Oligopoly, Mutual Dependence and Tacit Collusion: The Emergence of Industrial Organisation and the Reappraisal of American Capitalism at Harvard (1933–1952)

Chirat Alexandre et Guicherd Thibault. (Version du 8 février 2021 – révision EJHET)

Abstract: This article looks back at the early development of industrial organisation at Harvard. It seeks to understand the emergence of the "Harvard tradition" around the spread of a set of common and identifiable tools and concepts. The paper identifies a specific subject of study bringing together a group of economists. This is the hypothesis of "mutual dependence recognised", which fosters the development of the theory of tacit collusion in oligopoly. This theory was developed by Edward Chamberlin (1933) and gradually taken up in several contributions from the 1930s and early 1940s by economists like Bain, de Chazeau, Galbraith, Kaysen, Mason, Schumpeter and Triffin. These authors, who all had connections with Harvard, appropriated Chamberlin's theory in pursuit of four goals. First, the possibility of tacit collusion in oligopoly allowed them to provide theoretical grounds for industrial price rigidities. Second, the oligopoly issue fostered the development of new tools for identifying oligopolies and accounting for firms' behaviour and strategic interaction. Third, these tools were regularly mobilised in debates among economists about the "basing point system". This pricing method was used at the time in the iron, steel and cement industries and led these economists to address the question of how effective antitrust laws were. Fourth, it led some Harvard economists to entirely reappraise the very nature of mid-century American capitalism.

Keywords: oligopoly, mutual dependence, tacit collusion, Industrial Organisation, Harvard.

JEL Codes: B21 – L10 – L43 - D 43– K21

0. Introduction

It is generally acknowledged that the Harvard Department of Economics of the 1930s was one of the main centres for innovation in this period of "High Theory" (Shackle 1967). Harvard was the Ellis Island of the Keynesian revolution[1] and the birthplace of both monopolistic competition theory and the emerging field of Industrial Organisation (IO). While the dissemination and reshaping of Keynes's ideas from Harvard throughout the United States has attracted much attention, the emergence of IO at Harvard has been somewhat overlooked in spite of its impact on both economic analysis and public policy.

Obviously there is some literature on IO at Harvard. Jean Tirole, for one, speaks of the "Harvard Tradition" (1988, 1-4). Like many others, he equates it to the "Structure-Conduct-Performance" (SCP) paradigm developed by Joe Bain. However, the SCP paradigm only appeared as a structuring tool in the mid-1950s (Schmalensee 2012, 162). It is the preceding period that is of interest here. Richard Posner (1978) preferred to speak in terms of the "Harvard School on antitrust policy". He defined it through its post-war opposition to the Chicago School. The existence of a "Harvard school" is almost always taken for granted. Historians of economic thought have looked for its origins.[2] Emphasis has been placed on the role played by the two "Eds", namely Chamberlin and Mason. Harvard's tradition of case studies, illustrated by William Ripley's course on corporations, is put forward to explain the empirical stance of the subdiscipline. This literature also traces back the roots of IO to the work of pioneers of original institutional economics (R. Ely, J. Commons and T. Veblen) and their successors of the interwar period (J. M. Clark, W. Hamilton and W. Ripley).[3] In a recent paper on "American economists" who "sought to explain the Great Depression as a failure of competition", Roger Backhouse deals with the work Adolf Berle, Edward Chamberlin, John Maurice Clark and Gardiner Means. He argues that the links between their work and the "Harvard school of industrial economics" are "easy to trace" (2015, 121). However, it is a story that has not yet been written because the emergence of a Harvard Tradition in Industrial Organisation has yet to be demonstrated.

The aforementioned papers implicitly convey a vision of the spontaneous generation of IO at Harvard. They fail to account for the spreading of ideas and concepts among Harvard

[1] Hansen, Harris and Samuelson played prominent roles in these stories. On the Keynesian revolution at Harvard, see Galbraith (1965, 1981), Samuelson (1988), Tobin (1988), Colander and Landreth (1999) and Backhouse (2017).
[2] See Arena (1991), Phillips and Stevenson (1974) and Grether (1970). On the Chicago School and its divergence from the Harvard tradition, see Bain (1986) and Giocoli (2015).
[3] Phillips and Stevenson (1974), Rutherford (2011).

members.[4] All too often they involve an *a posteriori* grouping of actors based on the similarities of their research topics, but without any institutional and historical evidence of the way those scholars interacted. This paper looks to supply some of the missing links in the story of the emergence of Industrial Organisation at Harvard. Emphasis is placed on the diverse paths its development took. The starting point is to identify a specific group of economists. The task is not as easy as Backhouse gives us to believe. During the 1930s, the Harvard Department of Economics reflected the "interwar pluralism" of American economics (Morgan and Rutherford 1998). Far from being monolithic, a complete renewal of its faculty was underway. Three former PhD students successfully defended their PhD theses – Seymour Harris (1923), Edward Mason (1923) and Edward Chamberlin (1927). Recognised American economists – such as John D. Black (1927) in the field of agricultural economics, Sumner Slichter (1930) in labour economics and Alvin Hansen (1937) in business cycles – and prominent European immigrants – such as Leontief (1931), Schumpeter (1932) and Haberler (1936) – also joined the department. In the mid-1930s, Alan Sweezy and John Kenneth Galbraith went there as instructors. Many young economists passed their PhD grades, including Joe Bain (1940), John Cassels (1934), Gardiner Means (1933), Richard Musgrave (1937), Paul Samuelson (1940), Paul Sweezy (1937), Robert Triffin (1938) and Donald Wallace (1931). All these names testify to the diversity of epistemological approaches and to the areas of interest to be found in the Department. That is precisely why it is worth asking who really participated at the time in the emergence of a Harvard Tradition in Industrial Organisation.

There are three reasons for speaking here of a "tradition" rather than a "school". First, Mason and Bain are the only two figures systematically involved, with others being added or removed at the economic historian's discretion. But two authors do not form a school of thought. Second, "school" conjures up a vision of a leader consciously trying to build a school of thought. The paper shows that the story behind the emergence of IO is far less linear. Third, "school" does not reflect the specific role played by the Harvard Department of Economics as a professional community. Its members shared courses, attended seminars, collaborated on research projects, met up to deal with administrative issues, edited academic reviews, and supervised students' research. But such links still do not make a tradition. So without some unifying criterion, it seems impossible to understand the emergence of a tradition at Harvard.

