

# WORKING PAPERS IN ECONOMICS

MODELLING RATIONAL CONFLICT:  
THE LIMITS OF GAME THEORY\*

by

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1. *Introduction: The promise of Game Theory*

'Why do people fight?' is the simplest variant of a question reflecting the theme of this paper. Due to its dramatic impact, conflict has traditionally captured the imagination of scholars who felt the need to delve into its causes long before economists puzzled over relatively innocuous problems such as inflation and unemployment. Nevertheless, the merits of economics are often presented in terms that the non-economist student of conflict can relate to.

Economics is canvassed as the study of how agents come to an automatic settlement of antagonistic interests caused by scarcity. It is when agents relentlessly strive toward their personal interest, with little or no concern for the social effect of their choices, that the public good is best served. The economist construes the ideal social world as one surfacing because, rather than in spite, of the pervasiveness of the individual's belligerence. The key to the prevention of the transformation of those 'natural' tendencies into conflict is, of course, the work of Reason. Provided the institutions of the market are in place, individuals harness their instincts and, in doing so, forge a splendid resolution free of wasteful activity. Paradoxically, conflict is at once the guarantor of optimal social outcomes and the cause of its own demise. Equilibrium and stability are, therefore, the byproducts of a social order founded on the disposition to fight for one's self. Philosophers will understand the above as the supra-intentional work of Reason, and recognise that economics claims to have done their work for them.

Indeed, one is excused to think that, as far as economics goes, an explanation of conflict is not urgent; conflict has been designed out of the system. Industrial strikes, hostile takeovers, oligopolistic wars between firms and trade wars between governments are all, supposedly, minor nuisances that do not threaten the miracle of the market. Just as the odd meteorite does not give cause for changing our calculations of the length of Mars' year, the waste of resources inherent in the advertising war between Pepsi and Coke may not be sufficient reason for questioning the market's ability to eradicate conflict. Be that as it may, if economics is to command respect within social science,

surely we must have something meaningful to say about the occasional emergence of strife; after all astronomers do not remain mute about minor celestial phenomena. It is the contention of this paper that, unlike astronomers, economists have great difficulty in this respect.

Given the large doses of interdependent behaviour that is required in an analysis of conflict, game theory is the natural heir of marginalist method and holds much promise for those who wish to underpin and expand the marginalist perspective. Moreover, the attraction to game theory is not confined to economics. Elster (1982,1986) is just one example of a non-economist who argues in favour of game theoretic microfoundations for history and sociology. Most social scientists are, naturally, undecided. There are those who, like Elster, enthusiastically espouse game theory but also those who cannot justify the investment that would be required before they master it, either in order to use it, or for the purposes of articulating an informed critique of its use. The small question that started this paper can act as a catalyst for an assessment of who is right.

At this point a precise definition of conflict is essential. When an agent acts in a manner that destroys part of a valuable resource with a view to enhancing personal gain, we can safely claim to have observed an instance of conflict. Wars and strikes fall under this category. However, there is another less obvious category. When agents fail to reach an agreement that would have given rise to a net increase in the joint stock of wealth, conflict is again in the air. Political commentators, for example, would correctly describe a failure by European Community ministers to establish a free-trade protocol as a case of conflict. However, this definition may have licensed too much. One could protest that the second category classifies every process leading to non-Pareto outcomes as being indicative of conflict.

In fact, if we accept this definition, the small question on conflict touches upon some very thorny issues. Every dynamic disequilibrium process entails some Pareto loss before equilibrium is established. In terms of my definition of conflict, there is a relevant dimension in economic theory going back to Adam Smith, Ricardo and Marx. Classical economists, for instance, saw the

equilibrium level of prices ('natural' for Smith, 'productive' for Marx) as a function of the type of adjustment. In more recent terminology, the out-of-equilibrium behaviour of the system shapes the actual equilibrium. Consequently, an identification of conflict with welfare losses confers an abstract view of the market economy where conflict, though transient, plays a crucial role in determining the kind of harmony to which the choices of sovereign agents inexorably lead. Game theory is seen by many as an opportunity to provide a rationale to such quasi-functional speculation.

The essence of game theoretic reasoning is that agents intelligently assess the effects of their choices on the behaviour of their opponents before acting. This is a commendable departure from traditional myopic reaction functions but, unfortunately, is not enough. Progress along the game theoretic path requires that agents replicate each other's thoughts. The presumption that rationality is in place and commonly known allows game theory to use the notion of equilibrium in order to cut the Gordian knot of interdependent behaviour. Unfortunately, the same presumption undermines the relevance of the theory.

Although equilibrium conflict may sound like a contradiction in terms, non-cooperative game theory gives it its head. If I think (a) that you can replicate my thoughts, (b) that whatever I do you are better off shunning peace in favour of violence and (c) that if you choose violence I am better off doing so myself, then violence is the equilibrium outcome. Nothing can prevent this instance of counterfinitality from conducting war and driving a wedge between the Pareto and the Nash equilibrium outcomes. However, some equilibrium outcomes are more paradoxical than others. Confronted with the threat of an attack, our agent is alarmed. If the potential aggressor is kind enough to announce in advance that the assault will occur either today or tomorrow but that it will only take place provided that it is not anticipated with certainty on the day when it will occur, our agent will be reassured. She has reasoned that if the attack does not eventuate today then it cannot take place tomorrow either because it will be anticipated, thus violating the condition for the attack. Moreover, given that it cannot occur tomorrow, today is ruled out for the same reason; the paradox of backward induction. According to game theory's

equilibrium approach, our agent is right to feel safe. Ironically, it is because the assault can now take place without bending the rules that equilibrium thinking may not be appropriate.

Cooperative game theory (ie. the study of games where players can reach an agreement before acting) offers fewer opportunities for a theory of conflict. Suppose two individuals are locked into a dispute over a resource they both value. Can we develop a theory capable of predicting (a) whether war will erupt and (b) its history if it does? Suppose we can. The assumption of rationality is seen as tantamount to the proposition that agents are also capable of developing this theory. If they are, then they can unmistakably predict the outcome of any impending confrontation. Like two chess masters who abandon a game whose outcome is no longer in doubt, our two rivals will see no reason in fighting and will settle their difference peacefully. Hence, a theory of conflict becomes both impossible and unnecessary, unless this paradox of foreknowledge is breached.

So far, the simple application of equilibrium reasoning has spawned two paradoxes. The rest of the paper focuses on their implications for a game theory of conflict.

