The escape from conjectural variations: the consistency condition in duopoly theory from Bowley to Fellner

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The paper covers the 1924–1949 debate on the conjectural variations approach to duopoly theory and focuses on the evolution of economists’ views about the imposition of a consistency condition on the firms’ conjectures. The main point is that, although the consistency condition entailed a notion of interactive equilibrium that resembled the modern correct conjectures equilibrium, most neoclassical economists of the time refused to apply it because of the excessive requirements it imposed upon the firms’ forecasting abilities, and because of its failure to encompass an explanation of the equilibrating process.

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1. Introduction

This paper covers the debate on the conjectural variations approach to duopolistic competition in the period from 1924 to 1949. The focus is on the evolution of economists’ views about the imposition of a consistency condition upon firms’ conjectures in order to obtain a determinate solution to the duopoly model. The point that I wish to establish is that, although the consistency condition brought to duopoly theory a notion of interactive equilibrium that—looked at in retrospect—closely resembled the modern game-theoretic idea of a correct conjectures equilibrium, most neoclassical economists working in the period under scrutiny refused to apply such a notion because of the excessive requirements it imposed upon firms’ forecasting abilities, as well as its failure to encompass an explanation of the process leading to equilibrium itself. Quite the contrary, these economists preferred to avoid any kind of
equilibrium theorising about the firms’ conjectures and turned instead to a case-by-case, almost a-theoretical approach to oligopolistic competition.

Starting from Arthur Bowley’s 1924 *Mathematical Groundwork*, the idea began to spread that the duopolists’ Cournot-style reaction functions had to be given a conjectural interpretation. One of the issues was that of reconciling the reaction functions—an allegedly dynamic concept—with the static set-up of the standard Cournot model. The latter ambiguously mixed a static formalization of what today would be called a one-shot simultaneous game with a dynamic story in terms of firms’ actions, reactions and counter-reactions that would have better been modelled as a sequential game. In particular, each of Cournot’s reaction functions was formulated as a static equilibrium notion but featured as an argument the actual output produced by the other firm, a kind of information that each firm could obtain only in a sequential game. However, the feature of the Cournot model that raised most objections was yet another one, namely, the assumption that each firm behaved as if its rival would not react to its own move. This assumption was really puzzling, because in Cournot’s pseudo-dynamic story each firm would inevitably realise that its rival was not passive at all but did react to its moves.

Bowley offered a chance to overcome both problems with his new notion of conjectural variations which generalised the Cournot assumption and, at the same time, emphasised the *conjectural* nature of the reaction functions (see Section 2). Bowley’s approach gained further strength a few years later when another key model of oligopolistic competition—the Stackelberg model, originally formulated in terms of the tangible reactions to firms’ actual output choices—underwent a similar process of ‘conjecturisation’ (see Section 3).

Having dominated oligopoly theory for almost a decade, the conjectural approach came under attack in the mid-1930s when several objections were raised. To start with, where did Bowley’s conjectural variations come from? How did a firm form them? If a firm was able to perform a sort of instantaneous mental experiment in order to formulate a definite expectation as to its rival’s choice, why should it not be assumed that it was also able to anticipate the ultimate consequences of the experiment and thus to realise that the most profitable action for both duopolists was to produce the joint monopoly output? And, if this was the case, was it not reasonable for a firm to trust the reasoning ability of its rival and conclude that it too would reach such a conclusion? Finally, if, as in the Stackelberg model, we stuck to a dynamic set-up, how were firms supposed to revise their conjectures once they were falsified by their rival’s reaction? Did this learning process converge to the Cournot equilibrium or to the joint monopoly outcome?

It was in order to answer at least some of these questions that leading economists like Roy Harrod and Wassily Leontief suggested adding to Bowley’s and Stackelberg’s models the shortcut of imposing a *consistency condition* upon the firms’ conjectures, namely, that each conjectural variation had to coincide with the *actual* reaction of the rival (see Section 4). Yet the shortcut was far from conclusive. First, it called for very...
high demands upon the firms’ intellectual and forecasting ability, something that could not be easily conceded by most 1930s economists.\(^1\) Second, and more important, it extended to oligopoly theory a new image of economics as a discipline that looked only at the existence and static properties of equilibrium conditions, while neglecting the traditional issue of how and why the equilibrium was reached in the first place. In the specific case of duopolistic competition, this entailed that the imposition of the consistency condition begged the crucial question of how firms actually formed their correct conjectures.

It was on these grounds that other economists of diverse inclination, like Richard Kahn, George Stigler and William Fellner, contested the consistency condition and with it the whole conjectural variations approach (see Section 5). The critics focused on the fundamental uncertainty that affected the duopolists’ conjectures and argued that only the investigation of the actual functioning of oligopolistic markets could reveal how in practice firms circumvented such uncertainty. These criticisms helped to pave the way for the new approach, based upon a greater recourse to field work and the refusal to subordinate the empirical analysis to any universally valid theoretical scheme, that eventually came to dominate post-WWII industrial economics, especially in the US (see Section 6). A historically interesting implication of this story is that the research field that represented the most obvious outlet for the application of the newly formulated game-theoretic notions (above all, the 1950 Nash equilibrium), remained for a few more decades a rather hostile environment for the more mathematically-oriented kind of economics that emerged from the so-called formalist revolution.\(^2\) This, if anything, may provide a partial explanation of why non-cooperative game theory was largely neglected by US mainstream economists for the first 25 years of its life.

2. Bowley’s new idea

In his *Mathematical Groundwork*, Bowley argued that in order to solve the first-order conditions of a standard duopoly problem with quantity-competition

we should need to know \([q_2]\) as a function of \([q_1]\), and this depends on what each producer thinks the other is likely to do. There is then likely to be an oscillation in the neighborhood of the price given by the equation marginal price = selling price, unless they combine and arrange what each shall produce so as to maximise their combined profit. (Bowley, 1924, p. 38)

To grasp Bowley’s idea, consider two firms that produce a homogeneous product with output levels \(q_1\) and \(q_2\), and an aggregate output of \(Q = q_1 + q_2\).\(^3\) Provided the invertibility conditions are met, the market price associated with this output may be expressed in terms of the inverse demand function: \(p(Q) = p(q_1 + q_2)\). Each firm \(i\) is supposed to have a cost function \(c_i(q_i), i = 1, 2\). Assuming that the strategic variable for both firms is the output level, firm 1’s maximisation problem is:

\[
\max_{q_1} \pi_1(q_1, q_2) = p(q_1 + q_2)q_1 - c_1(q_1).
\]

\(^1\) See for example the exchange between Hayek (1939 [1935], 1937); Morgenstern (1976 [1935]) and Hutchison (1938, ch. 4), on the legitimacy of the perfect foresight assumption. On this debate, see Giocoli (2003A, ch. 3).