In this paper, we identify a specific concept delimiting a group of economists who contributed to the establishment of IO from the 1930s to the 1950s. This unifying notion was

[4] See also, for instance, Stephen Martin's book *Industrial Organization in Context* (2010, 16).

the hypothesis of "recognised mutual dependence", a hypothesis that potentially led them to reason in terms of "tacit collusion" among oligopolists.[5] The expression employed in this paper – "a group of economists" – is voluntarily loose. Indeed, we do not fully recognise this group as a genuine research community. We simply gather together a set of economists, belonging to a common professional community, who share common fundamental ideas. These ideas are a point of convergence among economists who endorse a variety of approaches. Moreover, using the loose notion of "group" means we can dispense with any *a priori* definition of the bounds of that group. Rather than viewing the group as a permanent phenomenon, we prefer a shifting approach that tracks the spread of the concepts of "recognised mutual dependence" and "tacit collusion" in oligopoly theory, thereby providing an understanding of the trajectory of the group and its individual members.

The first protagonist in our narrative is Edward Hastings Chamberlin, who was at that time the "rising star" of Harvard (Backhouse 2017, 111). The publication of his PhD dissertation (1927), *The Theory of Monopolistic Competition* (1933), saw the first occurrence of the term "oligopoly".[6] This concept appeared "revolutionary" to some of his colleagues in two respects (Bain 1964, 29).[7] First, it enabled the study of a particular intermediate case between the two ideal type cases of pure competition and pure monopoly. Second, Chamberlin analysed the behaviour of firms in oligopolistic markets by making new assumptions about their strategic interactions. The idea of tacit collusion in oligopoly hinged on the assumption that firms had some "recognition of their mutual dependence" (Chamberlin 1933, 46-7). Thus, competition referred not only to market structure but also to the strategic behaviour of competitors. This concept was directly linked to the emergence of Industrial Organisation. It served to build the first market classifications that were to be found in the work of Machlup (1937), Mason (1939), Clark (1940), Triffin (1940) and Bain (1942). These classifications were useful tools for understanding how a firm's environment affected its behaviour and how its behaviour shaped the market's outcome. They plainly led to the now famous Structure-Conduct-Performance paradigm. However, that three-stage sequence of events was not initially a one-way chain of

[5] It should be emphasised immediately that the expression "tacit collusion" did not have the precise and formal meaning it could have for researchers in IO today. In other words, tacit collusion, for members of our group, did not necessarily mean the maximisation of joint profits among oligopolists, as it did for Chamberlin. Its general meaning is that oligopolists did not engage in price wars and practised prices above the competitive level, so that they secured extra profits. On this general meaning and more specific ones, see Ivaldi et al. (2003, 5).

[6] On the genesis of Chamberlin's theory, which is beyond the scope of this paper, see Guicherd (2017, 2018).

[7] The problem of collusion had already been discussed by Adam Smith (Salvadori and Signorino 2013, 161). And the idea of oligopoly and tacit collusion is attributed to Cournot (1838). But what matters in this context is how Chamberlin's colleagues viewed his works and how it impacted theirs.

cause and effect. Moreover, reducing IO at Harvard to the SCP paradigm would not explain how this subfield of economics gradually emerged.

Our focus on the dissemination of the concepts of recognised mutual dependence and tacit collusion enables us to account for the formation of a limited "group of economists". The pluralism of the Harvard Department of Economics accounted for the diverse paths by which these concepts were reappropriated. Among the works produced by the group members, three kinds should be distinguished, relating to theoretical issues, instrumental issues and politico-economic issues.[8] First, the concept of tacit collusion in oligopoly prompted reappraisals of economic analysis. Chamberlin's *Theory of Monopolistic Competition* and Triffin's dissertation which Chamberlin supervised, illustrated this, since they sought to provide an alternative to the theory of competition and industry inherited from Alfred Marshall. Galbraith (1936) followed by Mason (1938) considered the behaviour of oligopolies, in the way Chamberlin theorised it, as one of the main causes of differential price rigidities between sectors. This issue was prominent in early research in Industrial Organisation, especially because of the Great Depression and the implementation of the National Industrial Recovery Act. It also had close connections with trade cycle analysis (d'Aspremont et al. 2011). Tacit collusion in oligopoly, explained by the hypothesis of recognised mutual dependence, led to a reappraisal of price theory so that it could better fit reality and provide micro-foundations for macroeconomics. In the same vein, the price behaviour of oligopolies became a subject of theoretical debate and a reason for the practical implementation of price controls at the Office of Price Administration.[9]

The second kind of work was more applied than theoretical. American economics went through an interlude in terms of theory between the 1930s and the 1960s (Backhouse 1998). But Chamberlin's theoretical concepts were appropriated by some Harvard economists for empirical analysis in order to tackle practical problems. Mention can be made of a report on the iron and steel industry, referring explicitly to the concept of tacit collusion in oligopoly (Daugherty et al., 1937). In the 1940s and 1950s, this problem was at the core of a major controversy involving Harvard economists over the consequences of the basing point system. This was a pricing system that typically arose in oligopolistic markets. In this debate, the way tacit collusion was considered determined the perception of antitrust laws. Either collusion was explained by explicit agreement – leading to cartels – which was thus judged as an anti-

[8] In reviewing this paper, we discovered that our three-way divide echoes, without being identical to, one produced by Machlup (1939). He recognises three kinds of economist: the "economic theorist qua theorist", the "economic analyst as observer of concrete situations" and "the political economist as government official" (1939, 227-8).

[9] See for instance Galbraith's (1941) and Clark's (1941) answers to Hansen (1941) concerning the measures required to counter wartime inflation.

competitive practice, or it arose as the result of the rational behaviour of firms and had to be considered the expression of the process of competition. Members of our group of economists at Harvard mainly endorsed this second view because of the idea of a specific "oligopolistic rationality". By this expression, Carl Kaysen meant that "the realization by each seller that his cuts will be followed by his rivals" – that is to say, the recognition of mutual dependence – explained the absence of cut-throat competition and consequently the greater price rigidity in oligopolistic sectors (1949a, 294).

As our narrative provides the rationale behind their general scepticism about antitrust laws, it leads us to consider a third kind of work dealing with the growing importance of oligopoly throughout the American system. In the spirit of nineteenth-century political economy, some economists used Chamberlin's theory as a starting point in their attempts to produce an analysis of the dynamics of twentieth-century capitalism. The works of Schumpeter (1942), Clark (1948) and Galbraith (1952a) were representative of this reappropriation and extension of Chamberlin's concerns. As the idea of oligopoly could not be separated from the growing concern about concentration and the emergence of great corporations, they discussed the dilemma of modern American Capitalism torn between efficiency and competition. Backhouse (2015) shows that, as early as the 1920s, American economists began to address the failure of competition. The concomitance of the Great Depression, the Keynesian revolution and the monopolistic competition revolution gave fresh impetus to those reflections on the changing nature of capitalism. Many economists were losing faith with the competitive system and growing uncertain about the foundations of economics.[10] Their beliefs were also shaken by controversies over resource allocation under socialism, the "managerial revolution" and the debate over "secular stagnation" that also raged in the very yard of Harvard.[11]