## 2. Conflict and Equilibrium I: non-cooperative games

The proliferation of solution concepts in game theory makes it impossible to offer more than a glimpse of its uses in the space available here. Nonetheless, it is possible to glean its substance via an eclectic discussion. In this section I examine a simple notion (subgame perfection) which allows the game theorist to weed out a number of eventualities that the uninitiated would not instantly discard as incompatible with rational play.

Consider the following depiction of a four stage game where each player chooses sequentially between war and peace.

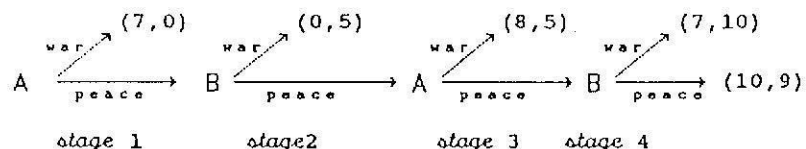


TABLE 1

A starts first and, provided conflict is kept at bay until the end, B signs off. No communication is allowed between A (who is female) and B (who is male) other than through their choices. Transcribing the same game into matrix (normal) form - table 2 -, we can see that there are two Nash equilibrium solutions to this game. The first has A going for war immediately [outcome (7,0)], while the second results in two periods of peace shattered by war when A gets her second chance [outcome (8,5)].

	WW	WP	PW	PP	
WW	7,0*	7,0*	7,0	7,0	
WP	7,0*	7,0*	7,0	7,0	
PW	0,5	0,5	8,5*	8,5*	(where * indicates
PP	0,5	0,5	7,10	10,9	a Nash equilibrium)

TABLE 2

Being evidently more tempting than the first, the second equilibrium should compel both players to espouse it. And yet, a logic similar to that which earlier gave rise to the paradox of backward induction, castigates this as muddled thinking. Since B will always choose war at stage 4, A knows that to play peace at stage 3 is to invite aggression (and thus payoff 7) when she can declare war at stage 3 and instantly get 8. B is expected to have thought so himself at stage 2 and, therefore, to have concluded that he has nothing to gain from prolonging the game. A has, naturally, anticipated all this and, at stage 1, will have a clear-cut choice between payoff 7 now or 0 at stage 2. Hence, A wrecks the prospects of peace right at the outset.

In game theory's jargon, the above deduction of a single equilibrium solution out of a set of two Nash candidates, is the result of requiring that a strategy should be in equilibrium not only for the whole game (both (7,0) and (8,5) are) but also for each subgame, a condition only satisfied by (7,0). This requirement of subgame perfection was proposed by Selten (1975) and proved a useful tool for significantly reducing the number of rationally playable strategies. The underlying logic is the same the one which in the previous section generated the paradox of backward induction.

To recap, we see that games such as those in table 1

demonstrate the compatibility between rationality, the equilibrium approach and conflict. Reason is presented as incompatible with anything other than conflict provided rationality is commonly known and agents have no means of prior communication. Are things different when communication and agreement prior to playing the game is permitted? In the next section I argue that although the implications of communication for conflict are pivotal, cooperative game theory's conclusions are highly suspect for reasons that also undermine the above analysis.

### 3. Conflict and Equilibrium II: cooperative games

In the game of table 1, the danger of conflict would have diminished, if not eradicated, provided agents had the opportunity to negotiate an enforceable agreement prior to playing the game. The bargaining problem is best portrayed in its simplest form. Two agents have their eyes firmly set on a unit of value which they both desire. However, unless they come to an agreement as to a division, neither gets any part of it. Bargainers target not physical shares of the unit 'pie' but, instead aim at the utilities that these shares bring. The convex possibility set in table 3 encases all feasible combinations of utility for each division between the two bargainers.

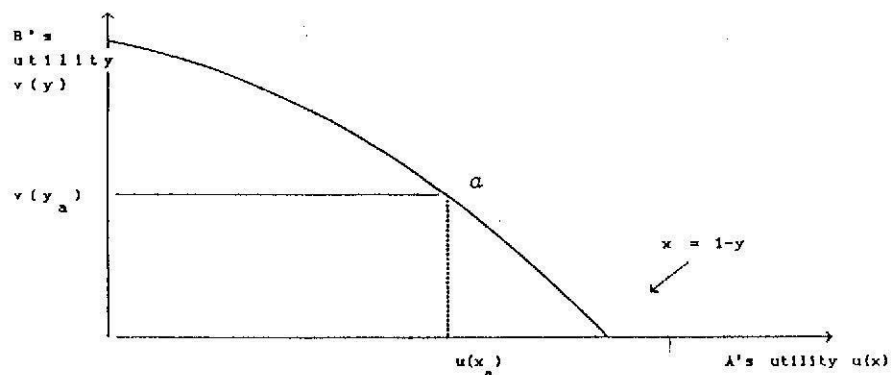


TABLE 3

Letting  $x$  and  $y$  be the final shares of A and B respectively, their objective functions are  $u(x)$  and  $v(y)$ . The only constraint faced by either is the threat of a failure to agree (ie. the threat of conflict) in which case utilities are assumed to collapse to zero. In the simplest version of the model, players submit sealed bids to an umpire. If they are compatible, both get what they asked for; otherwise no pie is distributed. Can we have a theory of what bids rational bargainers will choose? The problem here is that all  $(x,y)$  combinations such that  $x+y = 1$  are Nash equilibria. In his 1950 and 1953 papers Nash proposes the axiomatic approach:

"One states as axioms several properties that it would seem natural the solution to have and then one discovers that the axioms actually the solution uniquely" [1953].

The spirit, but not the letter, of Roth's (1979) reconstruction of the Nash program is adopted in what follows. Nash seems to have reasoned that a cooperative solution must maximise a weighted average of the bargainers' utilities subject to the feasibility constraint.

$$\max_{x,y} \{ au(x) + bv(y) \} \quad a,b \in (0,1) \quad (1)$$

The above is rather innocent since it does not determine either the fate of conflict or the type of distribution. All it advocates is that, if the two agree, we should expect them to attain a quasi-Benthamite division where the weighted average of utilities is greatest. In order to go further than this, a sequence of axioms is needed. I will not scrutinise each one of them, since this is not the purpose of this paper, but will offer some clues as to why neither conflict nor the question of distribution find their definitive explication in this, so-called, Nash program.