\(^2\) See Blaug (1999).

\(^3\) I have modified the notation in order to make it uniform throughout the paper. See however fn. 1, p. 610.
This shows that firm 1’s profit depends on the output choice of firm 2. In order to make an informed decision, firm 1 must therefore forecast firm 2’s choice. A similar problem can be formulated for firm 2.

According to the standard version of the Cournot assumption, each firm expects the other not to modify its behaviour as the market price changes. The first order conditions (FOCs) of the firms’ maximization problems are:

$$\frac{\partial \pi_i(q_1, q_2)}{\partial q_i} = p(Q) = p'(Q)q_i - c_i'(q_i) = 0, \; i = 1, 2.$$  \quad (2)

These two FOCs characterise what I shall call the basic Cournot duopoly model.

Generally speaking, modern microeconomics requires that in order for the firms’ choices to constitute an equilibrium, two conditions need be satisfied. The first condition is that no firm, on the basis of its own beliefs, must desire to modify its choice. The second condition is that the equilibrium actions of the firms are consistent with the beliefs upon which they act. Thus, in the duopoly model an equilibrium is given by every pair of output levels \((\hat{q}_1, \hat{q}_2)\) such that: (i) each firm is choosing its profit maximising output given the beliefs about the other firm’s choice; and (ii) each firm’s beliefs are correct at equilibrium. In our model such an equilibrium pair identifies the Cournot equilibrium.

Although it is now customary to characterise the duopolists’ reaction functions (RFs) in terms of each firm’s beliefs about its rival’s choice, before Bowley’s innovation the standard interpretation saw the interaction between the duopolists as taking place sequentially, so that each RF was a relation determining a firm’s action in a given period in terms of the other firm’s action during the preceding period. According to this view, the reaction function of firm 1 depicts how firm 1 will modify its output choice according to its rival’s output choice \(q_2\). Given that each FOC determines the optimal choice as a function of the rival’s output, firm 1’s RF \(f_1(q_2)\) is implicitly defined by the FOC \(\frac{\partial \pi_1(f_1(q_2), q_2)}{\partial q_1} = 0\). A similar equation holds for firm 2’s RF \(f_2(q_1)\). The RFs were thus static equilibrium notions applied to a sequential set-up. Bowley’s new idea was precisely to solve the tension between the statics and the dynamics of duopolistic competition by giving a conjectural interpretation of the reactions functions. This entailed the possibility (although not the necessity) of considering the duopolists’ interaction as a one-shot, simultaneous choice set-up.¹

The conjectural variation (CV) \(v_{ij} = \frac{\partial q_j}{\partial q_i}\) represents firm \(i\)’s conjecture about how \(j\) will respond to a small variation of \(i\)’s output. Call \(v_{12}\) the arbitrary conjecture that firm 1 formulates over firm 2’s conduct. Firm 1’s FOC becomes:

$$\frac{\partial \pi_1(q_1, q_2)}{\partial q_1} = p(Q) + p'(Q)(1 + v_{12})q_1 - c_1'(q_1) = 0.$$  \quad (3)

The same reasoning can be repeated for firm 2, whose conjectural parameter is \(v_{21}\).

Bowley’s introduction of the term \(\partial q_j/\partial q_i\) in the FOCs was a major novelty with respect to the usual representation of the duopoly problem. This term meant that the model’s solution depended on the exact value of each firm’s conjecture about its rival’s reaction. Hence, if \(v_{12} = v_{21} = 0\) we obtain the FOCs of the Cournot model, where each firm believes that its rival will not react to one’s own choice; if \(v_{12} = v_{21} = -1\) we

¹ See Friedman (1977, p. 149).
have the competitive model, where the FOC is nothing but the standard marginal cost-pricing rule; if \( v_{ij} = q_j / q_i \), \( i, j = 1, 2 \), we get the joint monopoly outcome (with \( v_i = 1 \) in case of identical firms).

That the conjectural variations approach could capture in a single parameter the intensity of firms’ rivalry and encompass as special cases the classic duopoly models entailed, on the one hand, that, in the absence of any a priori constraint upon the firms’ conjectures, the duopoly equilibrium was actually undetermined. On the other hand, this feature gave economists the freedom to devise values for the CV terms that warranted the desired solution. In short, Bowley’s proposal showed that the ghost of indeterminacy still hung over the duopoly model, but only up to a proper assignment of values to the CV terms.\(^1\) Thus, the CVs seemed to provide the Holy Grail of oligopoly theory, namely, that unitary approach to the topic that had been aspired to by more than one generation of economists. It is noteworthy that in such an approach, firms’ conjectures were explicitly under the spotlight.