As stated above, the aim of this paper is to show that an informal group of economists, sharing Chamberlin's ideas about recognised mutual dependence and oligopolistic tacit collusion, fostered the early developments of IO at Harvard. To this end, we use both published literature and archive materials from Edward Chamberlin's and John Kenneth Galbraith's papers. These records shed new light on the relationship among some of the protagonists. They provide new historical insights into how Chamberlin's oligopoly theory was at the core of re-examinations of the concept of competition, reappraisals of price theory, specific case-studies,

[10] For instance, on 8 August 1941, Frank H. Knight, as a leading interwar American economist, wrote to Clarence Ayres: "I have to say frankly, that between the institutionalists, the planners, and the Keynesites, I no longer pretend to know what is economics or what is an economist!" (Samuels 1977, 509).
[11] See Hansen (1938), Schumpeter (1942), A. Sweezy (1943) and P. Sweezy (1942).

debates over antitrust legislation and renewed visions of mid-century American capitalism. Our narrative begins by presenting the unifying theoretical content of Chamberlin's concept of tacit collusion among oligopolists, which hinged on the hypothesis of recognised mutual dependence (Section 1). We then focus on the direct dissemination of these concepts to attest that a group gradually emerged at Harvard between 1933 and 1937 (Section 2). Having identified this group, we show how, between 1937 and 1942, the notion of oligopoly was used to reappraise competition through the construction of a new tool – classifications of market structures (Section 3). This IO tool served two purposes: providing theoretical insights to drive empirical studies and evaluating antitrust legislation. We then show that the ideas framed by Chamberlin, and their reappropriation by his colleagues, were central to the basing point system controversy (Section 4). Finally, we examine some attempts by Harvard economists who seized on the oligopoly issue to use it for systemic reflection on the nature of mid-century American Capitalism (Section 5).

To conclude this introduction, we would like to provide a justification for the temporal framework in the paper. We choose to speak about a specific group of economists since Chamberlin's notion was quickly seized on. However, had we ended the story at the beginning of the 1940s, when the subfield of IO became institutionalised, we would have missed out on some major ramifications of its spread. There are three main reasons for ending our story in 1952. First, the SCP paradigm became dominant around the mid-1950s. Second, 1952 was the year of publication of *American Capitalism* by John Kenneth Galbraith, in which he appropriated, combined, discussed and synthetised the ideas of many contributors from the group under study. He referred for instance to Chamberlin (1933), Galbraith (1936), Clark (1940), Bain (1942), Schumpeter (1942), Mason (1949) and Machlup (1949) so as to provide a general theory of competition in modern America. The third reason is the publication of the two volumes of the *Survey on Contemporary Economics* sponsored by the American Economic Association. They crystallised economic knowledge of that time. In the first volume, initially published in 1948 and printed in 1952 for the third time, Joe Bain wrote the chapter on "Price and Production Policies". This was the first extensive review of the IO literature (Bain 1948). In the second volume, Andreas Papandreou, a Greek economist also at Harvard until 1947, wrote a chapter entitled, "Some Basic Problems in the Theory of the Firm" (1952). Both Bain and Papandreou suggested that game theory could become an important tool for the future of IO and the theory of the firm. This new framework seemed particularly suitable for dealing with the strategic interactions characterising oligopolistic markets. This association between game theory and Industrial Organisation might be taken for granted by contemporary researchers in

this field. But even if Von Neumann and Morgenstern published their book in 1944 and Morgenstern applied it to the problem of oligopoly in 1948, the association between game theory and IO did not become prominent until 1952. Accordingly, our paper provides a part of the history of IO before its turned to the SCP paradigm on the one hand and to game theory on the other.

1. Chamberlin's theory of oligopoly (1927–1933)

Several authors claim the term "oligopoly" was introduced into economics by Chamberlin.[12] Chamberlin himself shows that this statement is incorrect (Chamberlin 1957). In economics, the adjective *oligopolistische* was first used in the book *Theorie des Geld und Kreditwirtschaft* by Karl Schlesinger (1914). If "oligopoly" and all its derivations in English seem to come from Chamberlin,[13] the study of market situations where the number of competitors is small can be found from almost a century earlier. It went back to Antoine Augustin Cournot's *Recherches sur les principes mathématiques de la théorie des richesses* (1838). Before 1933, oligopoly had never been named and barely studied. The situation with a limited number of sellers was systematically treated through duopoly as a simplified situation where two competitors faced one another. In this respect, Chamberlin was no exception. Like his predecessors, he built his oligopoly theory as a generalisation of a duopolistic situation.

Oligopoly theory derived from pre-existing duopoly theories.[14] Despite the similar initial conditions, Chamberlin noticed that Cournot, Bertrand and Edgeworth came to contradictory conclusions. To solve this divergence, he proposed to reformulate their models by adding a new hypothesis to unify them into a single solution. In his duopoly theory, Chamberlin supposed a capacity for anticipation and farsightedness by competitors. This differed from preceding duopolies where competitors were just acting by considering opponents' prices and quantities as fixed, given and exogenous.[15] In Chamberlin's model, this hypothesis enabled the two enterprises to forecast all the consequences of their actions and consequently to directly choose the most profitable option. More than that, competitors understood their common interest in collusion and none of them had any incentive to deviate. In Chamberlin's model, collusion was

[12] Nichol (1934), Schumpeter and Nichol (1934), Stackelberg (1934).
[13] As early as 1929, Chamberlin had the term "oligopoly" in mind and it should have appeared in his article "Duopoly: Value Where Sellers are Few". The *Quarterly Journal of Economics*' editor F. W. Taussig rejected it. He considered the word a "monstrosity" (Chamberlin 1957, 33-35).
[14] There are versions of Chamberlin's duopoly and oligopoly: the third chapter of his 1927 PhD thesis, the 1929 article and the third chapter of *The Theory of Monopolistic Competition.* We refer mostly to the latter.
[15] Known as the reaction function in standard microeconomics.

stable. If applied to Cournot's and Edgeworth's cases, this hypothesis led to a situation of tacit collusion as well. Rational behaviour for competitors was to share the monopoly profit by splitting the monopoly quantity and charging the monopoly price.

