protestant inhabitants and the civilisation they had built. It followed from this that the American nation, thus narrowly defined, was unusually suited to playing a leadership role in world affairs. The mature *doxa* emerged fully formed after the Second World War.

<sup>63</sup> Bourdieu’s notion of the “cultural unconscious” complements that of the “*doxa*”. He introduces the term in a discussion of the sociology of intellectual and artistic creation. According to Bourdieu, “it is by the extent to which he forms part of an intellectual field by reference to which his [the artist’s or intellectual’s] creative project is defined and constituted, by the extent to which he is, as it were, the contemporary of those with whom he wishes to communicate and whom he addresses through his work, referring implicitly to a whole code he shares with them—themes and problems of the moment, methods of argument, manners of perception, etc.—that the intellectual is socially and historically situated. His most conscious intellectual and artistic choices are always directed by his own culture and taste, which are themselves *interiorizations of the objective culture of a*

was founded upon the elements that made up the *doxa*. The theory of nuclear deterrence had any relevance and legitimacy only in so far as the elements of the *doxa*, and the *doxa* itself, retained their currency and legitimacy. Similarly, the idea that nuclear weapons were to be used pre-emptively was supported by the *doxa* even though it specifically sanctioned neither “deterrence” as understood by the theorists nor pre-emptive use of nuclear weapons. The view of the place and role in the world of the United States and the view of the Soviet Union that were central to the *doxa* and which lay beneath the deterrence paradigm and the intellectual paradigm, were mutually reinforcing. The *doxa* was thus highly resistant to change and erosion and this ensured the continued relevance and legitimacy of deterrence theory and strategic studies even though they were out of step with the conceptual framework.

In Bourdieuan terms, strategic studies evinced a low degree of autonomy with respect to “external determinations”.<sup>64</sup> It was established under Air Force auspices and its continued existence was highly dependent on Air Force support and funding, and, therefore, on the extent to which it was, and was seen to be, responsive to the demands of the Air Force. The processes of the systematisation and formalisation of deterrence theory in RAND, were only made possible by Air Force, and SAC, patronage. This accounts for the extent to which “social arbitrariness” figured in the “system of presuppositions” which constituted strategic studies.<sup>65</sup> Put another way, the constitutive presuppositions of the *doxa* of strategic studies expressed the fundamental interests of the Air Force and, indirectly by virtue of the *doxa*, the other elites and groups which dominated American society. This was so even though the paradigm of the strategic theorists and the conceptual framework of the military planners and policy makers were considerably at odds with one another over what the proper purpose of nuclear weapons was or should be.

This raises the question of why the Air Force established the RAND Corporation in the first place and then continued to fund the research conducted by the strategic theorists who worked there. The setting up of RAND and the funding of the research projects carried out under its auspices had a lot to do with the kudos earned by the Air Force for its employment of scientists, who were drawn from the “hard” sciences *and* the social sciences (in particular, economics), to analyse and seek innovative solutions to the compelling problems of national security. To be sure, the work of the natural and physical scientists and engineers would be of far more immediate relevance and value to the Air Force than that of the economists and social scientists. Nevertheless, the Air Force’s employment of economists and social scientists to study and analyse problems that were regarded as being of vital concern to the national security of the United States would reflect favourably on it. The Air Force, and SAC in particular, was after all reputedly the service on whose existence and performance the national security of the United States most keenly depended.

The employment of scientists of all types (physical, natural, social), evidently to assist it in improving its performance of this mission on which national survival hinged, gave the impression that the Air Force was a progressive organisation which was open and receptive to new technologies and ideas that had been thrown up by cutting-edge research broadly in the area of air warfare. The use of experts from fields outside of the “hard” sciences was, of course, not unprecedented. The civilian analysts who staffed the US Army Air Forces’ Committee of Operations Analysts during the Second World War were lawyers and other professionals mostly with an economics, managerial or accountancy background, not “hard” scientists at all. Their

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*particular society, age or class.* [The objective culture]...constitutes the necessary precondition for the concrete fulfilment of an artistic intention in a work of art, in the same that language as the ‘common treasury’ is the precondition for the formulating of the most individual word.” Pierre Bourdieu, ‘Intellectual Field and Creative Project’ in Michael F.D. Young (ed.), *Knowledge and Control: New Directions for the Sociology of Education*, Collier-Macmillan Publishers, London, 1971, p. 180; emphasis added. The *doxa*, or cultural unconscious, of the deterrence paradigm and conceptual framework of pre-emption interiorized the objective culture to which the strategic theorists, military planners and policy makers all belonged.