Unfortunately, we now know that the CVs cannot constitute a satisfactory method for tackling firms’ behaviour. The reason is that they fall short of eliminating the confusion between statics and dynamics. Each CV term, in fact, indicates that one of the firms expects the other to react in some specific way to its own choice. But how can the rival react if the interaction takes place just once and simultaneously? Either we have an explicitly sequential set-up or we have to admit that the only reasonable CV is the Cournot one: the rival is expected not to react simply because the game ends with the simultaneous moves. Moreover, the CVs are quasi-dynamic concepts whose meaning and use in a dynamic setting together with static equilibrium notions like the RFs is highly questionable.\(^2\)

It is historically remarkable that these objections to the CV approach were raised quite soon, as early as the 1930s. Yet, as Cournot himself had anticipated (Cournot, 1971 [1838], p. 83), the most immediate effect of letting the mental variables in was that of enhancing the plausibility of the joint monopoly solution. In a review of Bowley’s Mathematical Groundwork, Allyn Young argued that if we allowed the conjectural element to enter the analysis, we also had to concede that each firm could anticipate the ultimate consequences of its rival’s chain of adjustments and thus discover that they were less profitable than the joint monopoly outcome. Collusion would then turn out to be the stable solution of the duopoly model because each perfectly rational duopolist would understand that deviating from it would cause losses to both firms (Young, 1925, p. 134). Similarly, in a 1928 paper Joseph Schumpeter argued that intelligent duopolists could not fail to realise all the implications of their situation, so that ‘they will hit upon, and adhere to, the price which maximises monopoly revenue for both taken together. [...] The case will not differ from the case of conscious combination—in principle—and be just as determinate’ (Schumpeter, 1928, p. 370). As I show in the next sections, the issue of joint monopoly behaviour was one of the two main reasons for the introduction of

\(^1\) As to Bowley himself, he was probably more inclined to highlight the indeterminacy of the result. This at least is what can be argued from the second part of the previous quotation in the text, where he seemed to claim that the dependence of the solution upon the conjectures entailed that the system would ‘oscillate’, unless an explicit collusive agreement was reached. Hence collusion represented for Bowley the way out from the indeterminacy caused by the conjectural term.

\(^2\) Cf. Tirole (1988, pp. 244-5); Varian (1992, p. 303). See, however, below, fn. 1, p. 615.
the consistency condition on the firms’ conjectures—the other being the conjectur-
isation of another duopoly model, namely, the Stackelberg one.

3. Stackelberg’s asymmetric duopolists

A new assumption in the realm of oligopoly models based upon Cournot-style RFs was introduced by Heinrich von Stackelberg, first in a 1933 paper published in Italian (von Stackelberg, 1933), and then in his 1934 classic Marktform und Gleichgewicht (von Stackelberg, 1934). The hypothesis was that duopolist 1 might know both the demand function and its rival’s RF, while firm 2 knew only the demand function. If this was the case, firm 1’s standard RF had to be discarded, since firm 2’s output was no longer given: firm 1 might in fact use its knowledge to set its own offer in order to affect its rival’s choice (von Stackelberg, 1933, p. 277). This turned the standard duopoly model into what became universally known as the Stackelberg leader–follower model. That a firm took into account its rival’s reaction was in fact the defining condition for it being a leader or, as Stackelberg called it in 1934, a firm in an independent position. A follower or, in Stackelberg’s terminology, a firm in a de-
pendent position, was by contrast a firm that took its rival’s offer as given.

According to Stackelberg, the new assumption ensured that a stable equilibrium obtained whenever firm 2 did not react and stuck to its own RF (ibid., p. 279). He also recognised that if the market were so transparent that firm 1 could have so much information about firm 2’s behaviour, the latter would also try to get the same information. Yet, if this was indeed the case, and if no firm gave up its efforts to obtain a better strategic position, no equilibrium could arise (ibid.). This kind of situation, that today is known as the double leader case, was termed by Stackelberg the Pareto duopoly case (ibid., p. 280).

The Pareto duopoly reveals that Stackelberg was explicitly reasoning in terms of an actual dynamics of offers and counter-offers—that is, in terms of a repeated strategic interaction—while making no mention at all of conjectural elements. He argued, in fact, that if each duopolist was determined to hold on, in order to convince its rival that the offer was invariable and so persuade it to adopt Cournot behaviour, it would always bring to the market the same quantity as if it were already the dominant firm (ibid., p. 281). In other words, in trying to achieve leadership each firm would act as if its rival followed the RF, although actually none did so, because the rival’s RF existed only in the mind of the would-be leader (ibid., p. 288). The eventual result of this double independent behaviour was that both firms would gain zero profits. Thus the outcome of the Pareto duopoly turned out much closer to the competitive rather than to the Cournot equilibrium, although it was actually not itself an equilibrium.

The 1934 book offered a more refined presentation of the previous analysis but no substantial improvement. The author’s view of the Cournot model was still that each

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1 Note that both Edgeworth (1968 [1897]) and Frisch (1951[1933]) had anticipated Stackelberg on this matter.
2 This on account of Pareto (1971 [1927], pp. 435–6). It became the Bowley duopoly case in the 1934 book.
3 The model was later reproduced by Stackelberg in a 1943 textbook in German, a later version of which, published posthumously in 1948, has been translated in English in 1952 by Alan T. Peacock (von Stackelberg, 1952 [1948]). The third chapter of part IV of the 1952 book is the standard reference in the Anglo-American literature for Stackelberg’s original model.
firm set its quantity by taking its rival’s offer as given. The rival reacted by choosing a different offer but taking the first firm’s output as a datum, and so on, until equilibrium was reached (Stackelberg 1952 [1948], p. 190). This was once more a sequential, not a simultaneous, kind of interaction. No mental variable was called forth in the form of, say, a stage-by-stage revision of each firm’s expectations about the rival’s outcome. Stackelberg continued to believe that, especially in the double leader case, the two firms would go on facing each other, each sticking to its wrong conjecture about the rival’s behaviour (namely, that the rival would behave as a follower), without any revision of its beliefs but just an endless repetition of interactions, none of which represented an equilibrium.