To go from duopoly to oligopoly, Chamberlin increased the number of competitors in his model to show that his result remained valid as long as competitors were able to recognise their mutual dependence. The impact of one competitor's action on the price, quantity and profit of other competitors was the specific source of its market power. However, it gradually faded away as the number of competitors increased. This strengthened the uncertainty around the capacity of rivals to properly take account of their interdependence. Chamberlin argued that the boundary between the existence and the absence of mutual dependence recognition was porous. If a critical number of competitors was attained, the price fell directly from the monopoly level to the competitive one as soon as competition became "pure" (Chamberlin 1933, 48). However, Chamberlin gave no clue as to how to determine this number of competitors.[16]

In this approach, oligopoly was a full-blown market structure leading to specific results that were very different from the results of monopoly, monopolistic competition (product differentiation) or pure competition. It was, however, based on a hypothesis about competitors' behaviour that could be generalised to any case. Firms' ability to use such capacity to foresee might vary depending on market situations. The recognition of mutual dependence was irrelevant in a monopoly situation – unless the firm considered the threat of potential competition.[17] Machlup expressed that idea by stating that "both the monopolist and the competitor in a market of very many sellers are unconcerned with rivals' reactions; the one because he has not any, the other because he has too many" (1937, 446). In a letter to Machlup, Chamberlin argued "to say that the monopolist has no rivals is another way of saying that he has so many that no one of them would care about what he does".[18]

We do not want to discuss in detail Chamberlin's oligopoly theory and its blind spots.[19] Those theoretical gaps did not prevent others at Harvard from building their analysis on the intuition of mutual dependence and collusive equilibrium in oligopoly. Despite the ground-breaking aspect that his concept of oligopoly had in the eyes of many of his contemporaries,

[16] This idea is captured in contemporary IO by the concept of "critical concentration ratio". A recent empirical study shows that there is "a significant increase in tacit collusion from four to three firms as well as from three to two firms" (Horstmann et al. 2018, 651).

[17] On potential competition, see Clark (1923, 1940), Schumpeter (1942) and Bain (1950).

[18] Chamberlin to Machlup, 20 January 1937, *Chamberlin Personal Papers* (*CPP*), *Duke University* (NC).

[19] Chamberlin did not provide any formal evidence for his duopoly. Actually, he simply reconciled the contradictory results in Cournot's and Edgeworth's diagrams by adding the hypothesis of mutual dependence recognized. He was then able to reach an identical result on both diagrams.

two main problems remained. First, Chamberlin's oligopoly could not claim any degree of generality. Concerning agents' ability to affect the market structure, Chamberlin left a significant void. He simply stated that the distinction between pure competition and oligopoly relied not only on each competitor's recognition of their influence on others, but also on their awareness that their responses would be conditioned by their rivals' awareness of that interdependence. In other words, even if the interdependence was clear and well known, each competitor's propensity to take up and abide by the optimal private choice was uncertain. Once again, the results would depend on the number of sellers and the recognition of their mutual dependence. But Chamberlin remained vague on this issue.

Secondly, there still remained important ambiguities about the links between an oligopoly situation (the number of competitors) and the theoretical framework of monopolistic competition (product differentiation) in which it was poorly integrated. The articulation between situations of oligopoly and monopolistic competition was blurred, especially when competitors applied the latter when the former was not possible. In a situation of oligopoly, the production on the market could be assumed to be homogenous which could then be called the "industry". This was no longer true in monopolistic competition where the notion of "group" prevailed along with the problem of the definition of its boundaries. According to Chamberlin, the differentiated oligopoly, or "small group" was practically relevant and important to discuss (Chamberlin 1933, 100-4). That was why, in later works, he redefined the concept of monopolistic competition as a combination of both oligopoly and product differentiation (Guicherd 2018).[20] This was exactly how Schumpeter (1942) and Galbraith (1952) were to define monopolistic competition in their respective reappropriations.

The withdrawal of the notion of the Marshallian industry allowed a group to be defined in terms of the interactions among its members. In this respect, this idea was an important theoretical tool that Chamberlin provided and which besides legitimated the difference between his theory and Robinson's imperfect competition. In the latter, strategic interactions were absent and market imperfections, including product differentiation, were assumed to be given and exogenous. Their contemporaries thus faced a kind of dilemma: should they adopt the imperfect and monopolistic competition theory on the basis of the general principles and hypotheses they shared?[21] In this case, the theory enabled economists to describe the whole continuum of

[20] See also Machlup (1939, 230).

[21] Despite the difference granted by the integration of strategic behaviour, many common points are noteworthy. The most striking include the use of marginal curves for the equilibrium of the individual firm or the tangency solution.

possibilities between monopoly and pure competition by assuming that competitors would adapt their policies according to given market structures. Or should they recognise the preponderance of strategic interactions causing differentiation and making the Marshallian notion of industry obsolete? If so, competitors' behaviours would influence the structure of the market and break the determinism of price and quantity equilibrium. The result of such strategic behaviour could lead to theoretical indeterminacy. Economists were then confronted with the question of knowing what a "normal price" was.

2. The early spread of Chamberlin's oligopoly at Harvard (1934–1937)

As stated before, Harvard's Department of Economics saw considerable turnover among its members in the 1930s. As Taussig and Ripley retired, new professors and PhD candidates brought in new blood. In this professional community, Chamberlin and Mason bridged the gap between the old and new generations. That is why they played a crucial role in the group of economists we have identified. Between 1927 and 1932, both were in charge of the courses on transport economics while Ripley taught the economics of corporations in the second semester. Starting from 1932, the two Eds taught both semesters with their course "Monopolistic industries and their regulation". Links between members of the department were embodied by their academic contacts and some directly found echoes in their theoretical contributions. An example was Donald Wallace's *Market Control in the Aluminum Industry* (1937a), which was considered to be the canonical case study in IO at Harvard (Grether 1970, 83). This book was adapted from his PhD thesis, defended in 1931 under the supervision of Taussig and Ripley. In the preface, Wallace acknowledged Chamberlin and Mason (1937a, x). He referred to "two discussion groups at Harvard" in which he was involved and where he presented his chapter XV, which discussed how an oligopolist's behaviour would depend on the characteristics of the market and the nature of its forecasts.[22] One of this group undoubtedly corresponded to the seminar on "Industrial Organization and Price Policy" organised by Mason. Even before being hired as professors, Chamberlin and Mason, in collaboration with their predecessors, had already been involved in discussions with other Harvard members and had influenced their work on some of the central issues of IO.

[22] Wallace also referred to Chamberlin when explaining that an oligopolist would avoid wasteful competition in advertising "because of the belief it would provoke similar expenditures by the others with no other result than an increase in expenses all around" (1937a, 198). Avoidance of advertising competition, as avoidance of cutthroat competition, explained as a consequence of the recognition of mutual dependence.

Being at the centre of numerous theoretical controversies, oligopoly could retrospectively appear to be a concept almost as old as modern economics. However, appearing to some as a new economic concept put forward by *The Theory of Monopolistic Competition*, it had to find its place in economics. As early as 1934, Heinrich Von Stackelberg used it in *Marktform und Gleichgewicht.*[23] The term was used in its etymological dimension and was often assimilated to the duopoly it was based on.[24] Oligopoly was connected to other theoretical issues like the optimal size of firms by Joan Robinson (1934) and the increasing demand elasticity with the number of competitors by Schumpeter and Nichol (1934). However, there was not, to our knowledge, any contribution from 1934 making a direct link between oligopoly, recognised mutual dependence and tacit collusion. The etymological use of the word oligopoly continued in 1935 and 1936. However, two contributions catch our attention because of their emphasis on mutual dependence and tacit collusion.