<sup>64</sup> Bourdieu, ‘The Specificity of the Scientific Field’, *op. cit.*, p. 34.

<sup>65</sup> *Ibid.*

contribution amounted to little more than offering a “quasi-scientific” rationalisation and justification for what the bombing commanders were already planning to do.<sup>66</sup>

Likewise, the Air Force and SAC made no commitment or pledge to accept the advice that the RAND theorists offered. This applied in particular to the economists who could offer little in the way of assistance on the “nuts and bolts” of bombers, bombs and their support systems. This puts the frustration experienced by Wohlstetter into perspective. It also casts light on Brodie’s observation in ‘The Development of Nuclear Strategy’ that the military (obviously he had the Air Force very much in mind) was strictly a customer, and a very selective one at that, of the ideas of the civilian strategic theorists.<sup>67</sup>

### **Strategic studies, economics and “the gap” between the deterrence paradigm and the conceptual framework of pre-emption**

As a vehicle for the application of the assumptions, models and techniques of economics to the analysis of nuclear strategy, strategic studies obviously drew heavily on economic concepts, methods and techniques. Any scholarly and disciplinary integrity which strategic studies acquired, therefore, reflected the status and integrity which economics had been able to achieve for itself over many years. In this sense, strategic studies was parasitic on economics, especially that part of it which relied on quantitative methods. The enthusiasm with which the strategic theorists latched onto economics cannot simply be put down to whim or caprice. For qualitative strategic analysis was much less likely than quantitative, scientific analysis to attract the attention and support of the Air Force and this notwithstanding the accusations of the strategic theorists that their uniformed counterparts were tradition-bound, unscientific and irrational. “Hard data” were required—even if, as in the case of Wohlstetter’s bases study, these were derived from self-serving and compromised Air Force intelligence estimates—not historically and culturally informed, that is “woolly”, judgements.

In this respect, Pierre Bourdieu’s critique of what he calls “official sociology” in the United States has important resonances for strategic studies. “The existence of more advanced sciences”, says Bourdieu, “is what enables official sociology to furnish itself with all the appearances of scientificity.” These more advanced sciences supply official sociology “not only with methods and techniques, which are generally made use of outside the technical and social conditions of their applications, but also [with] examples.” By furnishing itself with the appearance of scientificity, official sociology seeks to “realise an official image of science” not to “realise itself as a science” as such.<sup>68</sup>

Similarly, strategic studies sought to furnish itself with the appearance of scientificity by taking on board the methods and techniques of economics and applying these to the analysis of situations and contexts that, on a common sense view, seemed to be well outside of the economic domain. In other words, based on the assumption that economic behaviour and non-economic behaviour were both guided by rational decision-making processes and the quest for utility maximisation, economic concepts, methods and techniques were employed in the analysis and explanation of behaviour in contexts far removed from the economic domain. This was particularly evident in the case of game theory, specifically its use in the analysis of deterrence situations which were presumed by the theorists to be of the type in which the participants had both conflicting and common interests and therefore to elicit the same kind of behaviour as that observed in product markets.<sup>69</sup> But beyond these observations, what precisely does it mean to speak of strategic

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<sup>66</sup> Sherry, *Creation of Armageddon*, *op. cit.*, pp. 233-235. For more on the Committee of Operations Analysts, see Ronald Schaffer, *Wings of Judgment: American Bombing in World War II*, Oxford University Press, New York and Oxford, 1985, especially pp. 162-164

<sup>67</sup> Brodie, ‘The Development of Nuclear Strategy’, *op. cit.*, p. 67

<sup>68</sup> Bourdieu, ‘Specificity of the Scientific Field’, *op. cit.*, p. 37.

