GAME THEORY AS DOGMA

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ABSTRACT

In this paper, we shall examine the role and status of Game Theory as "the science of strategy" in the context of claims of many of its proponents that it now provides an essential intellectual foundation for the social sciences, as well as some of the natural sciences. We shall argue that, rather than providing a scientific basis for such study, much of Game Theory should be better characterised as dogma and antithetical to scientific and critical reasoning. It is argued that it has provided a distorting prism through which to study complex social phenomena, and also crowded out alternative approaches that could have been valuable in this context.

We can prove only that our Game and we are indispensable by keeping the Game ever at the summit of our entire cultural life, by incorporating into it each new achievement, each new approach, and each new complex of problems from the scholarly disciplines. Herman Hesse, The Glass Bead Game, 1943

GAME THEORY AS DOGMA

1. Introduction

> *Every active Glass Bead Game player naturally dreams of a constant expansion of the fields of the Game until they include the entire universe.*
> Herman Hesse, The Glass Bead Game, 1943

Herman Hesse's novel was about an imagined world of scholars set far in the future who developed the Glass Bead Game, "as a universal language and method for expressing all intellectual concepts and all artistic values and reducing them to a common denominator" (Hesse, 1943, p. 121). "The Game was not mere practice and mere recreation; it became a form of concentrated self-awareness for intellectuals" (Hesse, 1943, p.33). When the book was published in 1943, it could hardly be seen as referring to Game Theory which would have by then registered barely a blip on most intellectual radar, indeed it was not until the following year that John von Neumann and Oskar Morgenstern published "The Theory of Games and Economic Behaviour", the work that is universally regarded as setting out the foundations of modern Game Theory. Both books signposted different routes to Nobel Prizes, with Hesse winning the Prize for Literature in 1946. However, it took many more years for Game Theory to really take hold in economics and other disciplines, with the Nobel Prize for Economics being awarded to the Game Theorists Nash, Selten and Harsanyi in 1994, and to Aumann and Schelling in 2005.

Herman Hesse's scholars were actually players in the Game and not just mere describers of games as in Game Theory, though of course that does not preclude the possibility that Game Theorists are themselves today playing various games (however defined) with their fellow scholars. In this paper, we shall be concerned with the current role and status of Game Theory and the aspirations and claims of some of its strongest protagonists, with special reference to claims by some that it provides the "science of strategy". We shall argue that, rather than providing a scientific basis for such study, much of Game Theory in such areas should be better characterised as dogma and antithetical to scientific and critical reasoning.

Before going on to both qualify and justify these remarks, a brief description of what constitutes a game in Game Theory is called for. A game is specified by the set of players, their set of possible actions, and the set of all payoffs, with each player's payoff depending on actions chosen by other players[1]. Basic assumptions include each player having perfect knowledge of the rules of the game as well as the payoffs of his or her opponents, and that all players are rational and attempt to maximise their utility, though some modern developments such as behavioural Game Theory modify some of these strong assumptions.

In the next section we consider some aspects of science and dogma in the context of our discussion of Game Theory before introducing Dixit and Nalebuffs' (1991 and 1993) overview of the role of Game Theory in strategic interaction to help provide both structure and content for our subsequent discussion. The five sections after that will look

in turn at each of Dixit and Nalebuffs' five main categories in this context, before finishing with a section on Taking Things to the Logical Conclusion.

2. Science and Dogma

> *At various times the Game was taken up and imitated by nearly all the scientific and scholarly disciplines, that is, adapted to the special fields.*
> Herman Hesse, The Glass Bead Game, 1943

Game Theory has been extensively applied in many of the social sciences and some of the natural sciences in recent years, and can already be regarded as the dominant paradigm in many fields of economics such as Industrial Organization. This is a remarkable transformation given its roots in what were regarded by some as just toy puzzles only a few years ago. At the same time, there have been numerous criticisms of Game Theory as applied to the social sciences in recent years. It has been argued that its usefulness is qualified by the fact that Game Theory models may produce no equilibrium outcome, or too many[2]. It can be argued that Game Theory models can assume overly complex reasoning capabilities to properly represent what are in practice rudimentary decision making processes[3], or that the models are (at the moment) too rudimentary to deal with what are in practice complex phenomena[4]. It results can depend crucially on the precise specification of the model chosen[5]. It has also brought into sharp focus wider arguments as to whether economic analysis in general and Game Theory in particular can be properly judged as science, often with special reference to the role of falsifiability in Game Theory[6].

This paper takes a rather different approach to many previous critiques of Game Theory in that it applies yardsticks of critical reasoning versus dogma here, rather than those of science versus non-science. Not only will we argue that these yardsticks are more relevant in this context, an immediate benefit is that while it may not be straightforward to identify what constitutes science, especially in the context of economics and the other social "sciences", it is easier to identify what constitutes dogma. The New Shorter Oxford English Dictionary defines "dogma" as: "doctrines or opinions, esp. on religious matters, laid down authoritatively or assertively". The same source defines "dogmatic" as: "of philosophy or medicine based on a priori assumptions rather than empirical evidence".

Dogma is most commonly associated with a world view or frame of reference encompassing a set of interrelated and mutually supporting beliefs. A widely cited example of dogma is creationism, one variant often labelled "scientific creationism" by its proponents. Over years and many articles, the scientist Stephen J. Gould waged a war on creationism, "scientific" or otherwise as dogmatic and unscientific[7]. However, as we shall note below, dogma may also be a characteristic of a single belief, attitude or statement.

Dogma tends to have certain defence mechanisms, the better to protect and sustain its world view. These tend to group together into three main characteristics or features;

Feature (1) Logical fallacy and tautology.
Feature (2) If dogma and evidence conflict, the evidence is rejected, not the dogma.
Feature (3) Evidence is ignored or misinterpreted.

Here is an example of dogma or a dogmatic belief; Scotland is certain to win the 2010 Soccer World Cup despite their failure to qualify for the 2006 World Cup, they have not won it before so it must be their turn.

This position exhibits all three features of dogma. First, logical fallacy; even if teams took "turns" at winning the World Cup (which they do not) Scotland is only one of many countries who have not won it and there is no reason to suppose it would have primacy over these other countries in these regards. Second, potentially relevant evidence indicating that Scotland was not good enough to even qualify for the current World Cup is rejected. Third, there is no reference to other evidence which suggests that while Scotland's current and past performance in international soccer is respectable for a nation of 5 million souls, it is not sufficient to warrant a strong belief that they will become world champions in the foreseeable future.

Further, even if an unusual set of circumstances led to Scotland winning the World Cup in 2010, any prior belief in such an event would still likely rank as dogma, dogma is a reflection of how logic and evidence is treated, not how the world develops.

It is important to note that dogma is not necessarily "good" or "bad". Dogmas can be motivating and socialising forces for good or ill. Dogmas helped fuel the Renaissance and the Sistine Chapel, World War 2 and its atrocities. I may have a long standing and touching faith in the prospects of the Scotland football team that defies logic and experience but which can be energising, sociable and enjoyable, while ultimately frustrating. It is not a question of virtue or vice, the problem with dogma is that it is the enemy of critical reasoning and requires the suspension of the normal faculties of scepticism and disbelief that are essential for intellectual progress.

Whether or not a world view such as Game Theory is or is not scientific is actually a sideshow and distraction in this respect. Debates concerning science versus dogma cover more limited spheres of intellectual endeavours than the broader issue of critical reasoning versus dogma. The latter issue can encompass whole areas of art and literature as well as the natural and social sciences, and can be as relevant to literary reviews of The Glass Bead Game as it is to the studies of the origins of life on Earth. Consequently, the question that will be explored here is the relationship between Game Theory and critical reasoning, not Game Theory and science. We will argue that Game Theory has the same claim to be regarded as dogma as do accepted types of dogma such as creationism and Marxism. The evidence we shall cite in support of this claim includes outright fallacies, tautologies, neglect and misuse of evidence, and other failures in critical reasoning.

