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READING COURNOT, READING NASH: THE CREATION AND STABILISATION OF THE NASH EQUILIBRIUM*

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The concept of the Cournot–Nash equilibrium is central to noncooperative game theory and the latter's use in microeconomic theory. This paper considers the creation of this theoretical construct, examining the separate contributions of both Cournot and Nash, and showing how the two were ultimately joined together in the eyes of contemporary economic theorists. Rather than simply show how Cournot served as a precursor to Nash, we challenge the traditional approach, and emphasise the shifts in interpretation of their respective contributions. In so doing, we address some pertinent questions about the manner in which economic theorists view the evolution of their own discipline.

Any academic economist who today professes ignorance of game theory implicitly admits to having lost contact with the conceptual structure underpinning many recent developments in microeconomic analysis and with what is now being taught in microeconomics from intermediate undergraduate level upwards. Such is the transformation of the canon we have witnessed in recent years. Central to that change has been the development of non-cooperative game theory which, for many of its enthusiasts, now constitutes game theory (see Rasmusen, 1989, Kreps, 1990), and whose dominance even its critics are forced to recognise:

> 'That ascendancy appears fairly complete. Bright young theorists tend to think of every problem in game-theoretic terms, including problems that are easier to deal with in other forms. Every department feels it needs at least one game theorist or at least one theorist who thinks in game-theoretic terms. Oligopoly theory in particular is totally dominated by the game-theoretic approach' (Fisher 1989, p. 133).

Key to the game theory's hegemony is the concept of the Nash equilibrium:

> 'Nowadays one cannot find a field of economics (or of disciplines related to economics, such as finance, accounting, marketing, political science) in

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> which understanding the concept of a Nash equilibrium is not nearly essential to the consumption of the recent literature' (Kreps, 1990, p. 1).[1]

In what follows, we examine the early evolution of the central ideas of the non-cooperative game and the equilibrium point, both associated with John Nash. We look at his creative mathematical work, and at how it was received and interpreted by his contemporaries in mathematical economics. The main purpose of this paper is to offer an alternative perspective on the history of the Nash equilibrium, to give an interpretation, different from the standard one, of the development of game theory as it has influenced economic thinking.

The paper also has a second, more general, purpose, which is to pose some perhaps prickly questions about precursors, rational reconstructions and economists' understanding of the history of their own discipline. As in any other field of inquiry, in economics, practitioners cannot proceed without some perception of the historical development of their field. Whether this be formally published, as in survey articles and the introductory chapters of textbooks, or simply maintained as part of one's intellectual 'baggage', some conception of how one's area of interest has been shaped is inescapable. It is as inevitable an aspect of the general academic culture as knowing who are the current central figures in the discipline, or what are the competing schools of thought. This historical order, imposed retrospectively by theorists, very often takes a particular form, in which the past is viewed as a sequence of key contributions leading imperturbably to our imperfect, but nonetheless enlightened, present state of understanding. In this form, known as Whig history, precursors are ascribed a particularly central role: they pave the theoretical way for their successors to follow through, building on these foundations. A claim of this paper is that a more disinterested reconstruction of the practice of economists can help reveal the complexity of quotidian research in economic theory. Instead of looking at past theoretical advances as pointing inevitably towards the more recent developments with which we have since become familiar, we try to recover some of the uncertainty, the negotiation, and the debates, that characterise actual practice in mathematical economics. What we find is that new concepts and theories take time to be assimilated, to gain a role in the discussion, to have their meanings fixed: in short, to be stabilised. If one believes, as this author does, that the history of economics ought to pay greater attention to contingency and context, offering 'thicker description' of the practice of theorists, rather than tell a simple tale of how past economists heroically managed to anticipate the present, then the history of game theory constitutes a particularly rich, not to mention topical, area of research.[2]

[1] This view is widespread, from Tirole (1988) for whom it is 'the basic solution concept in game theory' (p. 206), to Rasmusen (1989) who writes: 'Nash equilibrium is so widely accepted that the reader can assume that if a model does not specify which equilibrium concept is being used it is Nash' (p. 33).

[2] In this regard, the paper is offered in the same spirit as recent innovative work in science studies, in which the traditional boundaries between history, philosophy and methodology and deliberately blurred e.g. Latour (1987). With few exceptions, eg. Mirowski (1989) and Weintraub (1991), the history of economic thought has remained largely aloof to such influences.

I. GAME THEORY IN THE LATE 1940S

Following its appearance in 1944, von Neumann and Morgenstern's *Theory of Games and Economic Behavior* elicited a rather mixed chorus of reaction, in both published form and in the departmental common rooms. Some of the early commentators, such as Hurwicz (1945), Marschak (1946), and Stone (1948), were enthusiastic, while others, such as Kaysen (1946), were more skeptical. That Cambridge caution evinced by the latter is echoed by Samuelson (1991), who felt that the book's cause was not helped by the antagonism of its authors on the seminar circuit: Morgenstern was, to put it bluntly, 'very Napoleonic' and although he made 'great claims', he himself lacked the mathematical wherewithal to substantiate them. Moreover, he had the irksome habit of 'always invoking the authority of some physical scientists or other'.[3] At the Princeton Economics department, caution had long since given way to overt hostility, and Jacob Viner led the ranks in heaping scorn on the never-too-popular Morgenstern.[4]

While the book was quickly and widely reviewed, active research building on the game theoretic content appeared less rapidly, and when such work was undertaken, it was done so largely under the influence of von Neumann. Ironically, in the process he found himself encouraging the development of ideas which were tangential, if not inimical, to the spirit of the original book. To a greater extent than Morgenstern, von Neumann's sphere of influence extended well beyond the borders of academia, and it was through his position as military mandarin that he won serious attention for game theory. At Princeton, he continued to hold court on military-related mathematical research, as he had done during World War II, and with his imprimatur, Office of Naval Research (O.N.R.) support for the development of linear programming and related ideas was forthcoming.[5]

[3] That Morgenstern and von Neumann were both vocal in their criticism of the use of the differential calculus in economics can hardly have helped endear them to the author of the *Foundations of Economic Analysis*. In their introductory chapter, they motivate their new approach by emphasising interdependence between economic actors and criticising 'the current overemphasis on the use of calculus, differential equations, etc., as the main tools of mathematical economics' (1947, p. 6). In his diary on October 8, 1947, Morgenstern wrote: '[Johnny] says [Samuelson] has murky ideas about stability. He is no mathematician and one should not credit him with analysis. And even in 30 years he won't absorb game theory'.