<sup>69</sup> See, for example, Thomas Schelling, ‘The Retarded Science of International Strategy’ in Schelling, *op. cit.*, pp. 3-20. For an interesting and critical analysis of game theory which looks at, amongst other things, its connection to economics, see Philip Mirowski, ‘When Games Grow Deadly Serious: The Military Influence on the Evolution of Game Theory’ in Crauford D.

studies as a vehicle for the application of economic concepts and methods to the analysis of nuclear strategy, in particular, nuclear deterrence?

This question can best be approached by thinking of economics in terms of a “theoretical, analytical core” and an “applied periphery”.<sup>70</sup> Richard Whitley argues that, in some respects, economics is similar to physics.<sup>71</sup> This similarity is particularly evident in the training which economics and physics students undergo. Recruits to physics are subjected to a “dogmatic initiation in a pre-established tradition that [they are] not equipped to evaluate.”<sup>72</sup> Training in physics seeks to produce in the student “closed mental sets” and this is largely achieved by the use of “concrete problem solutions”. Similarly in economics, “economics textbooks present the field as a fixed body of doctrines and ‘laws’ and often give students a number of highly abstract and general ‘problems’ to work through as a means of developing competence.” Such training does not put much store in the “values of accuracy, applicability, and empirical relevance”, rather it promotes the “values of coherence, simplicity, and formalism”. As a consequence, an economics degree says much more about its holder’s competence in solving “artificial analytical problems” than it does the holder’s ability to analyse and explain everyday economic events and phenomena. But the not inconsiderable price paid for this “insistence on analytical coherence”, contends Whitley, “has been the increasing difficulty of using economic theory to explain empirical phenomena.”<sup>73</sup> This suggests that the applied periphery of economics, on which strategic studies was located, is virtually doomed to practical and policy irrelevance.

Not only does economics training itself effectively insulate economic theory from empirical contamination, there are other features of the discipline which push very much in the same direction. Economic theories are not tested in manner that is comparable to what happens in physics and, what is more, economics does not have “standardized means”—“skills, procedures, cognitive objects and materials”<sup>74</sup>—which allow empirical studies to feed back into the “theoretical corpus”. Thus, while the “applied periphery of problem fields” in economics do draw their “methods and concepts” from the theoretical core, they “do not contribute to its modification or improvement”—and this even though there are acknowledged “contradictions and errors” in the core.<sup>75</sup> The difficulties which economic theory faces in explaining empirical phenomena even in the economic realm are magnified out of all proportion when it is applied to non-economic phenomena and behaviour. This is certainly true of strategic studies.

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Goodwin, *Economics and National Security: A History of Their Interaction*, Duke University Press, Durham and London, 1991, especially pp. 233-240.

<sup>70</sup> These terms are borrowed from Richard Whitley, *The Intellectual and Social Organization of the Sciences*, Clarendon Press, Oxford, 1984, p. 185.

<sup>71</sup> The similarity between economics and physics is not incidental or accidental. Leon Walrus, one of the founders of neoclassical economics, hoped to bring about something akin to a Newtonian revolution in economics. Walrus’ influence has been enormous on the subsequent development of economics which has deliberately fashioned itself in the likeness of physics. Comment Herman Daly and John Cobb, “The decision to follow physics was the decision to mathematize. Mathematics can work only which what can be formalized. In economics, this has meant, in practice, what can be measured. Hence the aim of mathematization biases economics towards aspects of the subject matter which can be measured.” Herman E. Daly and John B Cobb, Jr. (with contributions by Clifford W. Cobb), *For the Common Good: Redirecting the Economy toward Community, the Environment, and a Sustainable Future* (Second Ed. Updated and Expanded), Beacon Press, Oxford, 1998, pp. 30 and 31. For similar reasons to those cited by Daly and Cobb in regard to economics, strategic studies was also biased towards the aspects of its subject matter which could be measured.

<sup>72</sup> Kuhn, *The Essential Tension* cited in Whitley, *op. cit.*, p. 184.