We can add some caveats. It must be emphasised that this does not mean that all Game Theoretic arguments reasoning is dogma, or all Game Theorists are dogmatic. That itself

would be a failure of reasoning along the lines of arguing that, if creationism is dogma, all Christians must be dogmatic. There are Game Theorists, or those sympathetic to Game Theory, such as Thomas Schelling, David Kreps[8], Colin Camerer[9] and Richard Thaler[10] who have contributed thoughtful and informed contributions to, and critiques of, Game Theory that could in no way be classed as dogmatic. And while there are those Game Theorists who may be regarded as dogmatic in their approach to the potential contribution of their discipline, that does not mean, that everything they say is necessarily dogma. The position here is simply that much of Game Theory's present status and position has been created and buttressed by defence mechanisms designed to protect and promote it as dogma.

Nor is the position taken here antagonistic towards the development and application of mathematical, experimental and simulation tools in economics and the other social sciences. On the contrary, it is felt that approaches such as agent-based computational economics offer genuinely exciting opportunities for enriching and developing economic and other social theory. The comments here should be seen as specific to Game Theory.

In exploring the thesis of Game Theory as dogma, we shall use Dixit and Nalebuff (1991 and 1993) to structure our critique. Not only do they provide substantive argument and evidence which will be relevant in this context, the process by which they look at five major types of strategic interaction which they associate with the fundamentals of Game Theory will also be paralleled here. Their text has also been extremely influential and endorsed by leading economists, and has been adopted as main or auxiliary text in many strategy classes. It is indicative of a perspective that is broadly shared amongst many Game Theorists as to the actual and potential contribution of Game Theory to the study of strategy in particular, and the social sciences in general.

3. Game Theory and Strategic interaction

> *The true and ultimate finesse in the private Games of advanced players consists, of course, in their developing such mastery over the expressive, nomenclatural, and formative factors of the Game that they can inject individual and original ideas into any given Game played with objective historical materials.* Herman Hesse, The Glass Bead Game, 1943

In their precis (Dixit and Nalebuff, 1993) of their work on Game Theory and strategy (Dixit and Nalebuff, 1991), the authors start by stating "Game theory is the science of strategy". We note in passing that there are some problems with this seven-word statement, each of which is indicative of problems which we shall be looking at in more detail in this paper. First, the use of the definite article is unfortunate because it suggests that Game Theory is the only approach that can be seen as characterised by scientific method in this context, a view which may be contested by other social scientists. Second, it is arguable as to what extent Game Theory can itself be seen as characterised by scientific method, or whether it is instead better described as a self contained set of mathematical theorems and tautologies, a characterisation which, as we shall see, would not dismay at least one leading Game Theorist. Third, as has been noted (Kahan and

Rapoport, 1984) Game Theory is really not a theory at all, but is at best a collection of theories.

Dixit and Nalebuff (1993) then identify five types of strategic interaction which they say illustrate some of the fundamentals of Game Theory: (1) The Prisoners' Dilemma, (2) Mixing moves, (3) Strategic moves, (4) Bargaining, and (5) Concealing and revealing information. Each of these is discussed in more detail in their 1991 book, and we shall look at each of these in turn here.

4. The Prisoners' Dilemma

> *The mathematicians brought the Game to a high degree of flexibility and sublimation.* Herman Hesse, The Glass Bead Game, 1943

The first type of strategic interaction which Dixit and Nalebuff (1993) argue helps illustrate some of the fundamentals of Game Theory is the Prisoners' Dilemma.

In the original version of this game, two suspects are interrogated in different cells. Both prisoners can confess and implicate the other, or say nothing. The Dilemma is that each suspect can improve his situation by confessing, no matter what the other suspect does; he gets remission for confessing if the other also confesses, and he gets the benefits of turning State's evidence if the other does not. The Game Theory prediction is that even though they would both be better off if they could co-operate (not confess) that they will both defect (confess).

The Prisoners Dilemma has been the subject of literally hundreds of experiments. Roth sums up experiments in the single period game as typically reporting "a level of co-operation which responded readily to various kinds of experimental manipulation but which was bounded well away from either zero or one hundred percent" (p.998).. Roth (1988, pp.999-1000) also notes that a typical result for finitely repeated games is for co-operation to be observed for some periods but to break down towards the end. The puzzle from Game Theory's point of view is why any co-operation should be observed at all for those games, let alone the frequently high levels of co-operation that were observed in both single period and finitely repeated experiments.

Rather than reject the Game Theoretic approach to this problem in the light of observed behaviour, Game Theorists instead employ a variety of what could be described as defensive strategies.

(1) Ignore; for example, neither Dixit and Nalebuff (1991) nor Binmore (1992) refer to the abundance of experimental evidence that conflicts with the Prisoners' Dilemma model.

(2) Denial; after referring to the "thousands" of experiments on the Prisoners' Dilemma, Andreoni and Varian (1999) comment "Although researchers often find some fraction of cooperation, the general result in these experiments is that the incentives to defect can be

very powerful. When subjects are faced with a single shot of a Prisoners' Dilemma they seldom reach mutually cooperative outcomes" (p.10933 sic)

(3) Rationalize; referring to the results that the cooperative outcome is frequently observed in the Prisoners Dilemma experiments, Dixit and Skeath (1999, p. 271) argue: "such observed behavior can be rationalized in different ways. Perhaps the players are not sure that the relationship will actually end at the stated time. Perhaps they believe their reputation for cooperation will carry over to other similar games against the same, or to other opponents. Perhaps they think it possible that their opponent is a naive cooperator and they are willing to risk a little loss in testing this hypothesis out for a couple of plays."

Or perhaps it would first be appropriate to reject the Prisoners' Dilemma model. Each of these rationalizations is inconsistent with the Game Theory predictions, and any uncertainty about length of experiment or belief in reputational carryover should be eliminated in any reasonably designed experiment.

(4) Switch Games; Binmore (1992, p.312) notes (though without reference to empirical results) that if anyone argues that the players may care about the welfare of their opponents and so be inclined to co-operate; "such players will not be playing the Prisoner's Dilemma. They will be playing some other game with different payoffs" (italics in original). So rather than reject the Prisoners' Dilemma in particular and Game Theory's explanation in general, Binmore implies that contrary results could mean that the standard Dilemma is merely one of a family of games that might be played here. But does this not mean that this would reduce Game Theory to the status of tautology? Binmore anticipates this accusation; "such an accusation disturbs a game theorist not in the least. There is nothing a game theorist would like better than for his propositions to be entitled to the status of tautologies, just like proper mathematical theorems" (1992, p.314).

Tautology and circular reasoning are of course amongst the most common defence mechanisms of dogma. While Binmore must be commended for revealing the game objective of Game Theory[11], for those of an empirical bent, this does tend to point the way to unproductive circular reasoning. It implies that if results are consistent with the Prisoners' Dilemma they may be cited as supporting it (even if they actually reflect motives such as vengeance or sadism which are extraneous to the model), while if the results conflict with the Prisoners' Dilemma, they may be dismissed as indicating that another (undefined) game was being played. The price of defending the Game as tautology is, of course, empirical relevance. This defence would be all the more supportable if Binmore did not preface his text with "above all (this) is a how-to-do-it book" (1992, p.viii), perhaps this being indicative of the belief and the fallacy that it is possible to eat your cake and still keep it. A prescriptive text should first of all display a respect for empirical realities.

(5) Blame the subjects: Romp (1997, p. 230) notes two fundamental Game Theory assumptions for the Prisoners' Dilemma model; "the first is that subjects are

instrumentally rational, the second is that this is common knowledge," (underlining in original). He argues that if experimental evidence contradicts Game Theory's predictions here, it is not clear which of the two underlying assumptions have been violated.

However, this also involves fallacious reasoning. A corollary of Romp's argument is that if both assumptions held, we would expect to find the Game Theory predications hold. That is not necessarily the case, for example subjects may be altruistic and derive utility from making others better off (which Romp recognizes, 1997, p.234), in which case it might be instrumentally rational to not confess or defect, even with no problem of common knowledge. Beyond that, to blame the experiments' failures to support Game Theory's predictions on the subjects for alleged cognitive failures or limitations is to again to miss the point that it is the prediction that failed, not the subjects, who might have very different values and systems of preferences from those imputed to them by the Game Theorists.

(6) Accessorize: what might be described as the "accessorize" defence acknowledges that that actual evidence often conflicts with the game predictions, but that the problem is not that the Game Theory approach is inappropriate, instead that the model is presently too simple, and that the solution is to add further refinements to it[12].

This is clearly a tactic that could be pursued for as long as results fail to support the theory.