[4] Shubik (1991) recalls that 'the economics department just hated Oskar, not nearly just alone because they couldn't understand what was going on, but there was a certain aristocratic touch to Oskar, and as there was to a great extent a very Plebian economics department, that was an extra reason for the hate'. And on game theory: 'Not only were they not interested... no, they were interested negatively! With Viner being one of the most vituperative... Viner's attitude was: well, if they can't solve chess, then as economics is much more complicated than chess, what conceivable good can it be?'.

[5] Albert Tucker (1991) recalls that, in late 1947, George Dantzig, having developed the simplex method in linear programming, was sent from Washington D.C. by the Air Force to seek von Neumann's opinion on his work. With characteristic alacrity, von Neumann immediately observed the conceptual link between the minimax theorem and linear programming, and was enthusiastic about research possibilities. It was this that led to the O.N.R. trial project at Princeton, supporting research in linear programming and game theory. Led by Tucker, this project initially involved his students David Gale and Harold Kuhn, and ultimately became a 'gravy train' that ran for over twenty years between the O.N.R. and Princeton.

In the process, this support gave research in game theory a particular slant.[6] First, as von Neumann had pointed out above, linear programming had a game theoretic parallel in the *minimax* problem, the two-person, zero-sum game. Thus, subsequent research at Princeton would largely centre on exploring this parallel, arguably to the detriment of the development of n-person cooperative games, which had been given central focus in the book by von Neumann and Morgenstern. Second, this phase saw the 'economic behaviour' aspect of game theory slip into eclipse. Game theory was now in the hands of mathematicians who, knowing little about economics, did not share Morgenstern's desire to revolutionise economic theory. Their efforts were to be directed towards the development of 'tools' to aid economic decisions: routines the Navy could ultimately follow in deciding how to supply fuel to foreign bases, or how to tackle transportation tasks most efficiently. From this early collaboration,[7] centred on Tucker, there emerged a series of papers exploring linear programming and game theory, appearing in the first volume of the *Contributions to the Theory of Games* edited by Kuhn and Tucker (1950), and in *Activity Analysis* edited by Tjalling Koopmans (1951) and published by the Cowles Commission. (See Gale, Kuhn and Tucker 1950*a*, *b*, 1951.)

The two other centres of interest in early game theory were the University of Michigan and the RAND Corporation, initially called Project RAND, in Santa Monica. At the latter, with the emphasis distinctly on the 'big picture' and the longer view, the mathematics group run by John Williams was free to consider such new research areas as game theory and cybernetics.[8] There were direct links between there and Princeton, with Project RAND emerging directly from the wartime research milieu in which Princeton had been central.[9] And although by now von Neumann was in his computer phase and was interested in RAND primarily from that perspective, he exerted a general and direct influence on affairs in Santa Monica. Among the first

[6] Martin Shubik who, as a postgraduate student in economics, benefitted from O.N.R.'s funding Morgenstern's Logistics Research Project, recalls: 'I mean you did more or less what you wanted, but a great part of it was... linear programming'.

[7] At the outset, however, neither Tucker, Gale nor Kuhn were familiar with even the basics of game theory. As Kuhn (1991) recalls: 'We sat down and lectured to each other out of von Neumann and Morgenstern during that summer, that's what we did. We'd take a classroom, and someone would take a section, that had read it, and try to understand what the hell was going on, and lecture to each other about it'.

[8] Herbert Simon, who was later central to the development of artificial intelligence, as cybernetics came to be known, was also a RAND visitor in the early 50s. He suggests that the Zeitgeist of the period may be in part characterised by the use of symbolic logic: 'There seemed to be a formalism that was different from putting everything into real variables, and that really provided me with my central metaphor. And the cybernetic idea, the idea of feedback systems.' He emphasised the role of Lotka's *Elements of Physical Biology* in stimulating his interest in the latter: 'and if you walked up to somebody who had these kinds of interests, they had read Lotka's *Elements*' (Simon 1991). See Weintraub (1991, p. 41ff.) where he discusses Lotka's influence on Samuelson's *Foundations*.

[9] Williams had been a mathematics graduate student at Princeton, but forsook academics following a wartime stint of applied work with the Statistical Research Group attached to Princeton but located near Columbia University. He maintained close contact with such Princeton figures as von Neumann, statistician Samuel Wilks, and mathematician Henry Bohnenblust, who moved to Caltech shortly after World War II. The latter was a teacher of undergraduate Kuhn at Caltech and was also a key consultant to the Santa Monica group. See Leonard (1992).

to join Williams's group was Lloyd Shapley, who arrived at the beginning of 1948.[10]

For all its elaborate structure, the *Theory of Games*'s most significant proof still remained that by von Neumann (1928) showing the existence of the minimax solution for the two-person, zero-sum game. The stable set, developed as a solution to the general n-person game, still lacked a general existence proof.[11] Within a few months of arrival at RAND, mulling over the minimax theorem, Shapley, along with Roger Snow, came up with a proof that such solutions are always square: in equilibrium, the number of strategies employed by each player is the same. This was a key paper in orienting the RAND group towards game theory and for the next two decades it would remain a hallmark of their activity.[12] Such was the intellectual setting at both Princeton and RAND at the time that John Nash, still a teenager, was considering postgraduate study in mathematics.

II. MR NASH GOES TO PRINCETON

Nash arrived at Princeton as a comparatively young postgraduate student in 1948, where he quickly completed a Ph.D under the supervision of Albert Tucker.[13] The central part of his work during this period is his well-known proof of the existence of an equilibrium point for n-person, finite, non-cooperative games (Nash, 1950*b*). For all its brevity, his one page proof,

[10] After a wartime stint in China with the Army – where he showed some talent in cryptanalysis, decoding Japanese weather reports – Shapley finished his undergraduate degree in mathematics at Harvard. An older brother, who worked in the Budget Bureau administering the Air Force research budget, provided him with an entrée to the Williams group at RAND: 'This group – I guess McKinsey probably led us in the sense that anyone led us – used to have a seminar where we spoke about von Neumann and Morgenstern, where... we'd take turns doing the next chapter or section, elaborating on this or that point. That was my introduction to that book, and it was rather careful and detailed: we'd meet every couple of days for an hour or so, and just go through in some detail, turn around the proofs so that we'd know what's going on, and various early papers came out of that' (1992). Shapley spent two years there, before heading to Princeton for a Ph.D in mathematics, at the encouragement of Bohnenblust and Sam Karlin. While at Princeton, he spent his summers at RAND, and returned there after his degree. Ultimately, the free and easy academic atmosphere at RAND gave way to the more regimented environment of a paid consulting group, and so Shapley moved to the mathematics department at U.C.L.A. (Shapley, 1992).