Note that, in contradistinction to the non-cooperative case, Nash equilibria are also Pareto optimal (ie.  $x = 1-y$ ). The first order condition for Pareto optimality maximises (1) s.t.  $x = 1-y$ , and is given by

$$au'(x) = bv'(1-x) \quad (2)$$

Before (2) can be used to narrow down the set of Nash

equilibria, the sovereignty of the utility concept needs to be cemented by an axiom guaranteeing the irrelevance of utility calibrations (IUC) (this the axiom is also known as linear transformation invariance). The latter states that A and B will automatically translate each possible division from physical (or nominal) to utility units and will not be at all influenced by considerations that would have been relevant had players been mindful of non-utility considerations. Those who have doubts about the sovereignty of utilities in determining motivation will wish to challenge IUC. However, they would be wise to turn to another axiom, the celebrated independence of irrelevant alternatives (IIA), as a more fruitful ground on which to do battle against the Nash project.

The IIA axiom asserts that, when solutions that agents would not have chosen become infeasible, the outcome is not affected. This brings to mind the consequentialist failure to acknowledge that the feasible set may be as important as the consequences of choice. The IIA axiom seems to be equivalent with the argument that after having chosen to stay at home rather than go to the cinema, the imposition of a dusk to dawn curfew that makes cinema going illegal will never, by itself, reverse one's preferences.

Before digging deeper into the pertinence of Nash's axioms, let us see how game theorists derive the unique solution to this bargaining problem. With axioms IUC and IIA in place, we seek a solution with the specified properties. Looking at (2), if we know the ratio of utilities that bargaining would yield (ie. if we know  $a/b$ ), the solution is easily derived. Note that if  $au(x) = bv(y)$ , players receive identical increments of utility when compared with the conflict outcome. However, because there is no reason to expect that A and B will receive identical increments of utility over and above the conflict outcome, it is preferable to write

$$(1-k)au(x) = kbv(1-x) \quad \text{where } k \in (0,1) \quad (3)$$

The nearer  $k$  is to one, the greater the weight placed on A's utility and, hence, the more her 'power'. At this point I would refrain from taking the term 'power' too literally lest it is given a wider interpretation than it deserves. More on this shortly.

We are now just a small step away from deriving the unique

solution to the game. Substitution of (3) in (2) yields

$$kv(1-x)u'(x) = (1-k)u(x)v'(1-x)$$

which is solved by the final share of A  $x^*$  defined as

$$x^* = \underset{x}{\operatorname{argmax}} \{u(x)^k v(1-x)^{1-k}\} \quad (4)$$

We seem to have reduced the infinity of Nash equilibria to a single candidate. Nonetheless, continual alterations of  $k$  trace the entire possibility frontier and return us to square one. Moreover, Roth (1979) shows that if we are not prepared to follow Nash in banning conflict outright (by assuming agreement and Pareto optimality), but instead specify that if there is an agreement it must be Pareto optimal, then conflict remains a possible outcome. Nash champions the salience of  $x^*$  by imposing a priori both Pareto optimality and symmetry. The former assumes conflict away while the latter waives all differences between the two sides that are not subsumed in their utility functions. If this is accepted, neither player has more power,  $k$  equals 1/2 and the Nash solution maximises the product of the two utility functions. Furthermore, conflict never materialises.

Close inspection of (4) reveals that the less you have to lose from conflict relative to your opponent and the less pronounced your disappointment from failing to make a small gain compared to your disappointment from failing to make a larger gain, the greater your reward. As a predictive model, the Nash solution predicts the triumph of the strong and the risk-loving at the expense of everyone else. The fact that the ethical implications may be iniquitous is, of course, no reason for questioning the logical consistency of a positive theory. However, in this case what can be readily questioned is the claim that we are dealing with a positive theory.

If Nash's solution is to be accepted as a predictive device, two conditions must hold. First, the underlying axioms must be beyond reproach and, second, we should be convinced that to know one's utility function with certainty is to know her motivation. The second condition implies that starting with imperfect knowledge of your opponent, and as you begin to learn more about them, then in the limit your behaviour can be explained by a model

built on the presumption of perfect information. Indeed, there is a large imperfect information literature<sup>1</sup> which seeks bargaining and non-cooperative outcomes in the case where utilities are partly shrouded in mystery. Indeed, when game theorists are reminded that conflict cannot happen in the simple Nash game, they immediately retort "ah, but conflict is due to asymmetric information and it is asymmetric information models to which we must turn for a theory of conflict". I doubt that this is true.

Elsewhere<sup>2</sup> I have argued that a theory of conflict as the result of asymmetric information concerning the model's parameters is unsatisfactory. The problem is that the equilibrium approach turns the task of squeezing some conflict out of a conventional model into one of Sisyphean proportions<sup>3</sup>. Recall the paradox of foreknowledge. If a theory could predict conflict in circumstances of uncertainty concerning some parameter(s), hyper-rational agents would not fight; instead they would use the theory and settle at the first available opportunity accordingly. Is this paradox the end of the road or can we have a sensible theory of conflict?

Let me focus on the first condition for accepting Nash's solution. Both the IUC and IIA axioms could be otherwise. What they bring to the model is a degree of consistency which is necessary for a general solution. Binmore believes<sup>4</sup> that "...some consistency condition is clearly necessary in order to obtain a result...what is not so clear is why the independence of irrelevant alternatives is an appropriate condition". Take as an example a perfect information two stage game where in the first stage players split a pie which in utility terms translates into the game in table 3. The symmetric (ie.  $k=1/2$ ) Nash equilibrium corresponds to point a and, if Nash is right, will characterise the outcome at stage 1. In the second stage, players are given an identical pie to divide, only this time they are told that A cannot get less than  $x_a$  (ie. what she got before) and B cannot get less than half of his previous share  $1-x_a$ . What should we expect to happen?

On one account, it can be argued that the redefinition of the feasible set favours A and, therefore, that A should now expect a bit more than before. However, the opposite case can be argued, namely that since the two players are identical to their earlier selves and the outcome they would have reached without

outside influence is still permitted, they should, once more, settle for  $x_a$  and  $1-x_a$  respectively. This second account is what Nash assumes by imposing the IIA axiom. As a convention it makes sense but not more so than the first account which has a player favoured by the rules expecting to reap the benefits of this advantage. Neither convention can be singled out as more efficient (both yield Pareto optimal results) or rational and, thus, it is not clear why agents should invariably expect their opponents to act as if it could.

Critics of IIA and its claim to universal predominance, eg. Luce and Raiffa (1957), Kalai and Smorodinski (1975) and Binmore (1987a), propose alternative conventions. However, they have not explicitly denounced Nash's attempt to force a single convention upon a specific bargaining situation. It is important, I believe, to acknowledge that axioms decreeing a single convention are incompatible with the assumption of agents' rationality. The only way of procreating an axiom similar to IIA in Nash is by requiring that agents have a preference for it; but this would be absurd.