From a historical viewpoint it has, therefore, to be recognised that the original version of the leader–follower model was that of a game played by truly myopic agents who, contrary to what Cournot himself believed,\(^1\) did not simply abstain—on account of limited information—from formulating a conjecture about its rival’s behaviour, but formulated one such conjecture and stuck to it, despite its being repeatedly falsified by events. Thus, notwithstanding the brilliant initial intuition, Stackelberg’s own model was really an untenable one from a decision-theoretic viewpoint. The real merit of his construction was to stimulate the analytical wit of those economists who began to investigate the conditions for the compatibility of duopolists’ conjectures and the reasons underlying the instability of the double leader case.\(^2\)

In order to appreciate this theoretical evolution, let us transform the basic duopoly model of Section 2 into a Stackelberg leader–follower model. It is now customary to present the latter as a two-stage game where the firms move sequentially: one firm (the leader) moves first, while the other (the follower) observes the leader’s move and makes its optimal choice given this move. Let firm 1 be the leader and firm 2 the follower. As is standard in two-stage settings, to solve the game we start from firm 2’s problem in the second stage. Given the leader’s choice \(q_1\), the follower’s problem is:

\[
\max_{q_2} \pi_2 = p(q_1 + q_2)q_2 - c_2(q_2). \tag{4}
\]

The FOC is:

\[
p(q_1 + q_2) + p'(q_1 + q_2)q_2 - c'_2(q_2) = 0, \tag{5}
\]

which is identical to the FOC of the basic Cournot model, apart from the fact that this time \(q_1\) is known in advance and is not simply conjectured. We can, therefore, determine firm 2’s RF, namely, \(f_2(q_1)\), which gives the optimal level of the follower’s output for any possible choice of the leader. This RF is anticipated by firm 1, whose problem in the first stage of the game is therefore:

\[
\max_{q_1} \pi_1 = p(q_1 + f_2(q_1))q_1 - c_1(q_1). \tag{6}
\]

The FOC is:

\(^1\) See Giocoli (2003B, p. 184).
\(^2\) These efforts were further stimulated by Stackelberg’s dismal policy conclusions. Both in 1933 and 1934 he claimed that, since in the double leader case the firms would eventually realise that they had better reach an agreement and produce the joint monopoly output, the free market would escape from instability only by transforming itself into a system of powerful cartels. This justified State intervention in the market, thereby legitimizing, in his view, the economic policies of the Fascist and Nazi regimes.
The leader’s FOC therefore features a function, i.e., the follower’s RF, and not a simple output level. The leader knows that by moving first it will induce a specific reaction on the basis of the follower’s RF and so takes this into account to solve its own problem.

It is important to remark that under a decision-theoretic viewpoint, the follower in the Stackelberg model has much more information than a firm in the Cournot model, and thus also a richer strategy set. Indeed, it can set its own output level according to any possible function of $q_1$, including of course a function entailing a specific hypothesis about the leader’s behaviour, namely, the expectation that the leader will not counter-react: this function is the Cournot RF. Similarly, the leader may expect the follower to stick to any of the possible functions of $q_1$. It turns out that the dominant rule for the follower is precisely the RF and this is exactly what a rational leader should expect it to choose. As a consequence, the two conditions for an equilibrium are met: both firms maximise given their beliefs and the firms’ beliefs are validated by the equilibrium actions. A Stackelberg equilibrium obtains.

The model can also be given a static interpretation in terms of a simultaneous game. We can assume in fact that the two firms hold the following beliefs: firm 1 believes that its rival behaves following a Cournot RF; firm 2 has the same belief as in the basic Cournot model. If both firms stick to their beliefs, the conditions for a simultaneous equilibrium are met. While this is not the way the model was formulated by Stackelberg himself, the issue of finding a pair of simultaneous conjectures capable of supporting the Stackelberg equilibrium was actively investigated in the mid-1930s, as it was particularly important in the double leader case. Take the previous model to represent a simultaneous game and assume that both firms behave as leaders, i.e., formulate the conjecture that the rival reacts following a Cournot RF. Hence firm 1 believes that firm 2 follows $f_2(q_1)$, while firm 2 believes that firm 1 follows $f_1(q_2)$. Consequently, each firm produces the leader’s output, but, as Stackelberg had noticed (though in a different context), this outcome is not an equilibrium since the second condition for an equilibrium, that requiring the validation of the beliefs, is not satisfied: each firm believes the rival to behave as a follower, while instead the latter behaves as a leader. No equilibrium exists in the double leader case—unless of course we assume that the firms eventually collude and act as a joint monopolist. It is hardly surprising that this result raised a hot debate in the second half of the 1930s, as what was at stake was no less than the viability of the free market system.\(^1\)

4. Leontief’s call for correct conjectures

In his 1935 survey on monopoly theory, John Hicks noted that Bowley’s CV approach was becoming increasingly popular in the literature. After a brief presentation of the approach, he observed that there was no need to impose any particular consistency condition upon the firms’ conjectures in the short period (Hicks 1953 [1935], pp. 375–6). Yet, in a footnote added before the 1953 reprint of the paper in the AEA

\(^1\) See the previous footnote and, e.g., Nicholas Kaldor’s review (Kaldor, 1936, p. 229).
collection *Readings in Price Theory*, Hicks claimed that the most important work in duopoly theory since 1935 had dealt precisely with the imposition of a consistency postulate on the CV terms (*ibid.*, p. 376, fn. 26a). Hicks quoted as a reference, Richard Kahn’s 1937 paper on duopoly. Although, as I show in the next section, in that paper Kahn actually criticized the consistency postulate, Hicks’ addendum effectively captured what constituted the major theoretical development of Bowley’s approach.

The idea of tying down firms’ conjectures by imposing a consistency condition had been advanced slightly before Hicks’s survey in a short note published by Roy Harrod in the *Economic Journal*. Harrod argued that in order to obtain a determinate duopoly equilibrium all we had to do was to equalise, for both firms, the rival’s conjectured reaction to its actual reaction. This reduced the duopoly model to a system of two equations with two unknowns, so that the equilibrium was necessarily determined. He thus concluded that ‘there does not appear to be any reason of economic principle for supposing that the equilibrium of duopoly would on normal assumptions be any less determinate’ (Harrod, 1934, pp. 335–7). What was required by Harrod to achieve a determinate equilibrium was simply that the duopolists’ beliefs be mutually consistent. The equilibrium output and price could then be anything, provided the consistency condition was met.1

In modern terms, we say that the consistency restriction that needs to be imposed upon firms’ conjectures is that the CVs must be correct in equilibrium. A consistent conjectural equilibrium (CCE)2 is one in which firm $i$’s conjecture as to how firm $j$ will react to a small variation of $i$’s output is correct ($i, j = 1, 2$). The Cournot equilibrium of our basic model is not a CCE because both firms make inconsistent conjectures about the rival’s reactions, albeit their conjectures on the equilibrium level of the rival’s offer are correct.