[11] Not until Lucas (1969) was it made clear that it was quite possible to construct n-person games for which the stable set was empty.

[12] Shapley (1992) recalls von Neumann's influence here. The latter, on being shown this proof by Rand's Ed Paxson, became very excited and insisted that it be published. Von Neumann and Morgenstern's book had come out, says Shapley, full of mathematics, but, with two exceptions, Kaplansky (1945) and Loomis (1946), both new minimax proofs, 'the mathematicians hadn't taken the bait, and hadn't published a damn thing'. Thus, whenever there was a sign of something related being done by others, von Neumann showed enthusiasm. He response in this case was a boost for RAND morale and was key in turning Shapley towards games. 'The great man had spoken', he recalls: suddenly it was: 'just do game theory!' At von Neumann's intervention, the paper was accepted for publication in the *Annals of Mathematics*. A growing backlog of accepted papers for the *Annals*, however, meant that the delay before publication was getting longer and longer. Towards 1950, therefore, Tucker suggested that those related to game theory, including the Shapley–Snow paper, be taken to one side and published separately in a *Studies* volume, essentially a special issue. Hence *Contributions to the Theory of Games*, Vol. I. A further reason for the concentration of game theory in this particular journal is that, at the time, it was not considered by mathematicians to be sufficiently pure and respectable and thus had difficulty finding an outlet (Shapley 1992).

[13] Originally from Bluefield, West Virginia, Nash attended the Carnegie Institute of Technology as a scholarship undergraduate, from 1945 to 1948, beginning in chemical engineering but switching to mathematics after a year. Entering Princeton as a postgraduate student in 1948, he completed his Ph.D in 1950 and took a position at M.I.T. His academic career was interrupted by illness in the late 1950s (Nash (1991), Kuhn (1991, 1992) and Tucker (1991)).

presented to the Academy of Sciences by Solomon Lefschetz in November 1949, has had an influence on economic theory that, if anything, has grown stronger in recent years. Let us dwell a little on Nash's work.

Given that game theory was beginning to be studied at Princeton, and given that Tucker's project had the funds to support the best students, it was not unusual that Nash should have elected to work in the area. However, somewhat surprising and certainly less widely known is the fact that Nash had *already* undertaken a significant piece of work related to bargaining, or what he would subsequently label cooperative game theory, while still an undergraduate at Carnegie and in complete ignorance of the existence of von Neumann and Morgenstern (1944) (Nash 1991, Tucker 1991, Kuhn 1991). 'The Bargaining Problem' (1950*a*) was written for an elective class in international economics, which seems to have been Nash's only formal training in economic theory. It presents an axiomatic treatment of the two-person bargaining problem and shows how, given certain 'reasonable' requirements pertaining to invariance with respect to transformation of the utility functions; Pareto optimality; independence of irrelevant alternatives; and symmetry, the only feasible solution is that which maximises the product of the players' utilities.[14]

Nash's work on what he labelled the non-cooperative game, in which there are no opportunities for communication and hence coalitions, drew greater attention from his peers at Princeton. The associated equilibrium point, or Nash equilibrium, is accorded a certain history in the conventional wisdom depending on where one seeks the folklore. For general equilibrium theorists, the sequence begins with von Neumann's 1937 model of the expanding economy, then moves to the Kakutani (1941) fixed point theorem, and thence to Nash (1950*b*). Gérard Debreu points out that von Neumann's 1937 growth model, like his 1928 proof of the minimax theorem, 'remained very isolated for a number of years'. He continues:

> 'It was very interesting for many reasons, not only for the obvious reasons that he used linearity techniques for the first time, but there is that lemma, which is a precursor of the Kakutani Theorem, and which he did not need: such a powerful tool was not required to prove the theorem that he was after. The separation theorem for convex sets was quite sufficient. *That is the way in which I see his lemma (and Kakutani's work later, and Nash's work as a consequence of Kakutani's theorem) as an accident in von Neumann's writings: an accident within an accident*' (Debreu, 1992, italics added).

[14] By 'solution', Nash means the distribution to which rational players might be expected to agree, given their expectations about what is achievable. As Luce and Raiffa (1957) point out, this 'solution' is somewhat contrived in that it is the *only* one that satisfies the above requirements. The published version of the paper reflects the fact that Nash had met von Neumann and Morgenstern in the interim (p. 155, n. 1): their influence is evident in the presentation of the theory of linear cardinal utility, and Nash's allusions to the 'bilateral monopoly problem as treated by Cournot, Bowley, Tintner, Fellner, and others' (p. 155). The latter no doubt reflects the prodding of Morgenstern: Nash himself, as we shall see later, had certainly *not* read any of these authors. Incidentally, no amount of prodding could induce Nash to change the somewhat childish examples he had provided in the original undergraduate paper, and so the *Econometrica* version happily relies on Bill's and Jack's exchange possibilities for a book, ball, bat, toy, knife etc.! (Kuhn, 1991, Tucker, 1991).

Game theorists, focusing more on the game solution concept, often wax lyrical:

> '*Born more than a century ago in connection with Cournot's (1838) study of duopoly*, it is now extremely common in many different applications.... The Nash equilibrium is the embodiment of the idea that economic agents are rational; that they simultaneously act to maximize their utility. If there is any idea that can be considered the driving force of economic theory, that is it. Thus in a sense, Nash equilibrium embodies the most important and fundamental idea of economics' (Aumann 1985, pp. 43–4, italics added).[15]

In what follows, we shall try to reconcile the understandings represented by Debreu and Aumann with a closer look at Nash's creation of the noncooperative game, the machinations behind the published record. The accounts of the former, we suggest, constitute smooth, stylised interpretations in which the abstract theoretical *ideas* are given prominence, and the actual historical interactions between theorists – whether they were ignorant of, or read and interpreted, each other's work – are subordinated. Here, in contrast, we offer an alternative to such accounts. We attempt to recover some of the actual historical complexity, showing how and why economists seem to form stylised retrospective understandings of their own discipline.

The years 1950 and 1951 were fruitful for Nash, his work appearing in the following order:

1950*a* 'The Bargaining Problem', *Econometrica.*
1950*b* 'Equilibrium Points in *n*-person games', *Proceedings of National Academy of Sciences of the U.S.A.*
1950*c* *Non-cooperative Games*, Ph.D dissertation, Princeton.
1950*d* with Lloyd Shapley, 'A simple three-person poker game', in Kuhn and Tucker (eds.) *Contributions to the Theory of Games.*
1951*a* 'Non-cooperative games', *Annals of Mathematics.*
1951*b* 'N-Person Games, an Example and a Proof', *Project RAND RM-615*, 4 June.