(7) Downplay: another defence is to question whether the Prisoners Dilemma is actually a good game to play. Binmore (1992 p. 313, footnote); "my own view is that the Prisoners' Dilemma is almost never a suitable paradigm for the cooperation problems it is said to epitomise". Binmore instead recommends the Nash Demand Game.

Ignoring, denying, rationalizing, switching, blaming the subjects, accessorizing and downplaying can all be defence mechanisms for dogma. In practice, there may well be overlap and mutual support between these categories, for example blaming the subjects may be regarded as a form of rationalisation. None of these defence mechanisms explicitly reject the Game Theoretic description of the Prisoners' Dilemma as they should, the Dilemma being a demonstrably poor describer and predictor of behaviour, even in tightly controlled experimental set ups designed to create as closely as possible the conditions conducive to the Game Theoretic predicted outcome of immediate and invariable defection in games of finite duration. Indeed, rather than dealing with the evidence, Dixit and Nalebuff (1991) then go on to commit a logical fallacy which could be characterised as the loaded question, the complex question, or simply begging the question (Petitio Principii) when they explore how the Prisoners' Dilemma can be "resolved" in their chapter 4 (titled; "resolving the Prisoners' Dilemma"); "the underlying problem is the players' incentive to cheat in any agreement. Therefore the central questions are; how can such cheating be detected? What prospect of punishment will deter it?" (p.95). This is the "when did you stop beating your wife." sort of question where Dixit and Nalebuff assume that cheating is the underlying problem of the

Prisoners' Dilemma, which is itself the very conclusion they have not yet demonstrated, and indeed is a conclusion which can be difficult to reconcile with the evidence.

Despite its consistent failure, the invariant equilibrium solution of defect/defect is still frequently presented as the "correct" solution to the Prisoner's Dilemma in introductions to Game Theory, often without reference to the contrary evidence. Indeed, when many Prisoners' Dilemma experiments conducted by economists are studied, they often reveal crucial design flaws systematically biasing responses towards the Game Theoretic answer. The most common source of bias is to use economics students as experimental subjects, especially since many or most economic students will have already encountered the Prisoners' Dilemma, both the "problem" and its "solution" in their first year text books. Whether because of nature or nurture or both, the choice of economics students as subjects should be expected to bias experimental results towards the expected "rational" Game Theoretic answer. This is evidenced by Frank, Gilovich and Regan (1993) who found that when economics and non-economics majors were asked to play a one-shot Prisoners' Dilemma game, economics students defected 60.4% of the time and co-operated only 39.6% of the time, while the proportions just about reversed in the case of non-economics students who co-operated 61.2% of the time and defected 38.2% of the time.

Biases created by choice of experimental cohorts can be reinforced by other flaws such as instructions or cues given to the subjects. Recent studies by Kay and Ross (2003) and Liberman, Samuels and Ross (2004) have demonstrated how often apparently subtle influences can strongly affect propensities to cooperate or defect in the Prisoners' Dilemma. Even the label attached to the Prisoners' Dilemma by the experimenters can be crucial. For example, Liberman et al (2004) found when the Prisoners' Dilemma was called "the Community Game" that mutual co-operation was the rule and mutual defection was the exception, whereas the opposite was the case when the experiment was labelled "the Wall St. Game" (p. 15)[13].

Once the potential importance of subject selection and cue biases are acknowledged, it encourages a re-evaluation of many experiments conducted by economists in these areas. For example, Andreoni and Miller (1993) recruited subjects for their Prisoners' Dilemma experiments from enrolments on the introductory microeoconomics course at the University of Wisconsin. Subjects were first told; "This experiment is a study of economic decision making". The economics students here could be forgiven for thinking that they were not so much subjects in a behavioural experiment as taking part in an economics exam, and if it is noteworthy for anything, it is for how many students still "failed" this exam by co-operating rather than defecting. This is rather like testing a group of theological students to see if they believe in God, and then finding out that many do not, much of the time. Had the experiment been rerun with sociology students being instructed, "This experiment is a study of social decision making", we might have expected different results.

There is no paradox in the failure of Prisoners' Dilemma experiments to adequately predict behaviour using Game Theoretic models. Many people, in many social contexts,

much of the time, naturally incline towards co-operation; but also many people, in many social contexts, at least some of the time, do not. There is no surprise or paradox in the mixed results coming out of these experiments, it is what should be expected. In general, the Prisoners' Dilemma model and its prediction of invariant defection should be rejected. Behaviour should not be ignored, denied, rationalized, rejected, accessorized, downplayed, or the subjects blamed, by Game Theorists. The perpetuation of a discredited view of the world is dogma, not science.

5. Mixing Moves

> *The Glass Bead Game should admit of everything, even that a single plant should chat in Latin with Linnaeus*. Herman Hesse, The Glass Bead Game, 1943
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> *The trouble with game theory is that it can explain everything. If a bank president was standing in the street and lighting his pants on fire, some game theorist would explain it as rational,* Richard Rumelt, Napa, 1990[14]

The second type of strategic interaction which Dixit and Nalebuff (1993) argue in their précis helps illustrate some of the fundamentals of Game Theory is mixing moves. This is discussed in detail in their chapter 7 on "Unpredictability", Dixit and Nalebuff (1991) pp. 168-98. One theme running through this chapter is that it is important that your opponent does not discover and exploit any systematic behaviour on your part, and that randomizing your choices (e.g. by tossing a coin or picking a random number) can be one way to ensure this by keeping your opponent guessing. This chapter goes on to discuss the role in strategic interaction of such devices as: randomising your moves; bodyguards of lies; surprising yourself in order to surprise your opponent; decoys; dummy moves; price discounts; credibility and bluffing.

The only problem is that incorporating these ideas and concepts into Game Theory does make them Game Theoretic and validate Game Theory any more than would incorporating a shovel into my hand make me a gardener. These ideas and notions exist outside of Game Theory, existed before Game Theory, and indeed would exist despite, and without, Game Theory. The danger is that the process of importation of those concepts into Game Theory may be used to justify Game Theory in ways that may not be entirely legitimate.

A typical form of reasoning implicit in much of Game Theory is as follows:

(1) Entry deterrence explains firm dominance
(2) Game Theory can model firm dominance
(3) So Game Theory can explain firm dominance

We can demonstrate the fallacy here with another example;

(1) Gluttony explains why Homer Simpson gobbles his burger
(2) Game Theory can model gluttony
(3) So Game Theory can explain why Homer Simpson gobbles his burger.

Aficionados of "The Simpsons"[15] would have no difficulty in spotting the fallacy inherent in this argument. There is no strategic interaction or game playing underlying this character's propensity to gulp his food, it is a near-solipsistic primal impulse that is independent of, and insensitive to, the presence of any other human being in the known universe, or indeed any other material object, whether animal, vegetable or mineral, whether real or imagined, whether sentient or non-sentient. Not only is Game Theory not needed to explain such behaviour, it would be misleading at the very least to use Game Theory to model it. Even the cerebrally-challenged Homer would have sufficient command of rational principles to agree with that sentiment.

But fallacious reasoning involving non sequitars and where "can" is taken to imply "ought" is explicit or implicit in much of Game Theory. The implicit argument is that since Game Theory can model just about any form of rational human behaviour, it should model just about any form of rational human behaviour. The explicit argument used to achieve this outcome is called "no-fat modelling" (Rasmussen, 1989), which involves first observing a stylised fact, and then finding the simplest set of premises which together imply the stylised fact.

In a sympathetic review of the potential review of the relevance of Game Theory to research in strategy, Camerer (1994) notes that no-fat modelling with Game Theory has swept the economics profession and some allied disciplines, but he points out some problems with the approach; "the models provide a sufficient explanation for an observed fact, but the explanation may not be necessary. For example, many sets of premises other than those given … can explain why there are strikes, warranties, legal partnerships, and underinvestment. The firm mathematical footing underlying no-fat explanations may be a poor reason to prefer them to competing explanations which are hard to express formally" (p.209). In the following chapter, Postrel (1994) then gives an example of no-fat modelling in Game Theory when he develops a model for which there exists a subgame perfect Bayesian Nash equilibrium for Richard Rumelt's Flaming Trousers Conjecture (see above, this section) in which bank presidents set their pants on fire.