Formally, a CCE requires a pair of output levels ($\hat{q}_1, \hat{q}_2$) and a pair of CVs ($\hat{v}_{12}, \hat{v}_{21}$) such that:

(i) $\hat{q}_1 = f_1(\hat{q}_2)$ and $\hat{q}_2 = f_2(\hat{q}_1)$;
(ii) $\hat{v}_{12}(\hat{q}_1) = \frac{\partial f_1(\hat{q}_1)}{\partial q_1}$ and $\hat{v}_{21}(\hat{q}_2) = \frac{\partial f_2(\hat{q}_2)}{\partial q_2}$.

The conditions in (i) mean that each firm is correct about the level of the rival’s reaction. The conditions in (ii) mean that each firm is correct also about the slope of the rival’s RF. The slope of the RF, say, $f_i(q_2) = \frac{\partial f_i(q_2)}{\partial q_2} = -\frac{\partial^2 \pi_i}{\partial q_1 \partial q_2}$, indicates how each firm optimally reacts to a change in its beliefs about the rival’s choice. It can easily be shown that the usual conditions for the Cournot equilibrium, namely, conditions (i), do not suffice to obtain a CCE, since the zero CVs of the Cournot case differ from the slopes of the RFs ($-1/2$, in the linear case).

The idea that the imposition of a consistency condition upon firms’ beliefs could tie down the equilibrium of duopoly was the cornerstone of Wassily Leontief’s 1936 review of Stackelberg’s *Marktform und Gleichgewicht*. This review is particularly

1 It is hardly surprising that Harrod imported in the field of oligopoly analysis the new equilibrium concept that had just emerged from the larger debate on the perfect foresight assumption in business cycle theory (see above, fn.1, p. 603).

important since it quickly became the major source of information on the Stackelberg model for English-speaking economists.

Leontief focused on the two behavioural assumptions formulated by Stackelberg for the case of duopolistic competition. In Leontief’s presentation, these assumptions were rendered as follows. A duopolist might base its own behaviour upon the belief that either its own actions did not influence that of the rival, so that the rival would not react, or that they would systematically cause the rival’s reaction. These two kinds of beliefs characterised what Leontief termed, respectively, the follower (F) and the leader (L). Thus, if a firm thought that the rival played L, it had better play F, and vice versa (Leontief, 1936, p. 555). It is crucial to observe that, contrary to Stackelberg’s original formulation, Leontief defined the two ideal types, the leader and the follower, in terms of the expectations or beliefs that each firm held about its rival’s behaviour. Thus, throughout the review, Leontief presented the Stackelberg model as a static simultaneous game based on firms’ mental states, and not as a sequential game played with actual output choices.

Leontief duly reported the three possible cases of Stackelberg’s set-up—namely, two followers, two leaders, one leader and one follower—and argued that no stable equilibrium could arise in the first two situations because the rival’s actual behaviour differed from that expected. Only in the third case were the firms’ expectations confirmed, so that an equilibrium could be obtained (ibid., pp. 555–6). Yet, he thought that one relevant case was missing in such a taxonomy. In the double leader case each firm believed that the rival would play F and so played L. This meant that, say, firm 1 believed that firm 2 believed that firm 1 would play L and so in its turn would play F. In other words, the decision to play L by firm 1 was based upon a (mistaken) belief about its rival’s beliefs. In Stackelberg’s presentation, experience from repeated play would reveal to both firms their mistakes, since the rival did not behave as expected (but both behaved as they thought their rival expected them to!). This, however, neglected the possibility that ‘each of the two sellers expects a definite adjustment policy on the part of the other, chooses the role of the leader, and finds that the behaviour of his competitor actually corresponds to his expectations’ (ibid., p. 556). That is, Leontief envisaged the possibility that a well-defined expectation about the rival’s reaction might be fulfilled by actual behaviour, so that a stable equilibrium might exist also in the double leader case. The key notion to give shape to such a well-defined expectation was of course Bowley’s CV.

Leontief offered a simple formalization of his new idea (ibid., p. 557). Take the inverse demand function and the FOCs to be as in the basic duopoly model of Section 2. Assume now, as in the Bowley model, that each firm acts according to its beliefs about its rival’s reaction. Thus, firm 1’s expectation of firm 2’s reaction is $\frac{\partial q_2}{\partial q_1} = v_{12}$. This means that firm 1 expects firm 2 to play F. Firm 2’s expectation of firm 1’s reaction is $\frac{\partial q_1}{\partial q_2} = v_{21}$. This means that firm 2 also expects firm 1 to play F.

The actual behaviour of the two firms, conditional on the expected rival’s reaction, is given by the two RFs obtained by solving the FOCs. Firm 1’s RF is $q_1 = f_1(q_2)$, while firm 2’s is $q_2 = f_2(q_1)$.¹ According to Stackelberg, $(v_{12}, v_{21}) \neq [f_2(q_1), f_1(q_2)]$, that is, for both firms the rival’s expected reaction is different from its actual reaction,

¹ I stick to Leontief’s notation, although the CV terms should also feature as arguments of the RFs, i.e., $q_1 = f_1(q_2, v_{12})$ and $q_2 = f_2(q_1, v_{21})$. 
captured by the slope of its own RF. This entails that no stable equilibrium may exist. Leontief instead envisaged the possibility of correct expectations. This required adding to the two FOCs, a pair of new conditions that he termed stability conditions: $f'_2(q_1) = v_{12}$ and $f'_1(q_2) = v_{21}$. The two stability conditions plus the two FOCs generated a system of four equations in four unknowns that allowed the determination of a stable equilibrium even in the double leader case. If all conditions were met, in fact, neither firm ‘will have any reason to alter his expectations and revise his calculations, and thus to disturb the existing equilibrium’ (ibid., p. 559).

Leontief’s decision to deal with the firms’ beliefs, rather than with their actual behaviour, was crucial in this respect. In Stackelberg’s set-up, to characterise a leader we look at the independent output choice. In Leontief’s set-up, however, a leader is simply someone who believes that the rival will play F. It was this ‘conjecturisation’ of the leader–follower model that enabled Leontief to define the conditions for a static equilibrium in terms of a pair of expectations that are not disconfirmed by actual events.