Geoffrey Shepherd used the term oligopoly in its Chamberlinian dimension in his article "Competition and Oligopoly" published in the *Journal of Farm Economics* (1935). Thanks to the possibility of tacit collusion in such a structure, he explained the differences in policies between the manufacturing and agricultural sectors during the Great Depression. Known as a statistician and econometrician, Shepherd taught first at Iowa State University and, after defending his PhD thesis, at Harvard in 1932. According to the acknowledgments, he interacted with Chamberlin (Shepherd 1935, 575-7). In 1936, John Kenneth Galbraith also mobilised Chamberlin's idea of recognition of mutual dependence among sellers. Galbraith was at that time instructor at Harvard and published a paper in *The Quarterly Journal of Economics* edited by the University.[25] He tackled the theoretical problem of explaining differential price rigidities – that is to say the higher rigidity in industrial rather than agricultural sectors – that had been empirically observed by Gardiner Means (1935). Galbraith referred to Chamberlin's book but also quoted his 1929 article "Duopoly: Value Where Sellers Are Few". Chamberlin's initial contribution on tacit collusion in oligopoly focused on single firms and their supposed behaviour, a methodology close to standard microeconomics. In his history of the Department, Edward Mason explained that the business cycle and other macroeconomic issues left Chamberlin "cold".[26] But with "Monopoly power and prices rigidity", Galbraith (1936) paved

[23] Expressions like "Cournot oligopoly" or "Bowley oligopoly" cannot be found before this publication, in which the author explicitly praises Chamberlin for coining the concept (Stackelberg 1934, 2).
[24] See Bitterman (1934), Schumpeter and Nichol (1934), Nichol (1934).
[25] Galbraith was originally trained in agricultural economics at Berkeley (1931–1934).
[26] Mason and Lamont (1982, 423).

the way to a process of reappropriation that would link microeconomics, industrial organisation and macroeconomics.[27]

Galbraith regarded the explanation of price rigidity by cost rigidity as unsatisfactory since agricultural costs were relatively sticky during the depression while prices fell. If price rigidity was indeed impossible in cases of pure competition, Galbraith argued that monopoly power alone failed to explain why "administered prices" should be more rigid.[28] In fact, if an enterprise with discretionary power over its prices and production had to keep them rigid in response to reduced demand, it was for reasons that still needed to be given. Galbraith found two sets of explanations. The first hinged on the assumption of profit maximisation but provided amendments regarding the firm's time horizon (short-run *versus* long-run maximisation) and the process of price adjustment (menu costs).[29] The second set of explanations started with the assumption of "a 'rational' selection of rigid prices" in terms of maximisation so as to consider the role played by customs, habits and strategic interactions in price policy choice. Here, Chamberlin's assumption of "recognised mutual dependence" enabled Galbraith to explain differential price rigidities between oligopolistic industrial sectors and competitive agricultural ones. He argued that more rigid pricing policies acted as a convention to reduce the uncertainty of sellers' behaviour.

> Students of duopoly and oligopoly have noted a variety of assumptions which may be made as to the foresight of individual sellers and the degree of interdependence which they recognize in formulating their price and production policies.[30] It is believed, however, that there will be general agreement with the suggestion that the case of most practical importance is where the individuals concerned behave, however imperfectly, as though there was but one – that they recognize fairly completely the interdependence of their fortunes. The extreme likelihood in practice that tacit or formal understandings will be present under oligopoly situations (or rather will replace them) increases the probability of such a result (Galbraith 1936, 466).

[27] This paper was written and published before Galbraith read Keynes's *General Theory*. Galbraith spent a semester at Cambridge (UK) in 1938. There he met John Dunlop, a PhD candidate from Berkeley, who rapidly became a member of Harvard. When Dunlop wrote against Keynes's assumption of a negative correlation between nominal and real wages, he explicitly referred to Galbraith's (1936) paper for potential theoretical explanations (Dunlop 1938, 47). On Dunlop (1939) and Galbraith (1936), see d'Aspremont et al. (2011).

[28] In the same way as Chamberlin defined monopoly power as control over supply, Galbraith defined monopoly power as follows: "the power of the seller to exercise control over supply and price, regardless of whether he is enabled to do so as the result of a monopoly position or because of a condition of duopoly, oligopoly or monopolistic competition in the industry" (Galbraith 1936, 458).

[29] Means (1935) and Galbraith (1936) were the first economists to deal with this explanation of nominal price rigidity (Wolman 2000).

[30] Here Galbraith explicitly referred to Chamberlin in a footnote.

The year 1937 could be considered a turning point. Whereas Shepherd and Galbraith appeared as exceptions, many papers then defined oligopoly not only as a group of a few sellers but as a situation in which competitors had strategic interactions which, because of the recognition of mutual dependence, could result in tacit collusion over price. Two main reasons might explain this trend. The first was the publication of the report *The Economics of the Iron and Steel Industry* by Carrol Daugherty, Melvin de Chazeau and Samuel Stratton. De Chazeau and Stratton both studied the regulation of industries at Harvard in 1930 and respectively wrote dissertations entitled *Some chapters in the regulation of the electric industry in Massachusetts* and *Some chapters on the economic development of the fine steels industry in the United States*. Their report was representative of the appropriation of Chamberlin's concept in order to undertake empirical studies. One major explanation for current price policies in the iron and steel industries was based on the possibility of tacit collusion between oligopolists in the absence of uncertainty.[31] This report fostered several papers.[32] That was why it might have contributed to the spread of the conception of oligopoly and the possibility of tacit collusion inherited from Chamberlin.

The Economics of the Iron and Steel Industry made an impact since it raised a burning practical issue. Not only did the authors define those two industries as oligopolistic on the basis of observed price rigidity. But they also explained that rigidity by the basing point system which allowed collusion among sellers.[33] Such a system was identified by J. M. Clark in 1935 in the *Report of the National Recovery Administration on the operation of the basing point system in the iron and steel industry* (NRA 1935, 60-1). It consisted in price policies whereby two sellers charged the same price regardless of their spatial location, so that consumers paid phantom freight costs. During the whole period considered here, the competitive nature of the BPS was a subject of numerous controversies involving Harvard economists. We shall return to this issue in the fourth section.