These papers interlink and overlap in many ways and all are distilled from the effusion of discussion and ideas in the two years following Nash's 1948 arrival at Princeton. However, without entering into a tedious exegesis, several things should be made clear. First, while the distinction between cooperation and non-cooperation is not explicitly made until his dissertation, in 'The Bargaining Problem', Nash suggests that in a bargaining situation 'one anticipation is especially distinguished; this is the anticipation of no cooperation between the bargainers'. He then alludes to his contemporaneous, not yet published work on noncooperation when, in a reference to the 'stable set' of von Neumann and Morgenstern, he bemoans the fact that their theory 'makes no attempt to find a value for a given *n*-person game'. Such '*n*-person games should have values:...a set of numbers which depend continuously upon the set of

[15] This view is widely held. Debreu too, while he would be the last on earth to label himself a game theorist, also describes the Nash equilibrium as an idea which 'in a sense...goes back to Cournot' (Debreu, 1992). See also Friedman (1977), p. 8.

quantities comprising the mathematical description of the game and which express the utility to each player of the opportunity to engage in the game' (p. 157). Von Neumann and Morgenstern (1944) is the only reference used in this paper.

In the *Proceedings* announcement (1950*b*), again no mention is made of the distinction between the two types of game: Kakutani's Fixed Point Theorem is invoked to prove the existence of a fixed, or equilibrium, point for the correspondence which maps each element of the set of strategy *n*-tuples to its set of countering *n*-tuples, given continuity of the payoff function. The only two references are to Kakutani (1941) and again to von Neumann and Morgenstern (1947), the latter in the context of the equivalence of the minimax solution and the equilibrium point in the two-person zero-sum game. The paper, elegant in its brevity, caters to the tastes of the mathematician and is devoid of any economic discussion whatsoever.[16] Of particular significance for us, however, is the inserted credit thanking David Gale 'for suggesting the use of Kakutani's theorem to simplify the proof' (p. 49). What this of course means is that Nash did not draw on Kakutani for his 'original inspiration', and therefore that Kakutani cannot be viewed as a precursor of Nash. Debreu's latching Nash onto the von Neumann–Kakutani sequence is permissible only in a particular and strictly formal sense: a link, as identified by Gale, can be made between Kakutani and Nash's original proof. The reader of the paper, however, sees Nash using Kakutani, and thereafter views Nash in a particular light. That Nash's initial creative spark was apparently fired in ignorance of Kakutani is hardly relevant to the mathematical reader, and nor perhaps should it be. But the concentration on formal ideas can make for a somewhat stylised understanding of the evolution of theory.

What then was truly original in Nash's work? What was the proof that Gale helped simplify? We look to both Nash's doctoral thesis (1950*c*), perhaps the best indicator of his original thinking, and also to its published version, 'Non-cooperative games' (1951*a*), as there are differences between them that are worth noting.[17] Let us first consider what they share in common. Here, the

[16] Nash, largely verbally, presents the concepts of a finite *n*-person game, with its associated *n*-tuples of (possibly mixed) strategies, and payoff function. One *n*-tuple is said to *counter* another if the former yields to each player the highest obtainable payoff against the *n*-1 strategies of the other players in the counter *n*-tuple. The correspondence of each *n*-tuple with its set of countering *n*-tuples is a one-to-many mapping of the product space of *n*-tuples onto itself. Given the definition of countering, any point's set of countering points is convex, and the continuity of the payoff functions ensures that the graph of the mapping is closed. Given closure and convexity, by Kakutani's theorem, the mapping has a fixed point, i.e. there is an equilibrium point.

[17] It was at Tucker's advice that Nash used his equilibrium point proof as his Ph.D thesis. There is no exact date on the dissertation, but a note '5-26-50 doctoral dissertation' scrawled on the Abstract page suggests April 1950 as significant. The *Annals* version was received on October 11 of the same year. Nash spent the summer in the interim at RAND preparing the thesis for publication (Shapley, 1992). Tucker recalls: '[Nash's] Ph.D was done in 1949–50, the thesis was written. At that particular time, I was on leave of absence at Stanford, and so my contacts with Nash were by correspondence. But he didn't really need very much supervision. The only thing that I tried to get him to do – but he wouldn't – was to put in examples as he went along. His aim was to try to produce a complete theory of non-cooperative games in a short a space as possible. Any many years later he wrote me a letter saying that he finally understood what I was trying to get at. He realized now that in writing a paper, the important thing was to get the maximum amount of comprehension into the pages, per page!' (Tucker, 1991).

distinction between cooperation and non-cooperation is for the first time made clear: von Neumann and Morgenstern's book:

> 'contains a theory of n-person games of a type which we would call cooperative. This theory is based on an analysis of the interrelationships of the various coalitions which can be formed by the players of the game.
>
> Our theory, in contradistinction, is based on the *absence* of coalitions in that it is assumed that each participant acts independently, without collaboration or communication with any of the others.
>
> The notion of the *equilibrium* point is the basic ingredient in our theory. This notion yields a generalization of the concept of the solution of a two-person zero-sum game' (1950*c*, p. 1: 1951*a*, p. 286, emphasis in original).

Following a section on Formal Definitions and Terminology, a proof of the existence of the equilibrium point is given based on Brouwer's fixed point theorem.[18] This, it seems, is the original version of the proof used by Nash at the outset, and the one improved by Gale's suggested use of Kakutani. In the published *Annals* version, the notation has been cleaned up considerably. Apart from the fact that this was his own truly original contribution, Nash's decision to retain the proof based on Brouwer, rather than Kakutani, may, according to Luce and Raiffa (1957, p. 390) and Shapley (1992), well reflect the fact that the former is based on an intuitively more plausible dynamic story of adjustment towards equilibrium, whereas the latter simply states that such an equilibrium point exists. Following a section on symmetries of games, Nash offers definitions of various types of solution involving equilibrium points. In particular, a game is *solvable* if its set of equilibrium points are interchangeable, i.e. if any player can choose freely a strategy from *any* of her equilibrium strategies and the result remain an equilibrium point. *Strong solvability* implies that such interchangeability will furthermore have no effect on each player's equilibrium expected payoff. There follow several examples of two-person games illustrating these concepts. He then turns to a discussion of the geometrical form of solutions and of dominance and contradiction methods of finding equilibrium points. Here he shows, *interalia*, that no equilibrium point can involve a (strongly) dominated strategy.