No-fat modelling as practiced by game theorists is in fact a mathematical version of a classical dogma defence mechanism. No-fat modellers in Game Theory basically say, show me your facts, and I will give you an explanation for those facts that is consistent with my dogma. What is remarkable is that, despite the ability of dogma to interpret facts in its own light, Camerer (1994, p. 209) notes that Game Theory has made little headway to date in influencing research on strategy.

Camerer attributes this lack of uptake to a mismatch between the spare customized storytelling style of Game Theoretic no-fat models and traditional strategy emphasis on comprehensive empirical studies. But there is an even more obvious explanation. Game Theoretic explanations of strategy are simply inadequate or unpersuasive.

This is demonstrated by Dixit and Nalebuff's (1991) discussion of mixed moves in strategy. Ironically, after spending much of this chapter discussing serving at tennis, they note (1991, p.192): "So far there have been very few examples of mixed strategies outside the sporting world. Why are there so few instances of businesses using randomized behaviour out in the real world?" An obvious answer might be that, unlike serving at tennis, important business decisions can involve unique, unfolding, non-repetitious events and that there may be little, if any, similarity between serving at tennis and running a business. But Dixit and Nalebuff pin the blame on the subjects of the theory rather than the theory itself; "First[16], it may be difficult to build in the idea of leaving the outcome to chance in a corporate culture that wants to maintain control over the outcome" (1991. p.192)..

But this argument carries the seed of its own negation. We can see why with a simple mental experiment. Suppose that Dixit and Nalebuff are right and that deliberately randomising choices (e.g. through tossing a coin or picking random numbers) could give a firm a competitive advantage in the economic market place. However "corporate culture" prevents many or all firms from adopting such strategies.

In that case, any single firm that discovered the potential source of competitive advantage inherent in randomising their choices should have a competitive edge over their rivals, ceteris paribus. It should be able to out-compete and eventually dominate its rivals, again ceteris paribus.

However, the gains from adopting randomized behaviour would not stop there. This is not a strategy that should be firm-, industry-, technology- or market-specific; if it works in one context, Dixit and Nalebuffs' arguments suggest that it should work in other contexts as well. Even if we still had only that one firm that had discovered the secret of competitive advantage conferred by randomized choices, this source of competitive advantage should quickly spread to other sectors, whether through internal expansion, merger, acquisition, or simply the demonstration effect and imitation of a successful strategy.

But that has not happened, which would seem to suggest that there is something wrong with the theory rather than the strategists. Indeed, the cases that Dixit and Nalebuff identify (1991, pp. 193-95) as the "most widespread" use of randomised strategies in business are strategies to motivate compliance at low monitoring cost, such as random tax audits, random drug tests and parking meters. However, these are not actually strategies employed by <u>business</u>, these are strategies employed by <u>agencies</u> monitoring businesses in cases where there are populations involving reasonably standardised events (tax returns, drug taking, parking places) and a recognised propensity to cheat. In each case the authority may wish to deter cheating and may assess the costs and benefits of alternative levels of monitoring where the propensity to cheat is likely to be inversely related to the probability of being caught. Further, these are not really strategic games but are rather like the strategy of decimation (where one in ten of an underperforming Roman legion would be selected for death *pour encourager les autres*) where the game's

rule maker decides the rules under which malfeasance may be policed and punished, and those being monitored then choose how to respond. Modelling these as games is really overcomplicating the pudding, a simpler route would be a decision theoretic approach in which the costs and benefits of monitoring and probabilities of malfeasance are fed into the decision calculus, and the rule maker varies the rules and processes accordingly. Again the fallacy is to argue that because Game Theory can model such rule-making behaviour, they should model such behaviour.

6. Strategic Moves

> *You mathematicians and Glass Bead Game players … have distilled a kind of world history to suit your own tastes.* Herman Hesse, The Glass Bead Game, 1943

The third type of strategic interaction which Dixit and Nalebuff (1993) argue in their précis helps illustrate some of the fundamentals of Game Theory, is strategic moves; "the general principle here is that it can be in a player's interest to reduce his own freedom of future action. By doing so, he removes his own temptation to renege on a promise or to forgive other's transgressions" (Dixit and Nalebuff, 1993, p.4). A core idea here is the notion of credible commitments to which Dixit and Nalebuff (1991) devote chapter 6.

Dixit and Nalebuff (1991) give six examples of how the particular strategy of burning your bridges behind you can signal credible commitment to a given strategy. We shall discuss each in turn.

The first example given by Dixit and Nalebuff (1991, p.152) is that "armies often achieve commitment by denying themselves an opportunity to retreat. This strategy goes back at least to 1066 when William the Conqueror's invading army burned its own ships, thus making an unconditional commitment to fight rather than retreat."

The problem is that this is not what happened. "William decided to move his army to Hastings which was a better port in a more defensible area" (Ashley, 1973, p. 37). "William's aim was to keep in close touch with his ships because he knew that he had a severe struggle ahead of him and might even be thrust back to the sea" (Ashley, 1973, p.37). After Hastings "keeping close to his fleet, William advanced round the coast … before striking towards London" (Mathew, 1966, p. 84

Ashley (1973, p.37 even notes that William erected fortifications to protect his ships at Hastings. Ironically, the evidence such as it is, suggests that rather than William trying to destroy his own ships, his enemy Harold tried to cut William off from access to them (Douglas, 1964, p.197).

While historical records of the time are notoriously patchy and incomplete, other commentators have noted the burning of an invasion fleet would have been a waste of valuable construction materials and other accounts have the ships subsequently dismantled for the construction of forts once the invasion became land-based.

The second "burn your bridges" example given by Dixit and Nalebuff was where "Cortes burned his own ships upon his arrival in Mexico. He purposefully eliminated retreat as an option. Without ships to sail home, Cortes would either succeed in his conquest or perish. Although his soldiers were vastly outnumbered, this threat to fight to the death demoralized the opposition; it chose to retreat rather than fight such a determined opponent" (Dixit and Nalebuff, 1993, p.4).

Again, this did not happen. Dixit and Nalebuff (1991, p.153) add a secondary reason for Cortes burning his ships was that it would have encouraged his men to fight. However, Cortes actually used a much more conventional method for ensuring his men were committed to staying - he bribed them (Hassig, 1994, pp. 56-57)

The source of the myth here appears to be that, some weeks after arriving in the New World, Cortes discovered a plot involving a number of his crew conspiring to defect to Cuba and join Velasquez, who had been granted authority by the king of Spain over the dominions. Cortes' crew had divided loyalties and there was little hope of united support for his plans as long as escape to Cuba was possible, at which point Cortes ordered almost all of his twelve vessels to be grounded on to the sands and declared unseaworthy (Thomas,1993, p.222). In an echo of what might have happened with William the Conqueror, Thomas also notes that Cortes planned to use the wood for construction purposes. Cortes actually told his men that anyone who wanted to could go back to Cuba with the one boat that remained, but Cortes' promise of great riches and "shame (and fear of what Cortes' reaction might be) overtook the hesitant" (Thomas, 1993, p.223-34).

Thomas comments that all the accounts of those who were there, including Cortes, note it was a grounding of the boats and not a burning, one possibility he notes is that what was originally scribed in handwriting as quebrando "breaking" may have been misread a few years later as quemando "burning" (Thomas, 1993, p.223)[17].

Unfortunately, the myths surrounding William the Conqueror and Cortes' alleged ship burning are now being presented as historical facts in economics textbooks: e.g. Milgrom and Roberts (1992, p.133); Besanko, Dranove, Shanley and Schaefer (2004, p. 234).

The third example given by Dixit and Nalebuff (1991, is another burning ships example, this time explicitly based on myth (the Trojan Wars) but on this occasion involving the Trojans trying to destroy their enemies ships, an action most soldiers would regard as a reasonable objective, but which in Dixit and Nalebuffs' perspective simply meant that, "(t)he Trojans seemed to get it all backward" (p.153). Whether the Trojans got it all backward really depends on whether military strategists might consider it a good idea from time to time to get round to actually trying to destroy the enemy's assets instead of their own.

The fourth and fifth examples of such credible commitments given by Dixit and Nalbuff (1991) under their "burn your bridges" section has Walter Mondale promising to raise taxes if elected, and East Germany's Prime Minister Egon Krenz "dismantling parts of the Berlin Wall" as a credible promise to reform and so hopefully retain power; "by

(re)opening a bridge to the West, the government forced itself to reform or risk an exodus" (Dixit and Nalebuff, 1991, p.155). But both strategies failed. As Dixit and Nalebuff acknowledge, Mondale failed to be elected. The East German government lost power in just a few months with the reunification of the two Germanys, Krenz had been forced to resign as leader just one month after the opening of the Wall, and was later sentenced to six and half years imprisonment for Cold War crimes. It does seem strange to argue the merits of a strategy by using examples which failed to achieve their objectives.