<sup>73</sup> *Ibid.*, pp. 184 and 185.

<sup>74</sup> *Ibid.*, p. 165.

<sup>75</sup> *Ibid.*, p. 185. Whitley notes that there is “some scope for theoretical deviance in industrial, labour, and welfare economics”. *Ibid.*, p. 186. However, the highly formalized and standardized training which economics students receive militates against much deviation from the theoretical core even in these areas.

Even though what is considered to be an economic problem, and what is excluded, is “largely determined by the analytical framework of neo-classical economics”, factors external to economics “do effect the range of issues tackled and policy demands lead to extensions of that framework to new areas which were not previously considered appropriate.”<sup>76</sup> The insulation of the analytical core of economics from empirical contaminants and its extension to areas formerly not considered to be appropriate or relevant strongly suggest that the relationship between economic theory and economic policy is to say the least problematic.<sup>77</sup>

The relationship between strategic theory and strategic policy was also highly problematic. As a sort of applied, peripheral subfield of economics, strategic studies suffered even greater irrelevance and inability to analyse, explain and predict empirical phenomena successfully than other subfields which attempted to apply the analytical core to what were formerly regarded as inappropriate or irrelevant areas. For that matter, strategic studies did not, and indeed could not, simply apply the “analytical framework” of economics to strategic topics and problems “by redescribing these in terms of concepts and categories taken from theoretical economics and ‘solving’ them in the way taught in undergraduate textbooks.”<sup>78</sup> Rather, as seen above, applying game theory to deterrence situations was possible only if it were first assumed that economic behaviour and non-economic behaviour were guided by rational decision-making and motivated by the attempt to maximise utilities.

The question remains of how and why economic theory, via game theory, came to be applied to problems of American nuclear strategy. This question relates to the one that was asked above about why, apart from the legitimacy (and funding) conferred by the employment of a quantitative and scientific methodology, the strategic theorists wished to portray strategic studies in the image of economics. It also bears on the question of how the strategic theorists rationalised “the gap” separating strategic theory from practice. These can best be answered by considering some of comments of Brodie and Schelling which directly address the issues bearing on them.

In 1949, Brodie gleefully jumped on the economics bandwagon then rapidly gaining momentum at the RAND Corporation. In an article titled ‘Strategy as a Science’, he complained that the military profession lacked a “genuine understanding of military strategy”.<sup>79</sup> The “development of a theoretical framework” for strategic analysis would, he thought, encourage the development of such an understanding. However, Brodie acknowledged, the “military profession [was] not a scholarly calling” and this had therefore been neglected.<sup>80</sup> Indeed, he commented, it was true that both inside and outside of the armed services “strategy [was] not receiving the scientific treatment it deserve[d]”.<sup>81</sup> For Brodie, this neglect became most apparent when the rapid rate of technological development was compared with the “incredible and sometimes disastrous” failure of tactical and strategic thinking to keep up. He attributed the neglect to the “absence of the habit of scientific thinking” which characterised the military profession.<sup>82</sup> While for Brodie, many of the problems of military strategy were susceptible to analysis based on economic concepts<sup>83</sup>, he was not implying that this or that concept “borrowed from economics could magically resolve the strategic problems which confront us.” Rather, he was arguing that it was in the “field of

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<sup>76</sup> *Ibid.*, pp. 223-224

<sup>77</sup> See, for example, John Lodewijks, ‘Relevance, Scholarship and the Historian of Economics: Some Methodological Considerations’, *Journal of the History of Economic Ideas*, vol. 3, no. 2 (1995), pp. 131-155 for a critical discussion of, amongst other things, the relationship between economics and public policy. Lodewijks also discusses the scholarly and disciplinary integrity of economics.

<sup>78</sup> Whitley, *op. cit.*, p. 185.

<sup>79</sup> Bernard Brodie, ‘Strategy as a Science’, *World Politics*, vol. 1, no. 4 (July 1949), p. 467.

<sup>80</sup> *Ibid.*, pp. 467-468.

<sup>81</sup> *Ibid.*, p. 468.