What does Nash have to say of the economic relevance or broader application of this mathematical work? Following an analysis in which the

[18] This proof is *much* more involved. Letting $s = (S_1, S_2, \ldots, S_n)$ be an n-tuple of mixed strategies. Nash defines a sequence of continuous mappings, $s \to s'(s, \lambda)$, $\lambda = 1, 2 \ldots$, and shows that the fixed points of this sequence have a limit point which is an equilibrium point. In the mapping, $s'(s, \lambda) = (S'_1, S'_2, \ldots, S'_n)$, where $S'_i(s, \lambda) = \Sigma_\alpha \pi_{i\alpha} C'_{i\alpha}(s, \lambda)$. The construction of this mapping is based on three continuous function of s:

(1) $\rho_i(s) = \text{Max}_\alpha P_{i\alpha}(s)$, where $P_{i\alpha}(s)$ is the payoff to i of using pure strategy $\pi_{i\alpha}$, given the other n-1 players use the mixed strategies in s;

(2) $\phi_{i\alpha}(s, \lambda) = P_{i\alpha}(s) - \rho_i(s) + \frac{1}{\lambda}$; and (3) $\phi^+_{i\alpha}(s, \lambda) = \text{Max}\,[0, \phi_{i\alpha}(s, \lambda)]$.

Combining these,

$$C'_{i\alpha}(s, \lambda) = \frac{\phi^+_{i\alpha}(s, \lambda)}{\Sigma_\beta \phi^+_{i\beta}(s, \lambda)}.$$

Given continuity of $C'(.)$, the mappings $s \to s'(s, \lambda)$, $\lambda = 1, 2, \ldots$, are continuous. Since s is a cell, there exists, by Brouwer's theorem, a subsequence of fixed points, converging to s^*, which Nash finally proves, by contradiction, to be an equilibrium point.

equilibrium point for a simple three-man poker game is found, and where reference is made to a more elaborate examination of the same in Nash and Shapley (1950*d*), he considers applications, saying:

> 'The study of *n*-person games for which the accepted ethics of fair play imply non-cooperative playing is, of course, an obvious direction in which to apply this theory. And poker is the most obvious target' (1951*a*, pp. 294–5).

Poker, of course, had never been very far removed from game theory: von Neumann (1928) had intimated that his 'basic theorem' would help illuminate the practise of bluffing when playing the game, and subsequently gave the details in *The Theory of Games* (pp. 186–219), employing a simplified two-person poker. Thus when Nash was being prodded by Tucker to illustrate his theory with an example, poker, as Shapley (1992) puts it, was 'the order of the day'.[19]

Before leaving Nash's work on noncooperation, we should point out a significant difference between what appears in his original thesis and the 1951 published version. In his dissertation, Nash includes a penultimate section called Motivation and Interpretation. Here, he offers two motivations for the equilibrium point idea to show how it 'can be connected with observable phenomena'. In the first of these, the 'mass action notion', it is 'unnecessary to assume that the participants have full knowledge of the total structure of the game, or the ability and inclination to go through any complex reasoning processes'. Rather, it implicitly assumes repetition of a game which allows participants 'to accumulate empirical information on the relative advantages of the various pure strategies at their disposal'. Each player for each play is drawn at random from *n* populations, and it is assumed that 'there is a stable average frequency with which each pure strategy is employed by the "average member" of the appropriate population' (p. 21). Over time, players will learn the expected payoff associated with each pure strategy, and thus play optimal pure strategies. Thus, he claims, 'the mixed strategies representing the average behavior in each of the populations form an equilibrium point' (p. 22). And concluding, he says:

> 'The populations need not be large if the assumptions still hold. There are situations in economics or international politics in which, effectively, a group of interests are involved in a non-cooperative game without being aware of it: the non-awareness helping to make the situation truly non-cooperative.
>
> Actually, of course, we can only expect some sort of approximate equilibrium, since the information, its utilization, and the stability of the average frequencies will be imperfect.' (pp. 22–3).

The second interpretation is applicable to a game played only once, and he

[19] Game-playing was also part and parcel of life at the Princeton mathematics department. A visitor to the common room in Fine Hall might find himself interrupting anything from chess, Go, or poker, to a form of blind chess called Kriegspiel. And the same went on at RAND. Another game was called So Long Sucker! A form of attrition game, it involved the alternation of coalition formation and betrayal until one player remained. Herbert Simon who played it at RAND recalls: 'That was a vicious game! By the end of an evening you felt really mean about the whole human species' (1991).

asks: 'what would be a "rational" prediction of the behavior to be expected of rational [sic] playing the game in question?'. He replies:

> 'By using the principles that a rational prediction should be unique, that the players should be able to deduce and make use of it, and that such knowledge on the part of each player of what to expect the others to do should not lead him to act out of conformity with the prediction, one is led to the concept of a solution defined before.... In this interpretation we need to assume the players know the full structure of the game in order to be able to deduce the prediction for themselves. It is quite strongly a rationalistic and idealizing interpretation' (p. 23).

Between the dissertation and its published version, however, this economic discussion was simply dropped: the whole discussion by Nash of the economic intuition behind his mathematics is entirely omitted from the *Annals* paper! Regardless of who may have taken such editorial decisions, Nash or his editors, what we see here is the discourse being shaped by the community standards of those publishing mathematics at the time, and in the *Annals of Mathematics* in particular. Amongst mathematicians, who value theoretical elegance, ruminations about the economic intuition underlying the mathematics have little place, especially when they are likely to detract from the unity and positive tone of the paper. Formal requirements take precedence over debates about the behavioural 'reasonableness' of the underlying economic concepts. As Aumann (1991) notes:

> 'Mathematicians and economists look at their disciplines in different ways. As a rule, mathematicians don't consider a result worth publishing unless it is difficult to establish and, preferably, is of some generality; in brief, something that can be called a "theorem".'[20]

And so, just as the proof announced by Lefschetz in the *Proceedings* was pared to the absolute mathematical minimum, Nash's dissertation was whipped into shape to conform to the same minimalist aesthetic requirements, thus denying readers the benefit of his broader reflections.[21] From the moment he put pen to paper, Nash's work was the subject of interpretation, both by those deciding what in Nash should be published, what 'really matters', and by those subsequently reading the final article. Thus, his use of the Kakutani theorem allowed Debreu to link him to Kakutani and von Neumann, whereas in fact Nash's own work drew on the Brouwer proof which preceded them all. Nash's allusion to Cournot, Edgeworth and others in 'The Bargaining Problem' gave Herbert Simon (1991), and no doubt many others, the mistaken impression that he had read and was inspired by these early theorists. In fact, the direct

[20] Related to this tension in game theory between the 'mathematical' and the 'economic' is one between the normative and the descriptive, discussed by Luce and Raiffa (1957, pp. 62–3). They claim the theory to be purely normative, showing how agents should act if they wish to achieve certain aims, rather than descriptive, showing how they actually behave. This tension continues to pervade contemporary work on game theory.