A second reason for the spread of Chamberlin's views might have been the running of the American Economic Association Roundtable on monopolistic competition in December 1936. The president of the American Economic Association, Alvin Johnson, suggested that Chamberlin should organise a roundtable whose discussions would focus on "imperfect

[31] The first occurrence of the word "oligopoly" came along with a footnote referring to Chamberlin's idea of the possibility of collusion. All other occurrences explicitly quoted Chamberlin's contribution (Daugherty et al. 1937, 558, 588).
[32] Bain (1937), Bober (1937), de Chazeau (1937), Fetter (1937) and Humphrey (1937). The latter also discussed Means (1935), Galbraith (1936) and Wallace (1937b).
[33] Daugherty et al. (1937, 618, 698-703)

competition". Yet, Aslanbeigui and Oakes (2011, 475-491) show how Chamberlin took that opportunity to defend the specificity of his theory. He put forward the differences between his theory of monopolistic competition and Joan Robinson's theory of imperfect competition (Chamberlin 1937). They also find "evidence that he monitored the progress of Abramovitz's paper at Harvard" (Aslanbeigui and Oakes 2011, 482). In "Monopolistic selling in a changing economy", Abramovitz stated that tacit collusion was a possible outcome of oligopoly, even if neither the sole possible one nor necessarily stable (Abramovitz 1938, 193-195). Among many others, Chamberlin invited A. R. Burns, J. M. Clark, W. Shepherd and F. Machlup to attend the roundtable.

Among papers explicitly using Chamberlin's notions of recognised mutual dependence and tacit collusion in 1937, we identify "Monopolistic Competition and Economic Realism" by John Cassels, who wrote a dissertation entitled *A Study of Milk Prices* at Harvard, Donald Wallace's review of Arthur R. Burns's *The Decline of Competition* (1936) and Paul Sweezy's paper "On the Definition of Monopoly".[34] Sweezy's paper was then used by Fritz Machlup as an introduction to his article "Monopoly and Competition: A Classification of Market Positions" (1937). Although Machlup did not discuss the possibility of tacit collusion here, he proposed a first market classification matching Chamberlin's initial formulation of his oligopoly theory. It is built on two variables: the number of sellers and the differentiation of their products. Machlup was invited by Chamberlin to the 1936 AEA Roundtable, after having spent three months at Harvard in 1933.[35] We know from the archives that he submitted the manuscript of his 1937 paper to Chamberlin to see whether it could be published in the *American Economic Review*. Chamberlin considered it "excellent". He therefore wrote to Dewey, the editor of the review, to assert that Machlup's paper "should by all means be accepted".[36] The confirmation of known connections between the aforementioned economists was also given by a letter from de Chazeau to Chamberlin, dated January 1936:

> Your suggestions se my chapters in the steel industry have just arrived and, although I have not yet had an opportunity to relate them specifically to the manuscript, I know that they will be a great help to me in making final revisions. In

[34] Cassels (1937, 386-8), Wallace (1937b, 383), Sweezy (1937).
[35] In a letter from Machlup to Chamberlin dated 14 April 1934, we learn that Machlup "missed" Chamberlin when he was at Harvard but he told him that he hoped they could "meet again". *Chamberlin Personal Papers* (*CPP*), *Duke University* (NC).
[36] Chamberlin to Dewey, 19 May 1937, *Chamberlin Personal Papers* (*CPP*), *Duke University* (NC). Concerning correspondence between Chamberlin and Machlup: Machlup to Chamberlin, 8 January 1937, Chamberlin to Machlup, 20 January 1937 and Machlup to Chamberlin, 22 April 1937, *CPP, Duke University (NC)*

> fact, I have delayed even re-rereading my stuff until I should have your criticisms, Mason's, Wallace's, and Clark's before me. Now that they are all present, I shall set forth in earnest. (De Chazeau to Chamberlin, January 17th, 1936, *CPP*).

To sum up, between 1933 and 1937, debates about oligopoly, recognised mutual dependence and tacit collusion mainly took place among a specific group of economists composed of Cassels, Chamberlin, De Chazeau, Galbraith, Machlup, Mason, Schumpeter, Stratton and Wallace. The only economist mentioned before who did not have a direct link with Harvard was John Maurice Clark. Yet, as we can read in the previous letter, De Chazeau naturally included him in the group. Those links were not surprising because of Chamberlin's theoretical debt to Clark (1923). The latter was one of the most referenced authors in Chamberlin's 1927 PhD thesis and in successive editions of *The Theory of Monopolistic Competition*. In 1939, Clark sent Chamberlin a draft of his paper on "workable competition".[37] Its publication was a key moment for the emergence of market classifications as a new tool of IO for analysing the nature and consequences of different forms of competition.

3. From Chamberlin's oligopoly to the building of market structure classifications as a new tool in economics (1937–1942)

Market classifications are generally recognised as the primary tool of Industrial Organisation, especially in the Harvard Tradition. The aim of this section is threefold. First, we expound the fact that the building of market classification derives to a large extent from Chamberlin's theory of oligopoly. Second, we show that the diversity of market classifications stems from the variety of approaches developed by members of the group identified. Third, it enables us to provide the rationale behind the different appropriations of Chamberlin's concept. The main protagonists in the development of classifications of market structures are Machlup (1937), Mason (1939), Clark (1940), Triffin (1940) and Bain (1942). As stated before, Machlup built one of the first market classifications based on a "threefold classification of number of sellers (one, few, many) and twofold classification of product difference". This typology aimed at formalising Chamberlin's new insight, with Bain labelling it the "Chamberlinian market classification" (Bain 1948, 159).[38] However, for some members of our group, Machlup's classification proved unsatisfactory both for conducting empirical investigations and for

[37] *CPP, Duke University* (NC). On the institutionalist reception of Chamberlin's book, see Fiorito (2010).
[38] On Chamberlin's influence on market classifications, see also Bain (1964, 30).

making economics a relevant guide to public policies. Edward Mason was particularly representative of this stance.

In the other journal hosted at Harvard, *The Review of Economics and Statistics*, Mason (1938) published a literature review on "Price Inflexibility". Drawing on Galbraith's work (1936) among others, he aimed at identifying the meaning attached to "price flexibility" as well as the differences between statistical and theoretical understandings.[39] Regarding the latter, Mason stated that "price flexibility in the normative sense may be considered a relationship between actual and desirable price behavior". The normative superiority of competitive price theory had generally been thrown out by Harvard economists from the group we identified. Thus, they had to tackle the task of knowing what a good price was, especially because of the recognition of the existence of great corporations and oligopoly markets. For this purpose, Mason argued that economists had to be able to provide "judgement of the probable consequences for relevant economic quantities of type of prices behavior different from the one observed" (Mason 1938, 57).[40]

The task was far from straightforward. Although Chamberlin's assumption of mutual dependence removed the indeterminacy in duopoly, oligopoly did not conform to simple laws – even from a theoretical viewpoint.[41] This was the consequence of opening the door to the strategic dimension of enterprises' behaviour whereas the oligopoly pricing and production policies resulted from numerous variables that had been empirically observed. That was why extensive classifications of market structures appeared as a tool required for passing normative judgment. They had to help to organise the empirical research on American enterprises, to judge the probable consequences of their operations and finally to determine how industries' situations could be improved. On Tuesday 29 December, Mason set about tackling the challenge of building market classifications in the paper he presented at the annual meeting of the American Economic Association. Fritz Machlup, whose classification lacked some crucial variables affecting firms' behaviour according to Mason, was there as a discussant. Published the following year in the *American Economic Review*, "Prices and Production Policies of Large-Scale Enterprise" was regularly regarded as the illustration of what IO originally was.[42] It aimed

[39] Mason mentioned the statistical study of Mills (1927), Means (1935), Burns (1936) and Humphrey (1937). Mason, Mills and Humphrey participated in 1943 in a "Conference on Price Research" organised at the NBER. Stigler and Wallace also took part in it and Mason took care to send the report of the proceedings to Galbraith (JKGPP, Serie 3, Box 7).