<sup>82</sup> *Ibid.*, p. 473.

<sup>83</sup> Brodie cites “marginal utility”, the quintessential concept of neoclassical economics, to illustrate his point, *ibid.*, pp. 479-480.

methodology” that economics had the most to offer strategic analysis. Indeed, argued Brodie, strategic analysis required a “genuine analytical method” based on the theoretical framework and methods of the “science of economics”.<sup>84</sup>

Writing at the end of the 1950s, Thomas Schelling in similar vein complained that, while there had been some improvement and refinement of the concept of “deterrence” since it had first appeared on the scene, the process had been far too slow and was as yet incomplete. Deterrence and the concepts associated with it were still “vague” and the theory of deterrence remained “inelegant”.<sup>85</sup> Schelling thought that one of the reasons for this vagueness and inelegance was that, while “deterrence” could be incorporated into the framework of game theory, game theory had not yet fulfilled the promise which it had shown immediately after the Second World War when it was first thought that it could be profitably employed in strategic analysis. However, by far the more important reason (and here his sentiments are similar to those expressed by Brodie in ‘Strategy as a Science’) was that the military profession, unlike other professions such as economics and medicine, had no “identifiable academic counterpart”. Recently established “quasi-governmental institutions” such as RAND had begun to fill the void, but remarked Schelling, this could be “cited as evidence of the need” for an academic counterpart, not that that need had been met.<sup>86</sup> Commented Schelling,

Those who make policy in the fields of economics, medicine, public health, soil conservation, education, or criminal law, can readily identify their scholarly counterpart in the academic world. (In economics the number of trained people who are doing research and writing books compares well with the number engaged in economic policy or administration.) But where is the academic counterpart of the military profession?<sup>87</sup>

Why did the military profession need an academic counterpart? On this score, Schelling argued that the “intellectual skills” required for using and applying force (the traditional activities of the military profession) were different from those needed for threatening the use of force (that is, for fulfilling the deterrent purpose of nuclear weapons). “Deterrence” was “concerned with the exploitation of potential force” and with “persuading a potential enemy that he should in his own interest avoid certain courses of activity.” It followed that a fully developed theory of deterrence would, for all intents and purposes, be a theory of the “skilful *nonuse* of military forces” and therefore required something “broader” than the traditional skills of the military profession.<sup>88</sup>

## Conclusion

“The gap” which divided the strategic theorists’ assumptions about the proper purpose and role of nuclear weapons from those of military planners and policy makers was an extremely important factor in the development of the discipline of strategic studies. This was particularly so after 1949 when the two conceptions of “deterrence” began to move even further apart than they had been at the birth of the theory of nuclear deterrence. Indeed, “the gap”, and the theorists’ acknowledgement of it and estimation of its significance, was of paradigmatic importance for development of the discipline in its formative years.

“The gap” became wider as the theory was systematised and formalised by the application of systems analysis and game theory to the analysis of “deterrence” and its requirements. As the theory was systematised and formalised, so did the strategy it recommended become increasingly suited to the childish game that the theory imagined nuclear war to be. There was no blood involved, not even potentially, because the theorists’ would-be strategy of “deterrence” was intended to obviate any use of nuclear weapons by both sides. Because economics was regarded by the theorists as the model which strategic studies should emulate, and as they systematised and

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<sup>84</sup> *Ibid.*, p. 484.

<sup>85</sup> Schelling, *op. cit.*, p. 7.

<sup>86</sup> *Ibid.*, pp. 8 and 8-9.

<sup>87</sup> *Ibid.*, p. 8.

<sup>88</sup> *Ibid.*, p. 9.

formalised the theory of deterrence along the lines which the model suggested, so did their "game" of nuclear war become more and more like the perfect competition of classical economics, that is, a complete fiction of no relevance to the practical concerns of policy makers and military planners. In the end, the strategic theorists preferred superiority in method and technique, based as these were on rationality and science, to relevance and influence over the process of nuclear weapons policy and strategy formulation. Strategic studies crystallised into a discipline as superiority in method and technique increasingly effaced any opportunity for relevance and influence.