[21] Tucker (1992) suspects that this was simply another case of Nash's youthful preference for an austere mathematical style. This is certainly borne out by Nash (1951 *b*), for example, which contains most of the numerical results for various treatments of duopoly later appearing in Mayberry *et al.* (1953). What the latter elaborate over the course of thirteen pages, however, Nash reduces to a cryptic single page.

influence of any such author on Nash was nonexistent. Nash had read none of these: such retrospective links were created later.[22] It is to this continued interpretation of Nash's work that we now turn. Given that Nash himself had not read Cournot, what subsequently allowed Aumann (1985) and others to endorse the equilibrium point by claiming that it was 'born more than a century ago' in connection with Cournot? How was this link made? Our claim is that ascribing to Cournot the effective role of Nash's precursor is a complicated process, a constructed reading of events that took time, reflected several different perspectives and motivations, and, above all, required that we learn to read Cournot in a new and very different way, that we learn to 'see' the Cournot solution differently.[23]

Let us recall how Cournot was viewed in 1950. His *Recherches sur les principes mathématiques de la théorie des richesses*, written in 1838, is a pathbreaking work in mathematical economics.[24] That part of the book of interest to us is, of course, Cournot's famous treatment of duopoly: in this case, two owners of water springs, who act independently.[25] Each chooses the quantity he will produce,

[22] Shubik, who roomed next to Shapley and Nash at Princeton, points out that 'certainly among all of the people in game theory, I was the only one who had read Cournot and Edgeworth' (1991). Gale (1993) writes: 'I'm sure... that Nash did not read Cournot and probably had never heard of him. I don't think any of us had until much later... [In] fact, Nash read hardly anything, – which was probably one reason for his success because some of the problems he solved were ones which the experts had given up on as hopeless'. As a related speculative aside, one may see a complementarity between the whole notion of noncooperation and Nash's own personality: from the beginning, he was personally disposed towards working independently, and resisted requests for collaboration by others (Kuhn (1991), Shubik (1991), Shapley (1992), Tucker (1991)).

[23] It is worth deviating to note how the Nash equilibrium was initially received as a game-theoretic idea. Above all, as Shubik (1991) reports, von Neumann 'hated it!', clearly finding it foreign to his whole conception of game theory. He was opposed to ruling out communication among the players and opposed to the search for a single-point solution. Von Neumann stood by the stable set which, while it sacrificed precision, implicitly allowed for the effect of extra-theoretical influences i.e., social norms (see von Neumann and Morgenstern (1947), pp. 31–45). Nash himself recalls that when he first presented his idea to von Neumann, the latter was not only dismissive but was able to pre-empt Nash's theory in the course of their conversation! In retrospect, Nash felt that, in contrast to von Neumann, his own thinking was more individualistic, 'more American' (1991). (Looking back now on the non-cooperative equilibrium, he also felt that it was more applicable to long- or infinitely-lived players such as corporations or states: such behaviour could hardly be expected of players with short horizons and limited calculating ability). It is likely too that von Neumann's own personal stubbornness mattered here, insofar as he was never receptive to ideas that seem to challenge his own: witness his stance on the Borel priority debate (Fréchet, 1953; Leonard, 1992). As Tucker (1991) puts it: 'That was one thing about von Neumann: that he couldn't stand criticism, or the slightest hint of a joke at his expense. It was the one, I felt, flaw in his character'.

McKinsey (1952) remarks that 'Nash's theory – though it represents a considerable advance – has some grave deficiencies and certainly cannot be regarded as a definitive solution of the conceptual problems of this domain' (p. 609). He then proceeds to illustrate the difficulty of the lack of interchangeability of equilibrium strategies, and the conceptual problems associated with Nash's project to reduce all cooperative games to a non-cooperative basic game. Luce and Raiffa (1957) are more approving. While there are such problems, 'Nash's definition has one extremely desirable property of an equilibrium notion: existence' (p. 171). Then, by way of justification, they suggest that: 'if our non-cooperative theory is to lead to an n-tuple of strategy choices and if it is to have the property that knowledge of the theory does not lead one to make a choice different from that dictated by the theory, then the strategies isolated by the theory must be equilibrium points' (p. 173). This, of course, is one of the interpretations offered by Nash himself in his dissertation. However, while regarded as strongly 'rationalistic and idealizing' by Nash, in the opinion of Luce and Raiffa it becomes a 'very strong argument for equilibrium points' (p. 173).

[24] All references here are to the translation by Bacon (1927).

[25] In focusing on the duopoly model only, we are clearly being extremely selective here in our treatment of Cournot. For subtle historical treatments portraying the complexity of Cournot's work, see Ménard (1978), and de Ville and Ménard (1989).

taking as given the quantity produced by the other, his optimal choice indicated by a reaction curve. Where the two reaction curves intersect, we have a 'stable equilibrium' in the sense that

> 'if either of the producers, misled as to his true interest, leaves it temporarily, he will be brought back to it by a series of reactions, constantly declining in amplitude, and of which the dotted lines of the figure give a representation by their arrangement in steps' (p. 67).

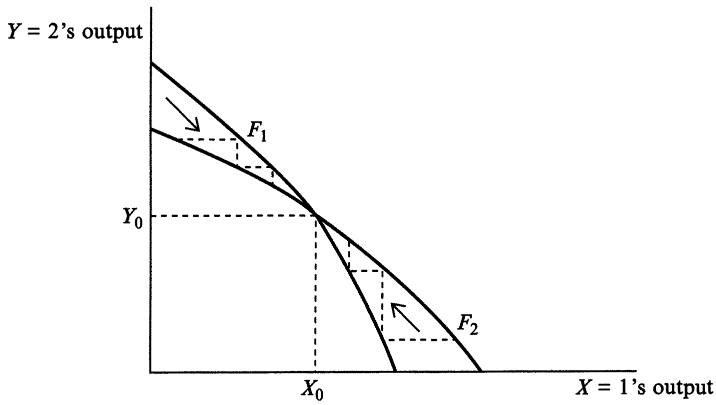

Fig. 1.