Scepticism over IIA is not only due to the fact that players may experience difficulty in coordinating expectations with regard to the eventual convention. For they may also work towards the establishment of their preferred one given the differences in rewards depending on which convention prevails. Conflict may, thus, be observed in repeated division games irrespective of the degree of conventional asymmetric information. My earlier reluctance to accept  $k$  as an index of 'power' stems from the observation that when two equally rational conventions each result into different distributions, power takes on a new meaning that the Nash program cannot fathom. It becomes a measure of one's ability to select the rules of the game. I think conflict is better examined in this context rather than in the Nash game theoretic framework where it is axiomatically abolished and where power is reduced to comparing the first order derivatives of utility functions.

Earlier I stated a second condition that must be met before the game theoretic approach is justified, namely that to know one's utility is to have a complete guide to their motivation. The above discussion points to non-utility determinants of behaviour by illustrating that it is unwise to expect individuals to

converge to the same way of thinking when there is no mechanism which can facilitate this convergence. The economist's instinct to adopt equilibrium notions has a long history but should be tempered in game theory.

There are a number of different rejoinders to the above rejection of the Nash approach. All of them utilise a procedural logic and fall into two categories. The first category involves the introduction of some 'noise' into the system. Either the game is perturbed through the interjection of shared uncertainty about the location of the utility frontier [see Nash 1953, Binmore 1987a], or players' decisions are contaminated by random errors [see Selten 1975]. In both cases, it is shown that, as these exogenous disturbances subside, (4) turns out to be the bargaining solution. However, the fact that the Nash solution has been reached without recourse to the axioms criticised above is neither here nor there. The reason why perturbations lead to (4) uniquely lies in their curious nature. If they are meant to be representative of the endemic uncertainty of players about what to do when faced by different conventions (or what to infer when one's opponent makes a mistake), then they should be endogenous to the model. By assuming exogeneity and postulating that agents latch on to the same probability distribution which governs these perturbations, we are effectively re-applying the groundless assumption that conventions will automatically be coordinated.

Rejoinders belonging to the second category adopt a sequential bargaining framework in aid of Nash's defence. The idea is to show that, given sufficient latitude to exchange offers and demands, players will avoid conflict and will, unassisted, converge to the solution. Rubinstein (1982) changed Nash's institutional arrangements so that, rather than having players submitting demands simultaneously, demands are issued in sequence. A stakes a claim which B can accept, thus ending the game. If he rejects it, he then has to make a counter-offer which, in turn, is either accepted or rejected. Agents are free to continue bargaining forever the snag being that, every time an offer is rejected, the value of one's share is reduced by a fixed proportion. So, conflict is not a binary event as in Nash's original game. Instead, it comes in various magnitudes ranging from zero (instant agreement), to a moderate level (a few rejections followed by

agreement) and, in the worst scenario, to perpetual disagreement. The earlier question, 'will there be conflict?' is therefore augmented to 'will there be conflict and, if yes, what intensity will it have?'

Let A and B be risk neutral and suppose that at time  $t=0$  their utility coincides with their (discounted) share. As time goes by, the same satisfaction associated with a certain payoff at  $t=0$  would require a larger payoff. Put simply, time is money and the earlier an agreement is reached the more it is valued. Letting  $d_a$  and  $d_b$  be the discount factors, players' utilities are given by  $x d_a^t$  and  $(1-x) d_b^t$  respectively. A proposes at  $t=0$  that she gets  $x_0$ , leaving  $y_0=1-x_0$  for B. If B is unimpressed, he will register his disagreement and move the bargaining process to  $t=1$ . Then, he proposes an alternative split  $(x_1, y_1)$  throwing the ball in A's court. If A rejects this offer, we move on to  $t=2$  and so on.

Rubinstein's (1982) ingenious solution is based on the following train of thought. Suppose we have reached  $t=2$  where A is required to make an offer. While pondering the choice of proposal, she must have in her mind an expectation of what she will get from the remainder of the game. Let this expected return be denoted by  $W$ . B is assumed capable of replicating her thoughts and he too expects her to get  $W$  if  $t=2$  is reached. Hence, at  $t=2$  he expects no more than  $1-W$ . This assumption provides Rubinstein with the foothold he needs before utilising a logic of backward induction similar to the one employed in the game of table 1. Once it is established that at  $t=2$  B expects to get at most  $1-W$  and A expects  $W$ , Rubinstein takes us to the previous stage ( $t=1$ ) and argues that A will reject B's offer  $x_1$  if it is less than  $d_a W$ . Equivalently, B will at  $t=1$  offer A at most  $d_b W$  knowing that the latter value is exactly as satisfying for A as anything she could expect from rejecting this offer and moving into  $t=2$ .

Now B faces a dilemma. If at  $t=1$  he offers A  $d_b W$ , she will certainly accept it thus leaving him with  $1-d_b W$ , the value of which, when assessed at the outset ( $t=0$ ), is  $d_b(1-d_b W)$ . If his offer is lower, it will be rejected forcing negotiations into stage  $t=2$  when he can expect  $1-W$ . The value of the latter at  $t=0$  is  $d_b^2(1-W)$ . Since the latter is less than the former, Rubinstein argues that B, if rational, will offer A  $d_b W$  at time  $t=1$ . We now turn to period  $t=0$ .

At  $t=0$ , A is offering and B has the power to accept or to stand firm with a view to do better for himself at  $t=1$ . Logically, he will only choose to reject A's proposal if it is worth strictly less than what he expects to get at  $t=1$ , ie.  $d_b(1-d_a V)$ . Hence, A must decide on whether she wants agreement in this period, which she can have by offering B  $d_b(1-d_a V)$ , or whether she favours temporary impasse. Instant agreement will reward her with  $x_0 = 1-d_b(1-d_a V)$ . Disagreement at  $t=0$  will take her to  $t=1$  where she knows B will optimally offer her  $d_a V$ . As this share at  $t=1$  is worth  $d_a^2 V$  at  $t=0$ , and given that this is less than  $x_0$ , Rubinstein expects her to offer B  $1-x_0$  right at the beginning, and, moreover, expects him to always accept. Recalling that  $V$  was defined as A's optimal expected payoff for the whole game and also that the largest possible payoff that A can reasonably expect is  $1-x_0$ , the application of backward induction points to the equality  $V = 1-d_b(1-d_a V)$ . Solving for  $V$ , we derive the unique subgame perfect equilibrium solution for the bargaining problem when offers are made sequentially:

$$\text{A's optimal payoff} = V = \frac{1 - d_b}{1 - d_b d_a} \quad (5)$$

In (5) we have the remarkable result that conflict is ruled out not by assumption, or a unique convention, but by pure reason. It can, in fact, be interpreted as the formalisation of the paradox of foreknowledge in the context of equilibrium theory. Although (5) does not coincide with Nash's (4), it is easy to show that there is a direct equivalence [see Binmore, Rubinstein, and Wolinsky 1986 and Binmore 1987a].