It is apparent that the two stability conditions proposed by Leontief anticipated the modern conditions for a CCE. What he failed to notice, however, was that even a pair of correct conjectures did not suffice for an equilibrium in the double leader case because they did not satisfy the first requirement for an equilibrium, namely, the absence of any unilateral incentive to deviate. Assume in fact that both firms believe their rival will play F, i.e., à la Cournot, and are confirmed in their beliefs, so that both produce the Cournot output. Each firm would then have an incentive to deviate. Assume instead that they hold the same beliefs, but do not produce the Cournot output. Then their beliefs are not confirmed, so that the second requirement for equilibrium is violated.\(^1\) The only way out of this logical puzzle is to derive endogenously how the firms form their correct CVs. As a modern critic put it, ‘conjectures, which are supposed to reflect one firm’s beliefs about other firms’ strategic responses to its actions, need a temporal story to be legitimate. (Responses require a temporal setting.)’ (Makowski, 1987, p. 46, emphasis in the original). Yet, even to conceive of such a way out presupposes that the economist be interested in understanding the ‘how and why’ of equilibrium, and not merely in deriving the equilibrium relations.\(^2\) The latter was indeed Leontief’s sole concern in the 1936 review.

5. The escape from correct conjectures

It did not take long for the consistency condition to be criticised. The common theme of the criticisms was that the new solution to duopolistic interaction envisaged economics as a mathematical discipline only dealing with equilibrium relations, rather than, as it had traditionally done, with the dynamics of market processes. In particular, the condition of correct conjectures seemed to place an excessive, unrealistic requirement upon the duopolists’ forecasting ability. This clashed with the trend in

\(^1\) See Daughety (1988, pp. 35–6) and Kreps (1990, pp. 329–30). Note that this difficulty had already been acknowledged by Kaldor (1936, pp. 229–30).

\(^2\) For example, Daughety (1985) still begs the issue as he employs an infinite-regress, rational expectations model to derive the firms’ correct CVs and demonstrate that the Cournot equilibrium also is a CCE. One of the referees addressed me to Dixon and Somma (2001), where it is shown how the correct CVs may emerge as the equilibrium outcome of an evolutionary process of a population of boundedly rational firms.
contemporary economic theory to delimit carefully, if not altogether abandon, the assumption of perfect foresight and showed the descriptive weakness of focusing too much on equilibrium conditions. Indeed, for those economists who in the 1930s and 1940s still believed that the goal of economics was to investigate the actual working of market forces, and not just formal equilibrium relations, there was little doubt that in the case of a contradiction between the necessary assumptions to establish equilibrium and the observations taken from real market interaction, something had to be conceded with respect to the former more than to the latter. Moreover, some critics argued that if firms were indeed endowed with perfect foresight, the outcome of their interaction would surely be the most profitable one, namely, the joint monopoly—and this not only in the duopoly case, but even with \( n \) firms.

In the following subsections I analyse three instances of these criticisms, namely, those by Stigler, Kahn and, above all, Fellner, whose 1949 book ultimately demolished the whole CV approach. The criticisms are presented according to their increasing effectiveness.

5.1 A curious result: Stigler, 1940

The first criticism is that by George Stigler. In a 1940 paper, Stigler argued that the basic problem of duopoly theory was how much additional knowledge should be attributed to each firm in addition to the full knowledge of the demand function and of its own cost function (Stigler, 1940, p. 524). He noted that when firms are endowed with perfect knowledge, a curious result arises. Two such firms would inevitably form a joint monopoly, but the argument could be extended to any number of firms: even a million firms, if endowed with perfect knowledge, would not fail to realise that the joint monopoly situation was the best for them! Yet, if all firms in all industries behaved like that, none of them would benefit from the collusive behaviour, since while it would sell its output at the monopoly price, it would also pay the monopoly price for its inputs. Anticipating this, no firm would want to collude in the first place, and so on ad infinitum (ibid., p. 525).

To avoid this logical puzzle, Stigler suggested that the perfect knowledge hypothesis should be discarded: each duopolist should be endowed with all the relevant information, except the knowledge of what its rival would do (ibid.). This assumption of quasi-perfect knowledge emphasised that the crucial issue of duopoly theory was precisely that of anticipating the rival’s behaviour; yet it also entailed that no general results could be reached in this respect: all economists could do was to deal with special cases, each characterised by an \( ad \ hoc \) assumption as to each firm’s beliefs about its rival’s reaction. This also included the case in which such beliefs were indeed correct, though Stigler remarked that the correct conjectures solution to duopolistic indeterminacy should not be credited with general validity, but at most with a benchmark role (ibid.). Moreover, the attribution of such a role to the correctness condition entailed that the generic CV terms introduced by Bowley had little, if any, relevance for the analysis. As Leontief had proved, in fact, whenever firms’ CVs were mutually incompatible or inconsistent with their rival’s actual behaviour, their usefulness as a guide to explain market interaction was nil (ibid., p. 528).

\[ \text{For a similar argument, see Morgenstern (1976 [1935], pp. 181–2), who added that, since in a world of universal monopoly the whole price system would become indeterminate, the question would be raised of how firms could perfectly foresee the prices in the first place.} \]
A stronger rejection of the correct conjectures condition came in a 1937 paper by Richard Kahn. The Cambridge economist explicitly placed the conjectural element at centre stage by claiming that there could be no unique solution to the duopoly problem because every position of equilibrium depended on the nature of the two competitors’ mental processes (Kahn, 1937, p. 2). He recognised that there was indeed a possible hypothesis for such processes which warranted the attainment of a unique position of equilibrium, namely, Harrod’s 1934 assumption of ‘sensible thinking’, where each firm was said to be capable of formulating a correct conjecture about its rival’s behaviour, and thus of following a demand curve that made full and correct allowance for such behaviour (ibid., pp. 2–3). Yet, he believed that the correct conjectures approach was hopelessly flawed, the fault lying in the interdependence between the duopolists’ demand curves and reactions.