After many years of oblivion, Cournot's work received the attention of Walras, and then Bertrand.[26] The latter, in (1883), heavily criticised Cournot's treatment of duopoly largely on the grounds that price, rather than quantity, was the relevant choice variable. Further related papers were concerned to give more elaborate treatment of duopoly: Edgeworth (1925) introduced rising costs, Chamberlin (1934) introduced product differentiation, and Stackelberg (1934) allowed one firm to act as leader. However, the closest and most critical discussion of Cournot's actual model did not appear until Fellner (1949), whose treatment is quite simply a damning indictment of the inconsistency of Cournot's reaction curve story. Put briefly, it is contradictory to expect each producer to act as if the other's output is given. Such an assumption makes sense only at the equilibrium: once away from it, it would pay neither producer to stay on his own reaction curve. If the first knows the second is operating along reaction curve $F2$, then the first will abandon $F1$ and choose that point on $F2$ which maximises his own profit. And the same can be expected of the second producer, so the whole notion of moving along the reaction curves to equilibrium breaks down. As Fellner writes:

> 'The intersection point...obviously does not express equilibrium unless the firms in question react in the manner reflected by $F1$ and $F2$, and it is quite unreasonable to assume that they would react in this manner. To

[26] Walras actually reviewed Cournot's (1863) *Principes de la théorie des richesses*, the 'nonmathematical' version of the *Recherches*. However, this drew both his and Bertrand's attention to the original. Walras thereafter habitually paid due credit to the former for having influenced his use of the calculus of functions in economic theory.

be sure, that firms should assume of one another that the other follows a policy of fixed output is conceivable, but on the way to the Cournot solution they would necessarily realize that their assumptions were incorrect and they would change their assumptions. This would, of course, destroy the validity of the Cournot reaction functions and of any analysis based on them' (p. 65).

Such was the standing of Cournot's analysis on the eve of the appearance of the Nash equilibrium. What is important to note is that all understandings of the Cournot duopoly up to this assume that the analysis involves the passage of real time, each firm simultaneously reacting to the other until equilibrium is achieved. There is no suggestion that producers make alternate production decisions, with only one entrepreneur making a choice in any particular period: that would come later. All, including Fellner the ultimate critic, read Cournot as telling a story about how the two producers make simultaneous decisions, both moving in each period, and achieve equilibrium through time, to which they are compelled perpetually to return. And as Fellner points out, given this reading, the Cournot equilibrium makes no sense.

After the appearance of the Nash equilibrium, what we witness is the gradual injection of a certain ambiguity into Cournot's account in order to make it interpretable in terms of Nash. Following Nash, Cournot is reread and reinterpreted. This may have several different motivations, of which we here present concrete evidence of two. In one case, it is a way of anchoring, or stabilising, the new and still freefloating idea of the Nash equilibrium. By showing that somebody in the past – and all the better if it's an eminent figure – seems to have had 'the same idea' in mind, the Nash equilibrium is given a history, it is legitimised, and the case for game theory is strengthened. In the other case, the motivation is to detract from the originality of Nash's idea, maintaining that 'it was always there', i.e., Nash has said nothing new, he cannot claim priority. In both cases, while the motivations are different, the effect is the same: Nash's work is linked with earlier contributions. Considering the latter case first, we look, perhaps naturally, to France.

III. COURNOT AS 'INITIATOR'

The work of Georges Guilbaud of Paris's Institut Scientifique d'Economie Appliquée, in the decade following Von Neumann and Morgenstern (1944), is remarkable, in that he shows a detailed knowledge of the literature at a time when it remained the purview of a select number of largely American cognoscenti. While his papers (1949, 1952, 1954, 1955) are best understood as accounts of the theory of games as constructed by others, rather than independent contributions to the literature, they take a broad historical sweep, with long preambles taking us back to the related work in probability of Pascal, Bernouilli and others. The particular paper of concern to us is his 1954 course in the 'Principal Elements in the Mathematical Theory of Games', given at the Centre National de la Recherche Scientifique over the period 1951–53.[27] Here,

[27] All translation is by the author.

Guilbaud provides a reading of the history of game theory which might well be described as tendentious. For, in addition to presenting an outline of the theory, he is determined to show that much of it can ultimately be laid at the door of one French theorist or another. Many of the key ideas, he claims, can invariably be found in the work of Borel, if not Cournot. And to do this in the case of Cournot, he reinterprets him in a manner that a less charitable reader would regard as forced. In short, this is Whig history with a passion.

He begins by giving all bimatrix-, or so-called rectangular, games the label 'Cournot games', suggesting that this is implicit in Cournot's duopoly model. He then chooses to describe all minimax and Nash equilibria in pure strategies as 'Cournot equilibria' (p. III–5)[28] remarking that:

> 'It is very remarkable that recent work on the general theory of games have been led to rediscover the methods of Cournot: thus the "equilibrium points" introduced by J. F. Nash [reference to Nash 1950*b*] and immediately adopted by all the authors as a preferred tool of analysis, find themselves to be the equilibria defined by Cournot, not that, it seems, the American author had dreamt of it' (p. III–5).[29]

Neither, he claims, did the more recent text by Borel (1938) 'receive the attention it deserved' (p. IV–1) even though the mixed strategy solution he developed 'represented a decided improvement on the method of Cournot' (p. IV–2). He then discusses several games with solutions only in mixed strategies (all of them zero-sum incidentally so that he avoids addressing the Nash equilibrium in mixed strategies), and offers an interpretation of the Cournot duopoly model as an extensive game, in which producers make alternate production decisions, in each period taking the output of the other as given. That such an interpretation is highly contrived and assumes a certain myopia on the part of the duopolists is not lost on Guilbaud:

> 'In the sequence of plays each player acts as if he should be the last to play. The "reactions" of Cournot are only such thanks to an illusion of the players' (p. IV–8).

But so keen is Guilbaud to secure a place for his compatriot that he is willing to push matters as far as necessary, plucking Cournot entirely out of context so as to hammer home his 1838 anticipation of these ungrateful Americans. Guilbaud's classroom enthusiasm is tempered a little in (1955), a reworked version of parts of his course, but this gets him into further linguistic contortions. He now continues to talk of, on one hand, the 'Cournot' (read Nash) equilibrium associated with a game played 'all in one go' while maintaining that the actual Cournot model relies on a dynamic, multi-period tâtonnement-type procedure. In short, we must distinguish between what Cournot actually said and what he really intended!

To the reader familiar with the Borel–von Neumann priority debate of this time, raised by Fréchet (1953), the whole spirit of Guilbaud's message must

[28] Each chapter is numbered independently. Thus III–5 refers to chapter III, page 5.