So, should I admit defeat and accept the premise that under perfect information rational bargaining eliminates conflict? There are two reasons why the argument has not been settled and both are due to the backward induction without which the solution in (5) is meaningless. First, I do not accept the implicit use of the so-called *Harsanyi doctrine* [see Aumann 1979] in constructing the foothold that backward induction must have before it unfolds. The doctrine alleges that rational agents with identical information sets must think the same thoughts. Rubinstein depends on this doctrine in that at  $t=2$  both players expect A to receive  $V$  from the remainder of the game. If this cannot be defended, then

the logical process yielding  $(V)$  crumbles. The fact that both are equally well informed does not necessarily imply that they entertain the same expectations as to what they will get. If there is more than one expectation that one can rationally have, why should A and B always converge to the same one? Rubinstein proves the uniqueness of  $V$ , by assuming that agents (a) think that  $V$  is unique and (b) converge to it. Relax either (a) or (b) and the expulsion of conflict, as well as the determinism of the distribution theory, lose their legitimacy.

The assumption that A and B will have common knowledge of  $V$  at  $t=2$  is as strong and unwarranted as an a priori faith that the solution exists independently of players's thoughts. Indeed it is analytically indistinguishable to Nash's assumption that agents instinctively accept conventions such as IIA and, moreover, that they assume that their opponents do likewise. This is yet another case of quasi-functionalist thinking where the cognitive processes in people's heads are chosen for the analyst's convenience with little regard for the canons of explanation.

Although I believe the above to be sufficiently condemnatory, there is a second criticism that inflicts even greater damage on equilibrium bargaining theory. It is the argument that, irrespective of whether backward induction has or has not a legitimate foothold, its use for the purposes of deducing future moves is imprudent.

#### 4. Beyond Equilibrium

Recent work by Sugden (1989a, 1989b) and Binmore (1987b) questions the plausibility of the backward induction used in the case of table 1 and by Rubinstein in the derivation of (5). The question I want to address here is whether we can accommodate these criticisms within the confines of equilibrium game theory. If we can, then a deterministic theory of conflict may be more complex but will still remain feasible. I contend that we cannot.

Consider a version of the table 1 War and Peace game in table 4 where there is a single Nash equilibrium outcome (war at stage 1) easily traceable by backward induction.

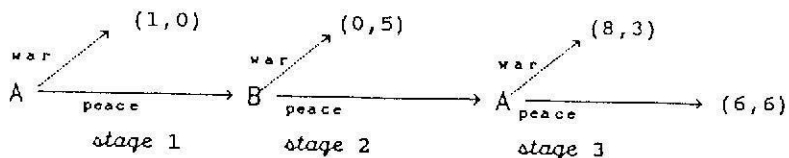


TABLE 4

Most game theorists would argue that, when a game has only one Nash equilibrium, it must represent the unique rationally defensible strategy. This is not always so.

Just before choosing war, A thinks to herself: "what if I choose peace? B believes that I am rational and will be puzzled by my failure to do the rational thing. There is a number of ways he can interpret my behaviour: [1] He may ignore it, convinced that it was a mistake unlikely to re-occur. If this is what he thinks, his faith in my rationality is touching. [2] Of course, I cannot count on this as he may doubt that I am fully rational. Should this matter to me? It should because if my 'irrational' choice at stage 1 makes him think that there is a probability  $p$  a shade greater than  $2/3$  that I will act irrationally again at stage 3, he will play peace at stage 2 in which case it may turn out that peace at stage 1 is perfectly rational for me! [3] However, there is always the possibility that B will replicate my thoughts and will anticipate that if I play 'irrationally' at stage 1 I am trying to induce him to play peace at stage 2."

Backward induction, and by extension game theory, assumes that, if A plays peace at stage 1, A will think either [1] or [3] but never [2]. It is another aspect of the assumption that replication of thoughts is automatic by equally rational agents. But as Pettit and Sugden (1989) argue, the only way [2] can be banished, is if we assume that a belief in your opponent's rationality is cast in stone rather than based on evidence. But why should agents be assumed to never question their opponent's rationality when the latter evidently act irrationally? In fact, all we need in order to breach backward induction is that A believes that B thinks that she is rational, but also thinks that there is a  $1/8$  probability that, upon observing an irrational choice at stage 1, B will think that there is a  $2/3$  probability

that his belief in A's rationality was unfounded. Is it irrational for A to have this expectation?

If advocates of equilibrium solutions insisted that players cannot consistently enjoy a strategic advantage over equally rational people who have the same information set, I would agree. However, they go much further when they protest, a la Harsanyi, that equally rational people starting with the same information cannot end up with different conjectures. Thus, they will argue in the spirit of Kreps and Wilson (1982) and Milgrom and Roberts (1982), that if the inviolability of the axiom of common rationality is to be relaxed, it must be done within the following equilibrium framework.

Suppose B observes peace at stage 1. What will he think once we relax the assumption that he is irreversibly convinced that A is rational? Letting  $p_0$  be his prior subjective probability assessment of B's irrationality, Bayes' rule can be used to incorporate the observation 'A chooses peace at stage 1'. If B expected A to invariably play peace at stage 1 when irrational, then B's assessment of A's irrationality rises from  $p_0$  to  $p$  according to

$$p = \frac{p_0}{1 - (1-p_0)r} \quad (6)$$

where  $1-r$  is the probability with which A will play peace at stage 1 in order to confuse B and thus make him play peace at stage 2 - ie. the probability that B conjectured as in [3].<sup>5</sup> Provided (6) yields a value just above  $2/3$ , backward induction is broken and peace is guaranteed at stage 1. Since B's expected returns from playing peace [ie.  $3(1-p)+6p$ ] must equal his expected returns from war [ie. 5], substitution of (6) in the appropriate equilibrium condition yields the optimal probability with which A will 'rationally' play war at stage 1:

$$r = \frac{\frac{2}{3} - p_0}{\frac{2}{3} (1-p_0)} \quad (7)$$

There are two reasons why the above offering from equilibrium theory should be rejected. The first stems from the simple point that (6) breaks down when  $p_0 = 0$ . When players start with an

absolute belief that their opponent is rational (ie. when  $p_0 = 0$ ), then violation of the optimal strategy prescription cannot be accommodated by Bayes' rule, a rule designed to deal with events which cannot occur when there is a zero probability attached to them. However, in strategic games the fact that B may have been certain that A will play war at stage 1 does not physically prevent A from choosing peace. It is A's ability to exploit counterfactual thinking (by acting in a manner to which B attaches a zero probability) that renders Bayesian logic inoperative.