Kahn’s argument ran as follows. Take firm 2’s demand curve. Each point on this curve corresponds to a certain equilibrium behaviour of firm 1. Harrod’s suggestion was based on the shift in firm 2’s demand caused by a change in firm 1’s offer, that is, on the size of the slope of firm 2’s RF. However, whenever firm 1 modifies its own offer, this is not a change in firm 1’s position of equilibrium, but rather a movement out of equilibrium (which may be done with the explicit goal of seeing what happens to firm 2). Since firm 2’s demand curve is drawn on the assumption that for every point on it firm 1 is in equilibrium, any such movement by firm 1 implies that firm 2’s demand ceases to have any significance. In terms of our basic duopoly model, the problem in Harrod’s (as well as Leontief’s) argument is that it is meaningless to impose the equality between a firm’s CV and the actual slope of its rival’s RF since the latter ceases to exists as soon as the variation featuring in the denominator in the former takes place. In short, firm 2’s reaction depends on firm 1’s equilibrium behaviour, but firm 1’s deviation is not equilibrium behaviour.

Kahn was aware that the real issue resided in the troublesome combination of statics and dynamics in duopoly theory in general, and in Bowley’s approach in particular: any CV term, being based upon a belief about the rival’s adjustment policy, was a pseudo-dynamic notion which clashed with the static character of the other elements of the basic duopoly model, as for instance, the RFs. As an alternative, he proposed (ibid., pp. 15–6) a mechanical, trial-and-error process leading to a short-run equilibrium based upon the knowledge of, rather than the belief about, the rival’s reaction—in other words, a return to a kind of ‘objective’ equilibrium, independent of the firms’ expectations and conjectures.

While Kahn’s general attitude towards the CVs turned out to be the winner in post-WWII oligopoly theory, it must be recognised that his specific effort to keep the epistemic variables out of the analysis of market interaction was a losing one. As was testified by the success of William Fellner’s Competition Among the Few, the prevailing approach became that of looking at the problems of imperfect competition from the viewpoint of the whole set of forces involved in the market process, including among them the psychology of the competing firms.

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1 The ‘reaction’ being, in the short run, that of not reacting at all, i.e., in the Cournot case, that of keeping the output unchanged: see Kahn (1937, p. 5).
5.3 The dismissal of CVs: Fellner, 1949

Fellner’s criticism of Bowley’s approach is contained in Chapter 2 of his highly influential 1949 book. In order better to appreciate this criticism it is necessary to begin with his objections to the Cournot model.

According to Fellner, the peculiarity of the Cournot solution was that duopolists’ beliefs proved correct only at equilibrium, while during the convergence process they were always mistaken (Fellner, 1949, p. 57). As he put it, the beliefs were ‘‘right’’ for the wrong reason: at equilibrium it was true that the rival went on producing a fixed output, but the reason for this behaviour was not that it behaved à la Cournot (i.e., neglected the other firm’s choice), but rather that this was what was dictated to him by his policy of profit maximisation via quantity adjustment (ibid., p. 58). From this remark, Fellner inferred that the intersection of the two RFs, i.e., the Cournot solution, did not represent a real equilibrium. It would if the firms were really to follow the RFs, but, like Kahn, he too argued that this was not the case, as it was unreasonable for both firms to do so when its rival deviated. Even if both firms did believe that its rival reacted along its own RF, they would immediately realise that the belief was incorrect, thereby destroying the RFs themselves. Thus, the system was unstable with respect to the firms’ doubts about their own beliefs about their rival’s behaviour (ibid., pp. 65–6).

At the bottom of Fellner’s reasoning lay the idea that duopolistic competition involved a real dynamic process towards equilibrium. The RFs could not hold during such a process because the latter took place in disequilibrium, while the former were static equilibrium notions. The dismissal of the RFs entailed as a corollary that the Cournot solution itself was useless, since it was an equilibrium concept for which we had no available explanation of ‘how and why’ it could be reached. Working in the tradition of the process view of economics, Fellner maintained that economists could never separate the equilibrium problem from the analysis of the disequilibrium mechanism. In the specific case of duopoly, it was pointless to isolate the intersection point of the RFs and study it as a static equilibrium in ‘pure’ theory, while leaving to ‘impure’ dynamic analysis the investigation of what happened when the system was out of equilibrium because, say, firms did not believe that their rival behaved à la Cournot (ibid., p. 67). Thus, Fellner explicitly rejected the idea that would later become so common among neoclassical economists of viewing the defining conditions of a static equilibrium as a separate and privileged issue with respect to disequilibrium analysis. To Fellner, as to many of the 1930s ‘high theorists’ before him, equilibrium was a useful notion only as long as it represented the end point of a dynamic process. This was even more true in the case of imperfect competition, where mental variables featured so prominently as to make a dynamic analysis capable of explaining the ‘how and why’ of equilibrium in terms of firms’ ever-changing conjectures the only kind of investigation that deserved to be undertaken (ibid., p. 94).

Given these premises, it is not surprising to discover that Fellner also rejected the CV approach. The approach was indeed nothing but an extension of the Cournot model in which the RFs were given a more general form. Thus some of the criticisms of the Cournot model extended also to the CV approach. First, the CVs were incorrect

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1 Fellner added (ibid., p. 66) that the stability property of the Cournot solution was of no help, because it was a property that referred to the movements away from equilibrium, not to the RFs or the actual convergence process.
outside the equilibrium, so that the equilibrium itself could not be reached for the reason already mentioned: whatever the original beliefs, they would be changed as soon as they turned out to be wrong, thereby making the CVs a useless tool (*ibid.*, p. 73). Second, as in the Cournot case, the conjectures were just ‘quasi-correct’ even at equilibrium, i.e., right for the wrong reason. The pair of equilibrium outputs were justified on the basis of entirely arbitrary beliefs on each firm’s reaction. Such beliefs were indeed correct at equilibrium—in the limited sense that each firm actually produced what the rival expected it to produce—but their correctness rested upon a mistaken idea of the rival’s behaviour. Again, any doubt concerning these beliefs would destroy the validity of the RFs. It followed that the CV model could maintain no claim of superiority with respect to the Cournot one (*ibid.*, p. 74).