But even more strikingly there was not even a strategy of credible commitment on the part of the East German government through "dismantling parts of the Berlin Wall", or even of opening the gates. What actually happened was that at the end of reading out a long press release in East Berlin, a party boss, Schabowski, gave the mistaken impression that anyone who wished to go to the West could do so immediately, when the actual intention instead was to relax the rules in a controlled and managed fashion. This error was compounded with TV and radio mistakenly reporting that Schabowski's announcement meant that the Wall was open. Thousands of jubilant East Berliners headed for, and through, the checkpoints, overwhelming the equally confused and astonished guards who were faced with the choice of giving way or massacring thousands. "Through ineptitude on the part of the (East German government) the Berlin Wall had been inadvertently breached" (Turner, 1992, p.234). And once the Wall was breached, it was impossible to rein in the expectations that those in the East could now travel freely back and forward to the West. After that, the actual physical dismantling of the Wall started with spontaneous popular action and also had nothing to do with government strategy. The rest is history, and as is the case with so much of history, tends to reflect accident rather than deliberate strategy.

However, it is the sixth and final example of "burning your bridges behind you" given by Dixit and Nalebuff (1991) that is particularly noteworthy, because it is the only example in this section that is explicitly about business strategy. This is the case of Polaroid, and as Dixit and Nalebuff (1991, p.154) note; "for many years, Edwin Land's Polaroid corporation purposefully refused to diversify out of the instant photography business. With all its chips in instant technology, it was committed to fight against any intruder in the market".

That is strictly speaking true, but there are two other facts that are relevant here. First, it is well documented that it was Edwin Land himself, Polaroid's inventor-founder who resisted strong arguments from his senior managers to reduce the company's dependence on instant technology, the technology he himself did so much to develop. Secondly, that strategy was an absolute disaster for Polaroid, it resulted in the company missing out or being late in adopting important technological developments and finished up with its business base eroded by threats as diverse as 33mm cameras, one-hour photo shops, disposable cameras and digital cameras. Unusually for a company of its size and reputation, it filed for Chapter 11 protection from bankruptcy in 2001.

Ironically, Dixit and Nalebuff (1991, p.154) recognise that around 1990; “lacking bridges, Polaroid began to feel trapped on a sinking island”. That is correct, where Dixit and Nalebuff cite Polaroid’s Nineteen-Eighties strategy as a model of a particular business strategy to be commended, researchers in strategic management frequently cite the same strategy as one to be avoided wherever possible.

Polaroid’s dependence on a narrow and obsolescing technological base did not represent a strength, it was a chronic weakness which led to its downfall. Firms making 35mm cameras, disposable cameras and digital cameras did not have to worry about treading on Polaroids turf, instead they created new turf which drew Polaroid’s customers away. To think that deliberately making yourself vulnerable is an effective way of deterring new technological competition represents at best a fundamental misunderstanding of how Schumpeterian gales of creative destruction work. To not have the protection of diversified technological and market bases in such a turbulent environment (unlike Polaroid’s rival Kodak) is not rational, it is potentially suicidal[18].

In the Conan Doyle story “Silver Blaze” Sherlock Holmes refers to the curious incident of the dog in the night-time. "The dog did nothing in the night-time," he is told. "That was the curious incident," he remarks. Similarly, the curious thing about the core strategies that Dixit and Nalebuff are identifying is the absence of evidence that these strategies exist, or if do they exist, that they are successful. If businessmen randomized their strategies with the help of coin tosses or tables of random numbers, the secret would surely have leaked out by now, while if invading navies made a habit of actually burning their ships on arrival in a strange lands, evidence of this would surely have accumulated, not least in the form of regular replacement orders in boat builders accounts. Similarly, what also makes Polaroid’s (failed) strategy so noteworthy is that it was so unusual. If such a philosophy of deliberately putting all your eggs in one basket worked for large companies in such dynamic technological environments, we would expect to see major high technology companies like Kodak, GE, Hewlett-Packard, Sony, Samsung and Siemens adopting Polaroid’s strategy. The fact that they do not, may be taken as evidence that this is not generally regarded as a rational survival strategy for large firms in such environments.

In short, these various commitments either never happened, or if they did happen they happened for different reasons than those cited by Dixit and Nalebuff and/or they failed in their objectives. In some cases an original error of historical fact was made by others, not Dixit and Nalrbuff, but in each such case there has been well documented evidence to question what has become a Game Theoretic interpretation, or to show that it is just plain wrong. If nothing else, this shows the importance of using a variety of sources and/or sources as close to the original story as possible, when historical events are being discussed.

All this hardly constitutes a persuasive case for “thinking strategically” along the lines advocated by Dixit and Nalebuff here. The analysis here has no sound foundation in logic or fact.

7. Bargaining

> *If the anatomist sees nothing but his pattern, and ignores the unique, individual reality of his object, then he is a Castalian, a Glass Bead Game Player, he is using mathematics on the least appropriate object.* Herman Hesse, The Glass Bead Game, 1943

The fourth type of strategic interaction which Dixit and Nalebuff (1993) argue helps illustrate some of the fundamentals of Game Theory is bargaining. In this context, they illustrate the fundamental underlying principle of backward induction; "two players decide how to split a pie. Each wants a larger share, and both prefer to achieve agreement sooner rather than later. When the two take turns making offers, the principle of looking ahead and reasoning back determines the equilibrium shares. Agreement is reached at once, but the cost of delay governs the shares. The player more impatient to reach agreement gets a smaller share" (Dixit and Nalebuff, 1993, pp.5-6).

There are several problems with this line of reasoning, but the most obvious is that "the principle of looking ahead and reasoning back" (backward induction) is not an accurate description of how bargaining takes place. That is not to say that players do not look forward and anticipate the consequences of their actions, in many cases of course they do. But that is quite different from saying that backward induction characterises this process, which in general it does not.

The principle of backward induction is demonstrated by the "Surprise Test" paradox (Chow, 1998). At the beginning of the school day, pupils are told there will be a surprise test during one of today's eight periods. The pupils know that the test cannot be left to the last period (period 8) because by then it would no longer be a surprise. This eliminates period 8 and means that period 7 effectively becomes the last period in which a surprise test can be held. But by the same reasoning that eliminated period 8, since period 7 is the last period during which a surprise test can be held, if it has not been held by period 7 it cannot be held then either since it would no longer be a surprise. This eliminates period 7 for consideration for a surprise test, and extension of this same logic of backward induction in turn eliminates period 6, period 5, and so on, until it has been shown that there is no period during which a surprise test can be held.

The teacher then decides to hold the test during the fourth period, to the pupils' surprise, which of course leads to the paradox.

At one level, the Surprise Test Paradox is an entertaining brainteaser and indeed I remember being puzzled by it as a child. I also remember being warned by teachers that there would be a surprise fire alarm sometime that day, then being surprised by it, which of course backward induction teaches us simply could not have happened. Perhaps the teachers pretended to surprise us and we pretended to be surprised. At another level, as Chow (1998) notes, the Surprise Test has been the subject of almost a hundred papers in mathematics, philosophy and Game Theory, and described as a "significant problem" for philosophy. It is worth remembering that the Prisoners Dilemma was also introduced as

merely a "toy game" to begin with (Binmore, 1992, 310). At a further level, it has been argued that there are similarities between the Surprise Test Paradox and what might be described as Game Theory's signature game, the finite repeated Prisoners' Dilemma where the solution is immediate defection by both players. Indeed, Chow (1998) notes that it has been argued by Sorensen (1988) that the Surprise Test Paradox and the finite repeated Prisoners' Dilemma are actually the same, though Chow does not go that far, pointing out that invariable defection is counterintuitive in the repeated Prisoners Dilemma, while the existence of the surprise test is a contradiction in the Surprise Test. Nevertheless, Chow recognises parallels between the two that he argues could have implications for Game Theory.