The second reason for rejection returns us to the familiar question on belief coordination. In an equilibrium model, probability  $1-r$  must be computed optimally by A and replicated accurately by B [see (7)]. I have argued that this is not possible because of the multiplicity of rational beliefs on  $1-r$ . But let me for the sake of argument accept the equilibrium view that there exists a unique theory that produces the best value of  $1-r$  and, further, that equally rational individuals will latch on to it and reach the same expectation. Because A knows this value and also knows that B expects her to use it, she may still wish to upset his thought processes by choosing a value for  $1-r$  different from what the theory suggests. Will B take this to be a sign of irrationality? If the theory is unique, he should. But if he does, then he will abandon the conviction that A is rational and may play peace at stage 2. Thus, A may be justified to contravene the equilibrium theory in pursuit of an outcome preferable to a Nash equilibrium and, in so doing, she will have rationally falsified it<sup>6</sup>.

Two questions demand attention at this point. Are the beliefs that support the departure from equilibrium compatible with the assumption that agents are rational, and will the departure from equilibrium analysis mean that the Nash equilibrium never materialises? Looking at table 4 again, it seems strange, especially to those with game theoretic training, that the assumption of a common belief in rationality is compatible with A playing peace at stage 1. The crucial aspect of A's conjecture is that she expects B to question her rationality (after her choice at stage 1) because of his rationality, not in spite of it. Such anticipation does not compromise A's rationality who, if anything, proves quite shrewd in harbouring this hope. In the case of Nash

division games I argued that uncertainty is endemic due to the multiplicity of conventions. Just as uncertainty cannot be banished from division games by making objective functions transparent, irrationality cannot be rid of by convincing people that they are all rational. Thus, irrationality's reverberations do not disappear when we assume it away; instead, they are left behind as testament to the depth of its presence.

The above does not preclude the realisation of equilibrium outcomes. For if it did, we would have swapped one rigid orthodoxy for another. Suppose A always concludes that peace at stage 1 succeeds in confusing B and causing him to play peace at stage 2. If that were the case, rational players would always expect to get as far as stage 3, a conviction which is irrational as it contradicts the assumption of a common belief in rationality at the very beginning. It is the dialectical coexistence of reason and illogicality which rears pure strategic reason. The essence of the peace strategy at stage 1 is to foster such coexistence. In a Hegelian twist, A's rationality in playing peace at stage 1 is supported by the dialectical (dual) nature of 'peace at stage one' as (a) a rational strategy and (b) a strategy which undermines B's belief in A's rationality. Reason's cunning demonstrates itself in strange ways, but none stranger than in strategic exchanges where it reaches its pinnacle by ostensibly undermining itself.

##### 5. Rationalisable conflict

In a simultaneous move game, the Nash equilibrium is built on the assumption of rationality and mutual knowledge of rationality of the same order. Consider the game in table 5.

		B			
		1	2	3	4
A	1	5,4*	6,3	7,2	8,1
	2	4,5	3,6	8,1	6,3
	3	3,6	5,4	2,7	9,0
	4	2,7	4,5	5,4	1,8

where \* denotes a Nash equilibrium

TABLE 5

Let us define a first order belief in proposition P (ie. 'I believe P') as  $O_1$ . An n-order belief in P  $O_n$  is therefore interpreted by the statement 'I believe that you hold a n-1 order belief in P'. In the above game, the unique Nash equilibrium is supported by the assumption of an  $O_5$  belief in common rationality which must, by definition, subsume  $O_1$  to  $O_4$ . This is how it works on the principle of successive elimination.  $O_1$  eliminates alternatives that rational agents would never consider. For example, in the game of table 5 a rational A would under no circumstances choose peace at stage 3. Similarly, in the game above A would be silly to play her fourth strategy since she can do better whatever B chooses. The assumption of  $O_2$  allows us to say that A believes that B believes that A will never play her fourth strategy. Hence, as a direct consequence of  $O_2$ , we can be sure that B will discard the possibility of outcome (1,8) materialising and will never play his fourth strategy. The same logic eliminates A's third strategy by  $O_3$ , B's third strategy by  $O_4$  and, finally, A's second strategy by  $O_5$ . The Nash equilibrium is the only outcome compatible with a fifth order common belief in rationality.

The above sounds like a convincing defence of the equilibrium solution. The only problem is that it cannot be relied upon to lend support to equilibrium solutions in all cases. In the game of table 6, the requirement of a common n-order belief in rationality fails to underpin the Nash equilibrium. The reason is simple:  $O_1$  cannot eliminate any strategy for either player.

		B			
		<i>non-violence</i>	<i>medium intensity conflict</i>	<i>high intensity conflict</i>	<i>high intensity conflict</i>
A	<i>non-violence</i>	4,3	2,3		0,4
	<i>medium intensity conflict</i>	2,2	3,3*		1,2
	<i>high intensity conflict</i>				
	<i>high intensity conflict</i>	3,4	2,0		2,0

where \* denotes a Nash equilibrium

TABLE 6

In the absence of a justification for the Nash equilibrium

based on orders of commonly held beliefs, the acceptance of game theory's proposition that (5,6) is the only rational outcome necessitates a new, hidden, assumption: in addition to having an n-order belief in their common rationality, players' epistemic states about each other are mutual knowledge of some order n. This assumption yields the Nash equilibrium alright but begs one question too many: why should epistemic states be mutually known?

Bernheim (1984) offers an appealing alternative to equilibrium logic. Harnessing an idea which appeared earlier in Spohn (1982), he insists that if a player is rational, she should be able to justify her strategy's wisdom in terms of her conjectures concerning her opponent. The difference between 'rationalisable' and 'equilibrium' strategies lies in the latter's unreasonable requirement that conjectures are common knowledge.

As for a theory of conflict, peace is both thwarted and sustained by equilibrium reasoning depending on whether the game is cooperative or not. If the restrictions on agents' expectations administered by equilibrium theory are implausible, then different patterns of conflict may emerge from a move to rationalisability. This is, of course, not a call for anarchy in expectations but only a demand that the expectations we reject are so rejected because they are not compatible with rationality. The fact that they fail to comply with some hidden unsubstantiated equilibrating expectations' mechanism is immaterial.