Fellner seemed, therefore, to acknowledge that even if, as required by the correct conjectures condition, the CVs happened to match the slope of the RFs, this did not suffice for the stability of equilibrium, because in case of a deviation by any of the firms the system would not go back to equilibrium since the RFs would be destroyed by the deviation. This proved once more that a static equilibrium condition could not be used to support a dynamic process. Any movement away from equilibrium, say, along one of the RFs, revealed to the firms the mistaken nature of the hypotheses upon which the RFs were based. The hypotheses would then be modified and new RFs would arise, but this would simply show the analytical uselessness of a tool that did not stay unchanged whenever its independent variable was modified. Fellner’s conclusion was sweeping: the traditional idea—also crucial for the CV approach—that the intersection of two independent RFs determined the equilibrium of the duopoly model could never constitute a fruitful foundation for oligopoly theory (*ibid.*, p. 94). This marked for a long time the end of the CV approach as a legitimate research technique and relegated it to the role of a mere didactic device.1

6. Conclusion: the winning trend in post-war oligopoly theory

Looking at the literature on duopoly theory of the period from the early 1920s to the late 1940s it is possible to single out two different research trends. On the one side, there were those economists who claimed that the outcome of duopolistic competition need always be determinate, so that proper theoretical assumptions about firms’ beliefs should be formulated to tie down the model’s solution. On the other side, there were those authors who seemed ready to accept that the solution might be indeterminate, or, better, who did not accept that the price to be paid for achieving the determinateness was to place ad hoc assumptions on the duopolists’ conjectures.

The most advanced theoretical position reached by the former group was that of the authors who solved the duopoly model by imposing a consistency condition on Bowley’s CV terms. This way out of oligopolistic indeterminacy represented a double improvement with respect to the naive assumptions of the traditional Cournot,

1 The CV approach regained momentum only in the 1980s thanks to the CCE literature and, above all, to the interpretation of the CV terms as reduced form parameters summarising the unspecified—and possibly very complex—patterns of behaviour followed by competing firms (cf. Schmalensee, 1989, p. 650). Today it is considered a more useful tool for empirical research in industrial economics than most game-theoretic models (cf. Martin, 1993, p. 30). Efforts have also been made to explicitly model the dynamic oligopoly game underlying the CV solution: see Dockner (1992) and Cabral (1995).
Bertrand or Edgeworth models. First, no firm was required any longer to neglect ‘irrationally’ the influence that its own actions exercised upon its rival’s behaviour, as the role of the CVs was precisely to account for the full duopolistic interdependence. Second, the recourse to the firms’ conjectures did not affect the determinateness of the equilibrium, since the consistency condition effectively constrained the value of the CV terms. Furthermore, the kind of conjectural equilibrium that emerged from the new approach was fairly in line with the contemporary theoretical developments at a more general level, epitomised by Friedrich Hayek’s definition of equilibrium as mutual consistency of plans (Hayek, 1937).

However, this kind of solution had at least two serious drawbacks. First, it required duopolists to be endowed with perfect knowledge and the ability to foresee correctly their rival’s reaction. Second, the solution begged the fundamental issue of the relation between statics and dynamics, so that it was not difficult for the most acute critics to point out the inconsistency of employing a static equilibrium notion, like the reaction function, to investigate dynamic disequilibrium phenomena. To make things worse, the two drawbacks were tightly related. To escape the second one, in fact, one could argue that the firms’ interactions were simultaneous and that the whole adjustment process was just a mental game performed in the duopolists’ mind. This way out would, however, worsen the first drawback, by requiring the duopolist to possess the intellectual ability to implement and uniquely solve such a game.

The limits of the correct conjectures approach brought victory to the second research trend, that is, to those economists who accepted the indeterminacy result. It is hardly surprising that these were also the authors who brought forward the general idea that the traditional tools of economic theory did not suffice to investigate oligopolistic competition. Less mathematics and more field work was indeed the winning slogan in post-WWII oligopoly theory. A proper analysis of imperfect competition had, therefore, to be more empirically oriented, as well as open to account for the psychological and sociological elements of market behaviour. The observation of real competitive processes should suggest to researchers which variables—beyond the strictly economic ones—to add to their models, while the requirements of mathematical rigour had to be loosened in order to accommodate the new variables. The introduction of these non-economic features would in turn make it easier to achieve a complete taxonomy of the possible outcomes of oligopolistic interaction.

One champion of the prevailing methodology was the same William Fellner who had so effectively demolished the CV approach. A few passages from his 1949 book should suffice to exemplify the new trend. The book opened with the surprising statement that orthodox value theory suffered from serious limitations that prevented it from giving a sufficiently realistic account of the formation of prices in a modern economy where competition was far from perfect (Fellner, 1949, p. 3). Yet, Fellner believed that the solution could not be given by an improvement in the mathematical details of the theory, but on the contrary by loosening the standards of rigour (ibid., p. 7). Actually, the fact itself that the heart of the oligopoly problem lay in the notion of interdependence increased the amount of information required to explain the

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1 See, for example, the explicit statements in Mason (1939, pp. 62–5), the pre-war manifesto of the structure-conduct-performance approach that was to dominate industrial economics until the late 1970s.

2 Surprising, I mean, from the viewpoint of a self-recognised neoclassical economist.
outcome of a specific market situation and put such an explanation beyond the reach of pure mathematical analysis. As a consequence, Fellner claimed that a part of the necessary information had to come from the kind of material that ‘one would prefer not to rely on in a search for dependable answers’ (ibid., p. 14), namely, on the socio-psychological features of the market.

It turns out therefore that the post-WWII development of oligopoly theory took the opposite direction with respect to the ever increasing use of mathematics in neo-classical economics. This shows that the consolidation of the image of economics as a formally rigorous discipline was hardly a uniform process in the various sub-fields of research. Despite the fact that the consistent conjectures approach had led it to the threshold of the game-theoretic notion of a CCE—itself a concept closely related to that of a Nash equilibrium—the citadel of oligopoly theory remained a stronghold of the traditional view well into the 1970s. This is a fact that should never be forgotten by those historians who have tried to explain the failure of modern game theory to receive due credit from post-WWII neoclassical economists. The sub-field that was the most obvious target for the new strategic notions and techniques happened indeed to be also the one that most fiercely rejected the idea that economics should deal only with fixed points, existence conditions and the like.

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