It is interesting to revisit Dixit and Skeaths' comments above about "rationalizing" co-operation in the repeated finite Prisoners' Dilemma in the light of Chows comments that invariable defection is counterintuitive in such games. The mathematician Chow is exactly right, cooperation is unsurprising and normal in such set ups, and indeed invariable defection is not only counterintuitive, it is rarely observed. Yet despite this, Dixit and Skeath persist in trying to find "answers" to why cooperation is frequently observed in such games and why the players do not play the game the way they should if they were playing rationally, which in their view means invariably defecting. This is analogous to trying to find out why individuals could be so irrational as to be surprised by surprise tests, surprise fire alarms and surprise kit inspections, instead of considering there might be a problem with the process of backward induction that creates apparent paradoxes. Similarly, instead of questioning the players reasoning in finitely repeated Prisoners' Dilemmas, it is more appropriate to question the process of backward indication that leads to conclusions that are both counterintuitive and persistently contradicted by empirical evidence.

And that is why a simple children's puzzle about tests holds profound implications for Game Theory. Ultimately it makes little difference whether the finite repeated Prisoners Dilemma is the same as the Surprise Test Paradox or just analogous to it; if predictions are counterintuitive and refuted by empirical evidence, then we should reject the model the predictions are based on, we should not reject the behaviour.

But accepting this point would have profound implications, not just the Prisoners' Dilemma Model, but for much of Game Theory. The rejection of the conclusions of the Surprise Test Paradox and the Prisoners' Dilemma model is actually a rejection of the process of backward induction which created the counterintuitive conclusions refuted by observation in both cases. If the process breaks down in these simple cases, on what basis can a case be made for applying the same process of backward induction in other more complex decision processes in Game Theory? Accepting this point would remove the foundations from much of Game Theory. However, it is difficult to see grounds on which it could be rejected or ignored.

Whether and how paradoxical or counterintuitive conclusions can be resolved here is not the issue. Paradoxical, counterintuitive and empirically refuted conclusions only appear when the process of backward indication is applied to these situations. Remove the

process of backward induction and you remove the source of these problems, and in turn the problems themselves. What should be put in its place to study such decision processes is another matter, but one for which psychologists, sociologists, organizational and institutional theorists have tools at their disposal

In the next section we will look at the fifth and final type of strategic interaction which Dixit and Nalebuff argue helps illustrate some of the fundamentals of Game Theory

8. Concealing and revealing information.

> *One who knows music only from the extracts which the Glass Bead Game distils from it may well be a good Glass Bead Game player, but he is far from being a musician, and presumably he is no historian either.* Herman Hesse, The Glass Bead Game, 1943

The fifth and final type of strategic interaction discussed by Dixit and Nalebuff (1993) is concealing and revealing information. The example they give here is of an extended warranty, which they argue can be a credible signal to the consumer that the firm believes it is producing a high-quality product.

That can indeed be the case, but consistent with the points made above, there need be nothing particularly Game Theoretic about extended warranties. The firm is making a credible commitment to quality, and as result the consumer may be more inclined to trust the company and buy the product. While the actual impact on consumer perceptions and propensity to buy may be difficult to estimate and measure in practice, these difficulties do not make warranties any more Game Theoretic than does my leaving a deposit as hostage for my rented tennis racket.

But one of the areas where the issue of concealing versus revealing information has become of major importance in Game Theory is not discussed by Dixit and Nalebeth, and that is patent races. A patent race is typically modelled as a race between firms to innovate first, the first firm to complete the project wins the patent race and exclusive rights to develop and market the product (Tirole, 1988, p.394). Scherer (1992) gives the example of a model with Cournot reaction functions for two paired rivals. In discussing a scenario in which one firm has established a lead in the R&D race, Scherer argues; "when Firm 1 awakes to the challenge, it commences a crash course to recoup .... Recognising the competition, Firm 2 accelerates .... Seeing that it cannot be the first mover, Firm 1 reacts submissively ...." (1992, p. 33).

The first problem here is the confusion between patents and innovation. There is rarely a one-to-one correspondence between patents and commercial innovation, a single patent may underlie many innovations if it leads to innovation at all (many patents are for defensive reason), and a single innovation may involve many patents. The long-expired 1955 US patent 2,717,437 (for Velcro) finished up embodied in a multiplicity of products ranging through clothing, aircraft, office equipment, sport, automotive, medical and space industries. In turn, a complex high technology innovation such as a car, an aircraft, or a

microprocessor, can be protected by a vast array of patents. Even a single software product such as McAfee's Entercept system protection version 5.1 (2004) is still protected by 13 US patents and 57 copyright notices[19]. At company level, Intel had over 450 products in 2004, but also secured over 1,600 US patents in that year alone, and had received nearly 10,000 U.S Patents since 1976[20]. Yet there is no known documented evidence of any patent race involving Intel, let alone the thousands we could expect it have been involved in if the patent race was a significant feature of technological competition.

Indeed, the remarkable thing about patent races is the lack of evidence that they exist. It seems that this is another case of the dog that did not bark (Kay, 1997, pp.22-28) One can almost imagine Holmes musing to his amanuensis "you know Watson, the curious thing about patent races is that despite the enormous volume of material that has been written about them, it seems no-one has actually seen one …" However, in the Sherlock Holmes canon there was only the solitary case of the dog that failed to bark in the night; in the world of Game Theory we are encountering a veritable pack of mute canines, from mongrel (or mixed) moves, through burning vessels, to patent races.

The obvious response from proponents of Game Theory is that commercial confidentially and secrecy makes it difficult or impossible for third parties to observe patent races. But of course that is the exactly the reason that makes it difficult or impossible for them to exist in the first place. It is not just the third parties themselves that find it difficult or impossible to find evidence of patent races, if races of the type described by Scherer actually existed, at least anecdotal evidence should be building up from the ex post accounts of the innovation process from scientists and managers. But the reality is that if firms wish to conceal their R&D activities from rivals, there are typically a number of (albeit imperfect) devices that they can use, ranging through simple secrecy, to nondisclosure agreements with employees, to burying project level information within the operations and aggregate budgets of diversified R&D labs.

We have a further layer of logical problems here. It is contradictory to argue that it is possible for an interactive race of the kind described by Scherer to exist if no-one, not even the supposed participants, can observe it.

It is true that firms do publish defensive patents to block other firms efforts, do contest and object to rivals patent claims, there is an abundance of evidence of simultaneous or near-simultaneous discovery or invention, and R&D information can leak to other firms. None of this is evidence of patent races, it is to be expected when different R&D teams are pursuing independent work building on the same known scientific and technological bases, especially if the work involves only marginal advances on these known bases. None of this is evidence that firms react to rivals' decisions pursuing similar patent targets during the R&D search process, which is what is typically assumed in the patent race literature.

Further, proponents of patent races frequently argue they are following in the tradition of Schumpeter (1954). In fact, this is another fallacy (Kay, 1997, 22-28). Dasgupta (1986)

bemoans “the lack of clarity in Schumpeter’s writings” (p.521), but he himself is guilty of the fallacy of equivocation where he uses the phrase “technological competition” to place the patent race literature in the Schumpeterian tradition. The phrase is in fact ambiguous, Schumpeter’s version of technological competition was competition from the new technology (Schumpeter, 1954, p.84), not for the new technology as in the patent race literature’s version of technological competition as set out by Dasgupta. The patent race literature cannot be validated by appropriating the theoretical and empirical tradition created by the quite different Schumpeterian literature.

Finally, Tirole, in a variant of the “accessorize” defence, comments (1988, p. 399); "the current (patent race) theories are much too rudimentary to be realistic". The potential fallacy here is to equate complexity with realism. Myths and legends can become more refined and complex over time, but that does not make them any more realistic. If your foundations are structurally and irredeemably flawed, you do not make the construction safer by adding a more complex superstructure. That is not just good building practice, it is good model-building practice.

9. Taking Things to the Logical Conclusion

> *Every day we witness the phenomenon: young elite pupils who have signed up for their Game course without any special ardour, and who have completed it dutifully, but without enthusiasm, are suddenly seized by the spirit of the Game, by its intellectual possibilities, its venerable tradition, and become our passionate adherents and partisans.* Herman Hesse, The Glass Bead Game, 1943

> *To know game theory is to change your lifetime way of thinking*, Paul Samuelson[21], Economics Nobel Laureate, 1999.