Table 6 demonstrates the case in point. A and B have three options: (i) non-violence, (ii) medium intensity conflict, and (iii) high intensity conflict. Will players in this game, like automata converge to the Nash equilibrium, or is some other course of action rational? In particular, we would like to know if peace - ie. the (4,3) outcome - can ever be aspired to without wishful thinking. The traditional argument is that although peace would be nice (Pareto superior to the Nash equilibrium), the payoffs are such that it cannot be brought about. Just as in the prisoner's dilemma, the impetus for collective rationality is absent.

If A expects B to choose non-violence, she will also choose non-violence. However, equilibrium theorists would be quick to argue, A cannot possibly believe that B will choose non-violence. But why not? If A thought that B will expect her to

choose high intensity conflict, then A would expect B to choose non-violence. Would B ever expect A to unleash a high intensity attack? Well, A would indeed do so if she expected B to also go for high intensity conflict. In summary, if A, right at the beginning, was thought by B to expect high intensity conflict from him, then A would be expected to choose high intensity conflict and B would rationally opt for his non-violence strategy. But why should B expect A to play high intensity conflict? Perhaps she expects him to anticipate non-violence from her in which case his best response is high intensity conflict. We have come full circle. A and B can now play either non-violence or high intensity conflict and have a consistent set of perfectly rational beliefs with which to back up their choice. If they try to outsmart each other by attempting to be one step ahead of their opponent on the perimeter of the circle of conjectures, both peace and total destruction become rational outcomes. The problem with equilibrium game theory is that it stops players from trying to outsmart their opponents by implanting in their minds the conviction that they cannot do so.

#### 6. Conclusion

If the question that initiated this paper is to be answered, conflict must be traced back to at least one of two potential sources: antagonistic objectives and cognition. Since the former is not seen by economists as a reason for rational conflict, it is the latter that bears the burden of explanation. The equilibrium approach to non-cooperative games confirms that peace may be doomed if there is no equilibrium cognitive process to guide players toward a mutually advantageous outcome. Conflict is what happens when players have no way of setting their own agenda and exploring the possibility of prior agreement.

Granted that communication can enhance the prospect of harmony, will conflict vanish when bargains become possible and contracts binding? Equilibrium cooperative game theory claims that under perfect information on agents' means and ends, the answer is yes. At that point, imperfect information is ushered in for an explanation of conflict. However, this last step may be premature if the argument in this paper is correct. For, I have argued, that the occurrence of conflict has not been shown to be inconceivable

under the assumptions of rationality and perfect information. The expulsion of conflict from situations with perfect information is accomplished by a shrewd blending of the two paradoxes in the Introduction. Rubinstein (1982) uses the paradox of backward induction to advance a model of rational agents who project into the future the history of conflict, focus on the outcome, and finally return to the present with a unique vision of what will happen if they fight. Since both players share the same vision, they no longer need to fight.

Two reasons were given for rejecting this logic. First, backward induction, as applied to game theory, is often indefensible on the grounds of rationality and, second, the assumption that there is a common vision to be had is unfounded. A simpler variant of Rubinstein's model is the paradox of foreknowledge which too states that conflict is rendered irrational because agents have a unique theory/ vision of the future. The crux of the matter is that there exist several theories yielding the appropriate expectation for Rubinstein's model and the crucial set of conventions for Nash., none of which is superior to the rest. Perfect information is, thus, not tantamount to omniscience even when coupled with hyper-rationality.

The non-uniqueness of any theory, and consequently the inappropriateness of the equilibrium approach, is guaranteed by the impossibility of a self-confirming deterministic prediction. Suppose this is wrong and an ingenious equilibrium theory capable of precise prediction of strategies were feasible. Then, players would be free to subvert their opponents' conjectures (see sections 4 and 5) by falsifying the theory and, unintentionally, reinstating conflict. The insistence on the equilibrium view of cognitive processes belies a confusion of natural science, where atoms and amoebas have no choice but to follow the rules that govern their behaviour, with social science which deals with notoriously creative subjects. In an important sense, it denies individuals the essence of strategic reason.

Social scientists have turned to game theory for a rigorous investigation of socio-economic phenomena in an effort to avoid the perils of unsubstantiated generalisation and functionalist speculation. Sadly, they may have ended up with more of the same.

When conflict is assumed away by equilibrium theory, what is really happening is that agents are expected to act in a way justified only by its consequences on the analyst's postulates. The argument that players will converge to the same thoughts or conventions, when there is a myriad of either that can be rationally held, is as dubitable as any sociological theory which explains an event by pointing to its benefits for society but without explaining how it came about.

On an optimistic note, there is a lot to learn from game theory provided some time-honoured traditions are abandoned. For example, in perfect information Nash division games it is clear that expectations matter. But what kind of expectations are these when A knows the objective function of B, and vice versa, to its last parameter? They are more like predictions of what will happen than expectations concerning other agents. Take an instance of social conflict, for example a war or a mass strike. Can we describe either in terms of neoclassical (positive) expectations alone? Union leaders and army generals surely mean something different, in comparison to game theorists, when they say that they expect workers and soldiers to fight<sup>7</sup>. Of course, neoclassicists will argue that this incites an unwise mixing of positive with normative expectations that is not the subject of game theory but the domain of philosophy. However, what if it is normative expectations that determine which convention prevails in Nash's division game? What if it is normative expectations that regulate the divergence of optimal expected payoffs at t=2 in the Rubinstein game? Then normative expectations will decide not only the outcome but also the rationality of conflict.

NOTES:

<sup>1</sup>For an incisive survey see Binmore's relevant chapter in Binmore and Dasgupta (1987).

<sup>2</sup>See Lyons and Varoufakis (1989) and Varoufakis (1990).

<sup>3</sup>An interesting example is the ease with which conflict disappears in one-sided asymmetric information models once agents are set free to communicate at will. See Gul and Sonnenschein (1988).

<sup>4</sup>See Nash Bargaining I in Binmore and Dasgupta (1987a).

<sup>5</sup>According to Bayes' rule, if p is seen as the probability that A is irrational conditional on the observation that she chose irrationally at stage 1, then

$$p = \frac{\text{Pr ( A would have chosen irrationally had she been irrational )} \times \text{Prior probability of A being irrational}}{\text{Probability of irrational choice at stage 1}}$$

The updating mechanism in (6) follows. There is, of course, a difficulty with what to assume an irrational choice to be. In the above, an irrational A is expected to play peace (ie. the non-equilibrium strategy) consistently.

<sup>6</sup>A similar critique relates to mixed strategies in games with multiple pure strategy equilibria. See chapter 6 in Varoufakis (1990b)

<sup>7</sup>See Hollis (1987), especially the chapter on rational expectations, for a lucid discussion of the interdependence between positive and normative expectations.