[40] See also Mason (1939, 73).

[41] On this matter, see Knight's (1946) communication and Chamberlin's (1946) reply at the AEA session on the "New frontiers in Economic thought" in Cleveland.

[42] On the difference between what Mason's views on IO were and today's IO, see Schmalensee (2012).

at mapping both internal and external factors of the firm that influenced its price and output decisions. However, Mason failed to provide a classification grounded on the variables identified. He acknowledged that he was still working on it with Wallace. His conviction that this tool would provide a useful guide for public policies was, however, firmly asserted since it could inform about the desirability or dangerousness of price flexibility according to the characteristics of each industry. It could also help implement antitrust laws by furnishing "evidence of violations" and then pass judgment on the desirability of public intervention in order to correct the failures of price competition (Mason 1939, 64, 69, 74).

A year later, at the annual meeting of the American Economic Association in Philadelphia, two sessions were devoted to the questions Mason tried to tackle. On Thursday afternoon, 28 December 1939, he chaired a session entitled "Preserving competition versus regulating monopolies" where Wallace (1940) presented a paper. As a discussant, Melvin De Chazeau summed up in a few words the issues that other participants fussed over. How could antitrust law, designed to combat "trust", be adapted to deal with oligopoly situations? To what extent and in which industry was oligopoly "a function of the inherent economic characteristics of production and distribution"? What means should be used so that business price policies moved in "socially desirable directions" (Mason et al. 1940)? The next afternoon, John Maurice Clark participated in a session on "Cost functions and their relation to imperfect competition" and provided the first extensive market classification Mason was asking for. It led to the publication of his famous paper "Toward a Concept of Workable Competition" (1940).

Clark called for a departure from the normative standard of ideal competition since real markets were always "imperfect" or "monopolistic". He sought, like Mason, to ascertain what kinds of market structures were viable and how they could be practicably improved. He identified no fewer than ten variables that were present in the economics literature to finally provide a classification with thirteen cases whose "indebtedness to professor Chamberlin is obvious". But he went beyond his theory of oligopoly. In reality, there were intermediate situations that were "more competitive" than the case of pure "oligopoly" – where few sellers quoted their prices according to the recognition of their mutual dependence so as to reach monopoly profits – but also "more workable" than a situation with open prices degenerating into cut-throat competition.[43] Clark's typology of market structures did not always seem

[43] Clark (1940, 244, 242, 253). If quoted prices are nowadays a standard, many products, not only in agricultural sectors, experienced at that time day-to-day variations.

logically consistent because of the trial procedure used to combine the determining variables. Nonetheless, the tool was gradually taking shape.

The year 1940 was also the year of publication of Robert Triffin's thesis, defended in 1937 and entitled *Monopolistic Competition and General Equilibrium*. Under the supervision of Chamberlin, Triffin proposed a tool that could theoretically identify an oligopoly. It hinged on a rejection of the Marshallian notion of industry that Chamberlin judged inconsistent for grasping the phenomenon of product differentiation. Triffin's goal was to dismiss the partial equilibrium analysis in favour of general equilibrium. But price theory still needed to identify "markets", that is, groups of competitors, without falling into arbitrary criteria of identification. To this extent, Triffin used cross-elasticities. This measurement of a competitor's policy impact on his rivals' prices, quantities or profits helped the analyst to delimit such a group of competitors and to determine its nature. Even better, cross-elasticities could be regarded as an objective measure of the recognition of mutual dependence among oligopolists. In 1941 Triffin emphasised the importance of cross-elasticities in "Monopoly in particular-equilibrium and in general-equilibrium economics". In many contributions of that period, monopoly power was measured according to the elasticity of demand addressed to a firm.[44] Triffin argued that the differentiation of market structures could not be based on that simple analysis of demand elasticity, since it could be similar in monopoly, oligopoly or pure competition depending on circumstances. Moreover, uncertainty about competitors' reactions, which was fundamental in the establishment of an oligopoly situation, was not captured by elasticity of demand.

At first glance, regarding the close ties between Harvard members, one might be surprised to observe that Robert Triffin's work on cross-elasticities was ignored by Mason. In fact, Triffin provided a tool for identifying oligopoly, a necessary step in order to understand their price behaviour and then implement public policies if the former were judged contrary to the public interest. In addition, both Mason and Triffin were looking for tools providing objectivation of oligopoly situations. But Mason's ignorance of cross-elasticities was in reality perfectly understandable. Like Triffin, Mason dismissed the elasticity of firms' demand curves as a relevant tool for identifying monopoly power. But the reason he gave was totally different: the mere fact that economists did not know the slope of demand curves.[45] How could they know

[44] See for instance Lerner (1934) and Sweezy (1937).

[45] Mason (1939, 62, 64). Referring to Cassels' study on milk (1934), Mason acknowledged that the individual demand curve could be approximate if one had empirical data on the variables he identified as relevant to build market structure. Bain explained that the individual demand curve in oligopoly could be determinate only if there was collusion that led to a distribution of the total market shares among sellers (1948, 138).

the value of cross-elasticities? As early as 1939, Mason clearly distinguished two paths in IO. There was firstly "the analytical method", which implicitly referred to Chamberlin and Triffin. It focused "on rivals' reactions as considerations in the determination of price or production policies and on the importance of non-price forms of competition".

We think that Paul Sweezy's famous paper "Demand Under Conditions of Oligopoly" (1939) can be included in this analytical pathway. In that article, he investigated the discontinuity of the oligopolist's marginal revenue curve, discontinuity due to a kink in the demand curve. He explained it by the differences in rivals' expected reactions depending on the upward or downward movement of the price. Like Galbraith (1936), Mason (1938) and many researchers in IO of that time, Sweezy was interested in price inflexibility in an oligopolistic market. However, this paper was not explicitly based on the works of members of our group at Harvard. Sweezy rather followed Kaldor (1934) and his review of Joan Robinson's book *Economics of Imperfect Competition* where he pointed to the lack of analysis of the "duopoly problem". Kaldor did not use the word oligopoly. The expression "duopoly problem" referred to the study of anticipation of the rival's policy. This latter sounded like Chamberlin's "recognised mutual dependence". But it did not necessarily lead to collusive situations (Kaldor 1934, 335).[46]

The second path identified by Mason was the "statistical approach". It began with the empirical investigation of price policies implemented by enterprises and then attempted "to correlate" these variations to those of other economic variables (Mason 1939, 63-5). This was the apex of the methodological divergences, mentioned in the introduction, between the Harvard economists we have identified. It concerned the precedence between theory/deduction and empiricism/induction.