[29] Guilbaud (1955) admonishes the 'American theoretician... [who]... did not believe it necessary to cite the name of Cournot!' and 'the theoreticians who wish to ignore the pioneer' (p. 168).

seem familiar. And, in fact, Guilbaud was in direct communication with Fréchet: he acknowledges as much and takes up the cudgel on Borel's behalf.[30] In short, what we see in Guilbaud's work is a particularly strained rewriting of the history of game theory, given particular energy by his nationalistic concern to 'set the record straight'. Guilbaud's account of the history of these ideas cannot be separated from his nationalistic fervour, and this accords with the prevailing attitude towards the United States in postwar France. The experience of the war left French dignity bruised, and the role played by the United States, if ultimately welcome, was nonetheless humbling. That mixture of fascination and repulsion with which America is held by French intellectuals was then at its peak, and such an attitude pervades the work of Guilbaud. While his direct impact on the world of English-speaking economists may not have been important, we present the case of Guilbaud here to illustrate how historical understanding may be shaped by a variety of forces. At the same time, however, back at Princeton, Nash's work was receiving a closer, and more influential, reading.

IV. GIVING NASH A HISTORY

In a recent interview, Shubik recalls that:

> 'as soon as Nash produced the non-cooperative equilibrium, I took one look at it and just said to John: this is Cournot. I don't want to knock John's contribution: it isn't Cournot, Cournot is a proper subset of Nash, *but it was there*! And then for the one and only time Nash and I decided to collaborate (1991, italics added).[31]

The collaboration in question is Mayberry *et al.* (1953) 'A Comparison of Treatments of the Duopoly Situation', which discusses various duopoly analyses of Cournot, Edgeworth and others from the perspective of game theory. Curiously, of one looks at the paper, there is no mention of the Nash equilibrium: Nash's work on non-cooperation does not even appear in the bibliography. Not only that but the Cournot solution is briefly described in four lines and the reader is referred to Fellner's *Competition Among the Few* for the details, where, as we have already seen, the whole concept of equilibrium in Cournot is effectively dismantled! Now admittedly, earlier in 1952, Shubik had made the link between Cournot and Nash, but even there does so in a peremptory and ultimately dismissive manner. Referring to duopoly as a two-person, non-cooperative game, with a Nash equilibrium as the solution, he says:

> 'This would yield a situation *somewhat like the Cournot solution*, depending upon what the competitor's variables were regarded to be. *This solution*

[30] See 1952 (p. 115, n. 1), 1954 (p. IV–19) and 1955 (p. 177).

[31] This is reaffirmed in Shubik (1989) where he writes that, following Nash (1951), it was: 'immediately observable that although Cournot's work with equilibria of games with a continuum of strategies was not strictly covered by Nash's work, conceptually Cournot's solution could be viewed as an application of non-cooperative equilibrium theory to oligopoly (see Mayberry *et al.* 1953)' (p. 123).

implies one possible state of society which is logically possible but does not seem to be very relevant to societies as we know them. We can consider the possibility of co-operation' (p. 146, italics added).

In these two early papers, therefore, Shubik displays a certain scepticism towards the Nash equilibrium, on the grounds that the hypothesis of non-cooperation is of questionable relevance, and towards the Cournot solution, in line with Fellner's criticism. A shift in perception surfaces in his 1953 dissertation, which succeeds both papers discussed above, and was later revised and published as *Strategy and Market Structure* (1959). In the former, Shubik draws attention to the resemblance between the ideas of Cournot and Nash and, in particular, sets about the business of reconciling the two conceptually. While the Cournot solution is 'very well known and has been discussed almost ad nauseam in the literature, *there still remain several interpretations of the work which can be made*' (italics added). In particular,

> 'Different interpretations of his work can be made concerning its dynamic or static aspects. Cournot implied that his pricing (sic) process was dynamic. Each producer adjusted to the other over a great period of time. Possibly it might take five or ten productions periods before the neighborhood of an equilibrium point was reached. Every iteration then would represent a production period. *It is also possible to regard his problem as static. All the computations are done by both sides who then commence with equilibrium point production rates*' (p. 81, italics added).

Six years later, in *Strategy and Market Structure*, Shubik puts it even more clearly. First, he cautions us that Fellner is not to be taken too literally:

> 'The reason for the rejection of Cournot's [solution] is that the hypotheses regarding the actions of the competing firms seem to be unreasonable. The original mathematical models have been recognized as implausible if they are meant to mirror reality. The problems involved may be too complex to be capable of simple symbolic formulation. Yet certain basic aspects of oligopoly may be explored by the use of simplified abstract models' (p. 2).

He later adds that the major contribution of Cournot's analysis may well be 'that it explicitly recognizes the nature of strategic interdependence and attempts to provide a mechanism to deal with such a problem' (p. 64).

The significance of the above account is as follows. Looking back today, when the link between Cournot and Nash has been stabilised and made acceptable, Shubik, quite understandably, offers a stylised reading of the history of these ideas: one in which Cournot's 'anticipation' of Nash became clear 'as soon as Nash produced the non-cooperative equilibrium'. In fact, the record shows that this connection was not perceived instantly, but involved a reinterpretation of the work of Cournot. In particular, Shubik's writings capture perfectly the gradual shift away from the implicit acceptance of Fellner's critique, in which the implausibility of the reaction curve dynamics is emphasised at the expense of the equilibrium, towards a static interpretation in which the dynamics are suppressed, and the equilibrium point preserved. To

Fellner, who preceded Nash, Cournot's analysis was only as good as the story underlying it: if the assumed behaviour was implausibly myopic, then the equilibrium was nonsensical. To Shubik and his successors, the duopoly model takes on a new perspective because 'what really matters', as Nash has unwittingly shown, is the point of intersection of the reaction functions, implausible as they may be. This is a move away from viewing Cournot's analysis as dynamic, contradictory and unrealistic, to seeing it as static, coherent and useful, and thus providing a historical foundation for the Nash equilibrium.

V. CONCLUSION

Economists make their discourses coherent and reach common understandings by settling on the meanings to be given to theoretical concepts. Part of this process of stabilisation involves linking new ideas with older ones, giving new concepts a history. And this inevitably requires the reinterpretation of both. Seeing Cournot as precursor, and Nash as successor, required that both be interpreted in a manner that rendered them consonant, so that, even though Nash had never read Cournot, we could still thereafter speak of the Cournot–Nash equilibrium. The resulting 'popular' understanding belies the considerable historical complexity that characterised the creative work of Nash and the manner in which it was received. Our account, by attempting to recover some of that complexity, can help remind economists of the richness and contingence of their own practice.

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18-2