- AUMANN, R. (1987)  
'Correlated Equilibrium as an extension of Bayesian rationality', Econometrica, 55, 1-18.
- BERNHEIM, D. (1984)  
'Rationalizable strategic behaviour', Econometrica, 52, 1007-28.
- BINMORE, K. (1987a)  
'Nash Bargaining Theory I and II', in K. Binmore and P. Dasgupta (eds.), The Economics of Bargaining, Cambridge: Cambridge University Press.
- BINMORE, K. (1987b)  
'Modeling Rational Players', Economics and Philosophy, 3, 179-214.
- BINMORE, K., A. RUBINSTEIN and A. WOLINSKY (1986)  
'The Nash bargaining solution in economic modeling', Rand Journal of Economics, 17, 176-88.
- ELSTER, J. (1982)  
'Marxism, Functionalism and Game Theory', Theory and Society, 11, 453-82.
- ELSTER, J. (1986)  
Making Sense of Marx, Cambridge: Cambridge University Press.
- GUL, F. and H. SONNENSCHNEIDER (1988)  
'On Delay in Bargaining with One-sided Uncertainty', Econometrica, 60, 1-11.
- HOLLIS, M. (1987)  
The Cunning of Reason, Cambridge: Cambridge University Press.
- KALAI, E. and M. SMORODINSKY (1975)  
'Other solutions to Nash's bargaining problem', Econometrica, 43, 413-18.
- KREPS, D. and R. WILSON (1982)  
'Reputation and Entry Deterrence', Journal of Economic Theory, 27, 253-79.
- LUCE, R. and H. RAIFFA (1957)  
Games and Decisions, New York: Wiley.
- LYONS, B. and Y. VAROUFAKIS (1989)  
'Game theory, bargaining and conflict', in J. Hey (ed.), Current Issues in Microeconomics, Macmillan.
- MILGROM, P. and J. ROBERTS (1982)  
'Informational asymmetries, strategic behaviour and industrial organization', Journal of Economic Theory, 27, 280-312.
- NASH, J. (1950)  
'The Bargaining problem', Econometrica, 18, 155-62.
- NASH, J. (1953)  
'Two-person cooperative games', Econometrica, 21, 128-40.
- PETTIT, P. and R. SUGDEN (1989)  
'The Paradox of Backward Induction', Journal of Philosophy
- ROTH, A. (1979)  
Axiomatic Models of Bargaining, New York: Springer-Verlag.
- RUBINSTEIN, A. (1982)  
'Perfect Equilibrium in a Bargaining Model', Econometrica, 50, 207-11.
- SELTEN, R. (1975)  
'Re-examination of the perfectness concept for equilibrium in extensive games', International Journal of Game Theory, 4, 25-55.
- SPOHN, W. (1982)  
'How to make use of game theory', in W. Stegmüller, W. Balzer and W. Spohn (eds.), Philosophy of Economics, Berlin: Springer-Verlag.
- SUGDEN, R. (1989b)  
'Conventions, creativity and conflict', in Y. Varoufakis and D. Young (eds.), Conflict in Economics, Harvester Wheatsheaf.
- SUGDEN, R. (1989a)  
'Game Theory without backward induction', mimeo.
- VAROUFAKIS, Y. (1990a)  
'Conflict in Equilibrium', in Y. Varoufakis and D. Young (eds.) Conflict in Economics, Harvester Wheatsheaf.
- VAROUFAKIS, Y. (1990b)  
Rational Conflict, Oxford Blackwell (forthcoming)

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- 29 W.P.Hogan, I.G.Sharpe & P.A.Volker Economic Letters, 6 (1980), 7 (1981)
- 30 W.P.Hogan Australian Economic Papers, 18(33), December 1979
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- 39 G.Mills Journal of the Operational Research Society, (33), 1982
- 41 U.R.Kohli Canadian Journal of Economics, 15(2), May 1982
- 42 W.J.Merrilees Applied Economics, 15, February 1983
- 43 P.Saunders Australian Economic Papers, 20(37), December 1981
- 45 W.J.Merrilees Canadian Journal of Economics, 15(3), August 1982
- 46 W.J.Merrilees Journal of Industrial Economics, 31, March 1983
- 49 U.R.Kohli Review of Economic Studies, 50(160), January 1983
- 50 P.Saunders Economic Record, 57(159), December 1981
- 53 J.Yates AFSI, Commissions Studies and Selected Papers, AGPS, IV 1982
- 54 J.Yates Economic Record, 58(161), June 1982
- 55 G.Mills Seventh Australian Transport Research Forum-Papers, Hobart, 1982
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- 57 P.Saunders Economic Record, 59(166), September 1983
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- 70 F.Gill Australian Quarterly, 59(2), Winter 1987
- 71 W.J.Merrilees Australian Economic Papers, 23(43), December 1984
- 74 J.Yates Australian Quarterly, 56(2), Winter 1984
- 77 V.B.Hall Economics Letters, 20, 1986
- 78 S.S.Joson Journal of Policy Modeling, 8(2), Summer 1986
- 79 R.T.Ross Economic Record, 62(178), September 1986
- 81 R.T.Ross Australian Bulletin of Labour, 11(4), Sept. 1985
- 82 P.D.Groenewegen History of Political Economy, 20(4), Winter 1988
- 84 E.M.A.Gross, W.P.Hogan & I.G.Sharpe Australian Economic Papers, 27(50), June 1988
- 94 W.P.Hogan Company and Securities Law Journal, 6(1), February 1988
- 96 B.W.Ross The Economic and Social Review, 20(3), April 1989
- 97 F.Gill Australia's Greatest Asset: Human Resources in the Nineteenth and Twentieth Centuries, D.Pope(ed.), Federation Press, 1988
- 99 R.T.Ross Australian Bulletin of Labour, 15(1), December 1988
- 100 L.Haddad Hetsa Bulletin, (11), Winter 1989
- 101 J.Piggott Public Sector Economics - A Reader, P.Hare(ed.), Basil Blackwell, 1988
- 107 B.W.Ross Prometheus, 6(2), December 1988
- 108 S.S.Joson Rivista Di Diritto Valutarario e Di Economia Internazionale, 35(2), June 1988
- 114 V.B.Hall, T.P.Truong & V.A.Nguyen Australian Economic Review, (3) 1989
- 116 G.Kingston Economics Letters, 15 (1989)
- 117 V.B.Hall & D.R.Mills Pacific and Asian Journal of Energy, 2(2), December 1988
- 122 W.P.Hogan & I.G.Sharpe Economic Analysis and Policy, 19(1), March 1989
- 123 G.Mills Journal of Transport Economics and Policy, 23, May 1989