The fundamental problem with Game Theory is that it has no “off switch”. The existence of “off switches” is a prerequisite for intellectual progress. In science, the off switch may be refutation; in the arts, the off switch may be reputation. In science, a theory such as Lamarckism may be refuted; in the arts, a philosophy such as Freudian psychoanalysis may fall out of favour. It is critical reasoning in its different forms that helps provide the off switches in these very different contexts, but dogma is antithetical to critical reasoning and eliminates off switches.

The dangers inherent in the elimination of off switches are now evident in what has happened in economics, particularly in an area like industrial organisation. This is exemplified by Binmore who argues;

> “The dismal science is supposedly about the allocation of scarce resources. If resources are scarce, it is because more people want them than can have them. Such a scenario creates all the necessary ingredients for a game. Moreover, neoclassical economists proceed on the assumption that people will act rationally in

> this game. In a sense, neoclassical economics is therefore nothing other than a branch of game theory" (1992, p.14).

It is instructive to analyse these five sentences and their relationship to each other. The first two sentences are uncontroversial though the "supposedly" in the first sentence is redundant. The third sentence is fallacious, these are not "all the necessary ingredients for a game", a game as commonly described by Game Theorists typically makes very strong assumptions regarding the nature of the game, the players, outcomes, and levels and nature of knowledge possessed by players. The fourth sentence is another example of Petitio Principii and circular reasoning, by now Binmore has assumed that a game exists and that the role of neoclassical theory is to study this game. The final sentence completes the process with a non-sequitar, arguing that because neoclassical theory studies what is now assumed to be a game using the assumption of rationality, that it must be a branch of Game Theory. In fact, much of neoclassical economics is concerned with scenarios that realistically would not be described as Game Theoretic, such as monopoly and perfect competition. However, Binmore does not see this as a problem since he views monopoly as a game with one player, and perfect competition as a game with an effectively infinite number of players (1992, p.14). This solves that logical problem at the expense of another one, potential tautology, since it now appears to effectively interpret Game Theory as any study of human action which invokes the assumption of rational behaviour.

That this interpretation is warranted is supported by Binmore's view of the proper role and status of Game Theory in the study of Man; "indeed, one could argue that all the social sciences are nothing more than subdisciplines of game theory (Binmore, 1992, p. 3). But does that not mean that Game Theorists are the people to ask for answers to all the world's problems? Binmore at this point dissents from going down that path and answers in the negative; "this is because game theory <u>as currently developed</u> is mostly about what happens when people interact in a <u>rational</u> manner" (Binmore, 1992, p. 3, second set of italics in original, first set added).

But if Binmore's view of the potential scope and possible future importance of Game Theory was indeed a reasonable one, then all students in the social sciences should study Game Theory as a foundation class. Dixit and Skeath would endorse this recommendation, and indeed go further;

> "We believe that there is a strong case for reversing the usual order whereby general introductory courses in each subject are followed by advanced subject-specific courses in Game Theory. In the more natural progression, all students interested in the social and biological sciences would complete a freshman course in elementary Game Theory before going on to more detailed study of one of the specialized fields. Students intending to specialize in the natural sciences would also find Game Theory a more interesting and useful way to satisfy their distribution requirements than many introductory courses in particular social sciences." (Dixit and Skeath, 1999, p. xx).

In short, Dixit and Skeath are putting forward Game Theory as either essential or highly desirable foundation introduction for all students studying social, biological and natural sciences. At first sight an agenda based on claims to be a near-essential across-the-board intellectual foundation based on a single "theory" would seem to have no precedent, certainly in contemporary Western universities. Unsurprisingly, the nearest parallels in history tend to be based on dogma, whether Marxism in communist countries past and present, or the religious doctrines that characterised Medieval European universities.

The First Amendment of the US Constitution guaranteeing the separation of church and State and its fervently anti-communist culture has helped create legal and social barriers to the infiltration of its educational system by dogmas such as creationism and Marxism. But there is no parallel amendment to prevent the infiltration of educational systems by the "culture" of Game Theory in the US and elsewhere. In recent years it has spread from being a specialist research area practiced by few, to being taught as core material at intermediate levels in mainstream economic degrees, and if the agenda set out by Dixit and Skeath is fulfilled, it will become a compulsory foundation subject for first year students wishing to go on to studies in social and biological sciences, and an important option for students in the natural sciences. Such saturation coverage of foundation science teaching would inevitably lead to it trickling down to school level and eventually challenging the dominance of such subjects as biology, history and physics in the school curriculum.

We are clearly a long way from even approaching such a situation, and it might seem fanciful or even ludicrous simply to contemplate it. But several facts should at least give pause for thought. Firstly, Dixit and Skeath are highly respected and able academics and it must be presumed that they are serious about what they see as desirable developments for all students in the social and biological sciences. Secondly, their work has the explicit endorsement of many leading economists, including two Nobel Prize winners, Samuelson and Nash[22] with Nash explicitly lauding the proposal to teach Game Theory "at the earliest stages of the undergraduate curriculum" as "progressive and praiseworthy". Thirdly, even just two decades ago, few would probably have realised or expected Game Theory to have taken such a firm hold in the heart of much of the curriculum and research agenda in modern economics, so it may be a mistake to think that the extent of its influence in teaching and research will stabilise at current levels. For such reasons, it may be wise to at least treat Dixit and Skeaths' aspirations seriously.

But Dixit and Skeaths' proselytising zeal and empire-building aspirations for Game Theory pale in comparison to sentiments expressed by Binmore in his concluding paragraph;

> "(Game Theory) may well revolutionize the way we run our societies one of these days. Perhaps it may be optimistic to look forward to a time when the stupidity, ignorance and prejudice with which we currently run our affairs withers away under the cool light of reason" (Binmore, 1992, p.602).

The fallacy and indeed the dangers inherent in such a belief are bound up in its implicit self-referentiality. Just because Game Theorists study a form of rational decision-making does not necessarily mean that it provides, or could provide, adequate positive and/or normative models of strategic behaviour. The evidence of this paper is that it has thoroughly failed in this enterprise so far, with little reason for a belief that this state of affairs could improve. Further, it is also the view taken in this paper that the very human characteristics described by Binmore here are not confined to the world outside of Game Theory but can in fact be a characteristic of Game Theorists themselves, which should raise warning flags against entrusting human affairs to their advocacy or custody. At one level it is comforting to know that they are only human like the rest of us (bearing in mind "only human" does not mean "only rational"), at another it is always worrying when an individual or group claims to be able to point the way to knowledge and enlightenment and wishes to change the inferior ways that societies think and behave to bring them in line with their views. While it is a legitimate aspiration to change the way mankind thinks and behaves, that is the role of institutions and disciplines such as education, philosophy, culture and politics, not a sub-branch of mathematics.

It is the view here that there is no more case for according Game Theory the status it has acquired in economics, and the enhanced status that Dixit, Skeath and Binmore wish for it, than there would be for according creationism such status in university teaching. Both are dogmas, and the way that Game Theory has been practiced and preached by its proselytisers is essentially antithetical in many respects to the principle and practice of critical reasoning. To require students to take a course in Game Theory before they go on to study real sciences would be to potentially undermine the training and development of social and other scientists no less than would requiring them to take a course in creationism. Therein lies the danger with all dogma, once students and professionals have been inducted and have undertaken what can be the arduous and demanding work required to comprehend and master the complexities and refinements of the dogma, it may create a rigid mental set or predisposition to interpret the world using the templates and frames of reference provided by the dogma, converting the casual inquirer into active proselytiser.

Indeed, it could be alleged that the effect of teaching Game Theory could be even more damaging; at least creationism can be seen and identified as dogma by embryonic scientists, while Game Theory is more difficult to identify as such since it has adopted much of the mathematical tools, techniques and jargon of the natural sciences.

Even that most fair-minded of observers, Colin Camerer is guilty of fallacious reasoning when he asks the question; "does strategy research need Game Theory?" and answers; "it wouldn't hurt" (1994, p.218). But there has been, and would still be, considerable hurt through the misallocation of intellectual resources using this distorting prism. And that is before its crowding out of alternative economic, social, psychological and institutional research (and researchers) is taken into account.

So what is the proper strategy for dealing with these problems? The answer is the way that dogma in other contexts is dealt with, by reasoned criticism and challenge. A few

years ago I interrupted a seminar to my department by a leading Game Theorist to point out that there was a possible problem with the n-member cartel in his model in that, in reality, free riding was likely to be an increasing danger the larger that "n" became. He replied that, yes, that can be a major problem in the real world, but it was not a problem in his model - and went back to his exposition. The proper response at that point would have been; "what is your model for?", but I said nothing. Afterwards, one of my colleagues said to me; "what did you expect? He's a Game Theorist", to which the proper response would have been; "what should we expect? We are an Economics Department", but I said nothing.

It may be too late to stop the colonisation of economics and economics departments by Game Theory, which if nothing else shows the consequences of saying nothing. But as long as some point out loudly and strongly enough that the new emperor is inadequately clad, some protection may be afforded for the other social sciences where it has less of a foothold, at least for the moment. And who knows, perhaps at some point in the future, critical reasoning will once again find a strong voice in economics departments and say clearly that which was for too long only whispered, if it was voiced at all; w*as this game really the highest, really the sovereign in the realm of the intellect? Was it not, in spite of everything, in the end merely a game after all?* Herman Hesse, The Glass Bead Game, 1943

REFERENCES

Andreoni, J and J. H. Miller (1993) Rational cooperation in the finitely repeated Prisoner's Dilemma: experimental evidence, Economic Journal, 103, 570-85

Andreoni, J. and H. Varian (1999) Preplay contracting in the Prisoners' Dilemma, Proceedings of the National Academy of Sciences, 96, pp. 10933-10938

Ashley, M. (1973) The Life and Times of William I, London, Weidenfeld and Nicholson.

Besanko, D., D. Dranove, M. Shanley and S, Schaefer (2004) Economics of Strategy, 3rd edition, New Jersey, Wiley.

Binmore, K. (1992) Fun and Games: a Text on Game Theory, Lexington, D. C. Heath

Brandenburger, A. M. and B. J. Nalebuff (1996) Co-opetition, London, Harper/Collins

Camerer, C. F. (1994) Does strategy research need game theory?" In Rumelt, Schendel and Teece (eds. (1994), 195-219.

Chow, T. Y. (1998) The surprise examination or unexpected hanging paradox, American Mathematical Monthly, 105, 41-51.

Dasgupta, P. (1986) The theory of technological competition, in Stiglitz, J. E. and G. F. Mathewson (eds.) New Developments in the Analysis of Market Structure, London, Macmillan, pp.519-49.

Dixit, A,. and Nalebuff, B (1991) Thinking Strategically: the Competitive Edge in Business, Politics and Everyday Life, New York , Norton

Dixit, A,. and Nalebuff, B. (1993) B. Game Theory , The Concise Encyclopedia of Economics, The Library of Economics and Liberty, online entry at: http://www.econlib.org/library/Enc/GameTheory.html

Dixit, A and S. Skeath (1999) Games of Strategy, New York, Norton.

Douglas, D. L. (1964) William the Conqueror: the Norman Impact upon England, London, Eyre and Spottiswode.

Ferraro, F., J. Pfeffer and R. I. Sutton (2005) Economics language and assumptions; how theories can become self-fulfilling, Academy of Management Review 30, 8 -24.

Frank, R., T. Gilovich and D. Regan (1993) Does studying economics inhibit co-operation? Journal of Economic Perspectives, 7, 159-71

Gould, S. J. (1991) Bully for Brontosaurus, London, Penguin.

Hassig, R. (1994) Mexico and the Spanish Conquest, London, Longman.

Hesse, H. (1943) The Glass Bead Game, New York, Henry Holt

Kahan, J. S. and A. Rapoport (1984) Theories of Coalition Formation. Hillsdale, NJ, Lawrence Erlbaum Associates Publishers.

Kay, A. C. and L. Ross (2003) Nonconscious influence and conscious refection; the interplay of implicit cues and explicit situational constraints in the prisoner's dilemma, Journal of Experimental Social Psychology, 39, 634-643

Kay, N. M. (1997) Pattern in Corporate Evolution, Oxford, Oxford University Press.

Kreps, D. M. (1990) Game Theory and Economic Modelling Oxford, Clarenden Press.

Liberman, V., S. Samuels and L. Ross (2004) The name of the game: predictive power of reputation vs. situational labels in determining prisoner's dilemma game moves, Personality and Social Psychology Bulletin, 30 1175-1185.

Mathew, D. J. A. (1966) The Norman Conquest, London, B.T. Batsford

Milgrom, P. and J. Roberts (1992) Economics, Organization and Management, New Jersey, Prentice Hall.

Neumann, J. v. and O. Morgenstern (1944) The Theory of Games and Economic Behaviour, New Jersey, Princeton University Press.

Postrel, S. (1991) Burning your britches behind you: can policy scholars bank on game theory? in Rumelt, Schendel and Teece (eds.) (1994) 221-24.

Rasmussen, E. (1989) Games and Information: an Introduction to Game Theory, Blackwell, London.

Romp, G. (1997) Game Theory: Introduction and Applications, Oxford, Oxford University Press

Roth, A. E. (1988) Laboratory experimentation in economics: a methodological overview, Economic Journal, 98, 974-1031.

Rumelt, R. P., D. E. Schendel and D. J. Teece (eds.) (1994) Fundamental Issues in Strategy, Boston, Harvard Business School.

Saloner, G. (1994) Game theory and strategic management: contributions, applications, and limitations, in Rumelt, Schendel and Teece (eds. (1994), 155-94.

Scherer, F. M. (1992) International High Technology Competition, Cambridge, Harvard University Press.

Schumpeter, J. A, (1954) Capitalism Socialism and Democracy, 4th edn. London, Unwin.

Sorensen, R. A. (1988) Blindspots, Oxford, Clarendon Press.

Teece, D. J. Pisano, G and A. Shuen (1997) Dynamic capabilities and strategic management, Strategic Management Journal, 18, 509-33.

Thaler, R. (1988) Anomolies; the ultimatum game, Journal of Economic Perspectives, 2, 195-206

Thomas, H. (1993) The Conquest of Mexico, London, Hutchison/

Tirole, J. (1988) The Theory of Industrial Organization, Cambridge, MIT Press.

Turner, H. A. Jr.(1992) Germany from Partition to Reunification, New Haven, Yale University Press

ENDNOTES

[1] See Kreps (1990) for discussion of the basic concepts of Game Theory.
[2] See Kreps (1990, pp.95-102) for discussion of the problem of multiple equilibria in Game Theory models.
[3] See Saloner, (1994) p. 157.
[4] See Tirole's arguments below that patent race models are still too rudimentary.
[5] See, for example, the discussion in Teece, Pisano and Shuen, 1997, p. 512.
[6] The debate as to whether or not Game Theory can be falsified is in many respects a continuation of a long standing debate as to whether or not rational choice models in economic theory can be falsified. This is often set in the context of Poppers advocacy of the importance of falsification in science. See Blaug (1980) for further discussion.
[7] For example, see Gould (1991), pp.455-56
[8] See, for example, Kreps (1990).
[9] See, for example, Camerer (1994).
[10] See, for example, many of the articles in the "Anomalies" series published by Thaler (eg. Thaler 1988) and associates in the Journal of Economic Perspectives since 1987.
[11] It must be said that not all those describe themselves as Game Theorists would agree with Binmore here.
[12] For example, by introducing the probability that the player has an altruistic opponent (Andreoni and Miller, 1993)
[13] See also discussion of these two studies in Ferraro, F. J. Pfeffer and R. I. Sutton (2005).
[14] Comments by R. P. Rumelt originally made in discussion at conference, Fundamental Issues in Strategy; a Research Conference, Napa, California, 1990
[15] For those few who may be unaware, Homer Simpson is a fictional character in a cartoon TV series.
[16] I could find no evidence of a second substantive point here.
[17] Ironically, one of the authors subsequently cites Thomas and the possible quemando/quebrando confusion (Brandeburger and Nalebuff, 1996, p.166) but does not really go beyond that in terms of setting the historical record straight, and indeed maintains much of the confusion by referring back (pp.275-76) to Dixit and Nalebuff's analysis (1991) "for more on the strategic analysis of this story", which just helps perpetuate what is largely a closed, circular and flawed treatment of historical evidence here.
[18] See Kay (1997) for more detailed discussion of these issues.
[19] Company information.
[20] Company information.
[21] Endorsement on the cover of Dixit and Skeath (1999)
[22] On the back cover of the book.