> Some theorists, pursuing their analysis on a high plane, refer to work as 'tool making' rather than 'tool using'. A 'toolmaker', however, who constructs tools which no 'tool user' can use is making a contribution of limited significance. Some knowledge of the use of tools is probably indispensable to their effective fabrication (Mason 1939, 62).

Mason "devoted [his] time to trying to work out classification of markets that would permit an analysis of the relationships between the structure of markets [and] the behavior of the firm".[47] But it was Joe Bain who published a more complete and consistent classification

[46] On the kinked demand curve, see Stigler (1947, p. 438-444).
[47] Letter from Mason to Phillips, 23 February 1973. Cited in Phillips and Stevenson (1974, 336).

than Machlup's and Clark's. Bain wrote his PhD dissertation, entitled *The Value, Depreciation and Replacement of Durable Capital Goods*, under the supervision of none other than Chamberlin, Mason and Schumpeter. Published in the *Quarterly Journal of Economics*, Bain's "Market classifications in Modern Price Theory" appeared to be the direct product of the shared research interests of our group of economists. He began by arguing that Chamberlin's price theory, logical as it was, had two shortcomings: an oversimplification of firms' behaviours and a lack of empirical verification (1942, 561-2).[48] Presenting Machlup's classification, he identified the shortcoming seen by Mason, more specifically that markets differed according to many other characteristics than just the number of sellers and product differences. He added that without other characteristics, Chamberlin's and Machlup's works were disappointing. In fact, as we suggested before, the result of oligopoly is *in fine* dependent on the *ad hoc* assumption regarding sellers' anticipations and strategic interactions.

"Oligopoly [being] an extremely broad category", Bain's market classification aimed at shedding light on different patterns of price policies. The implicit idea was that adding other objective variables might enable economists to observe regularities and further their understanding of strategic interactions without adding psychological assumptions (Bain 1942). Bain's classification hinged on six variables: the number of sellers, the durability of the product, the differentiation of the product, the variation of the product, the number of buyers and the nature of the buyers. Each of these criteria was twofold, so that he obtained a classification with 64 cases. Those criteria had already been mentioned in Mason (1939) as relevant to the purpose. Bain explained that his work had been fostered by his participation in the Harvard seminar on IO and price policy. From this exhaustive classification presented as a hypothesis that should be submitted to empirical verification, he proposed a shorter one reduced to 14 cases. He excluded those which had no sufficient counterpart in the real world "to justify generalization" (Bain 1942, 566-573).

To sum up, we see that in less than a decade, the publication of Chamberlin's oligopoly theory irradiated the work of a group of economists around Harvard. It gradually led to research institutionalised by the American Economic Association in 1941 under the label "Industrial Organisation". The successive appropriations of Chamberlin's work gradually led to the construction of a new tool for economists. It might be argued that accounting for the steps of

[48] But he praised Chamberlin's theory compared with those of Robinson (1933) and Hicks (1939) for "greater empirical content", since Chamberlin recognized that a difference in number of sellers and a difference in the degree of product differentiation between markets would lead to different prices (Bain 1942, 565).

construction of market classifications drove out the notions of mutual dependence and tacit collusion in oligopoly. However, it did not. Oligopoly turned out to be the third main theoretical market situation with monopoly and pure competition. In addition, the distinction between oligopoly markets according to numerous variables enabled those economists to challenge the idea of a perfect continuum of market situations. It helped them to think in terms of a finite number of situations between the two pure cases of monopoly and competition. If it is remembered that market classifications aimed at improving economists' judgments about the consequence of price policies and antitrust policies, so as to enlighten public interventions, the issue of the collusive nature of oligopoly emerged as a burning one. The topic of antitrust policies obviously opened up an endless literature. But the identification of a specific group of economists during the first two phases has enabled us to focus on a specific controversy around the basing point system. Studying it provides the rationale behind the general scepticism about antitrust policy that characterised the Harvard Tradition in IO.

4. Tacit collusion, the basing point system and antitrust laws

The full history of the Basing Point System (BPS) extends well beyond the scope of this paper.[49] Already in the 1920s, Frank Fetter (Princeton), John Commons (Wisconsin) and William Ripley (Harvard) addressed it in a report for The Federal Trade Commission. It was echoed in Fetter's publication of *The Masquerade of Monopoly* (1931) in which he unconditionally condemned the BPS practice and lamented the decreasingly competitive nature of American industries. Even if it could be refined, the logic of price policies under the BPS was simple to understand. It was based on a geographical distribution of the few competitors in a market. Among them, a productive unit emerged as a reference, because, say, of its greater efficiency. This reference unit charged its price for the whole territory, with each buyer paying this price plus the transport cost. If another productive unit could supply the same places as the reference unit, it would charge the exact same price since there was no incentive to charge less. Indeed, its assumed limited productive capacity did not enable it to entirely supply this demand and the opportunity of being closer generated an incentive to mimic the rival's price.[50] As soon as a firm was identified as a *basing point*, other firms could understand the benefit of following

[49] For more details, see Stocking (1950). For an analysis of the stance by Fetter and Clark in the public controversies, see Dumez and Jeunemaître (2001, 1282-1289).

[50] For instance, a firm based in New York produces an article whose f.o.b. price is $20. Because of transport costs, this article is offered at $22 in Boston, $24 in Chicago and $30 in Los Angeles. Another producer based in Salt Lake City might charge $30 in Los Angeles even if its production cost is $22 and the transport cost $6 per unit ($28). The opportunistic profit of $2 is called *freight absorption*.

this price instead of implementing aggressive pricing that would end in cut-throat price competition.

We do not defend here the idea of a direct relationship between Chamberlinian oligopoly and the BPS controversy. The spatial differentiation in the latter was, in itself, very different from the assumed homogeneity by Chamberlin. But it is still interesting to report a correspondence between him and Harold Hotelling about his famous article "Stability in Competition" (1929). Chamberlin already had in mind the instability of a spatial duopoly in the case when one of the competitors cannot supply the whole demand. The perfect inelasticity assumed by Hotelling allowed the other seller to propose any price for the unsatisfied demand, encouraging the first one to follow this rise (Hotelling 1929, 45).[51] In addition, the existence of the BPS raised a question that could be addressed from Chamberlin's concepts. Was this system grounded on *explicit* agreements between sellers or on *tacit* collusion? On the one hand, explicit agreement was forbidden, and the existence of evidence of collusion made law enforcement possible. On the other hand, if the BPS was the product of tacit collusion, then enforcement was more problematic.

In 1935 the National Recovery Administration, through the voice of J. M. Clark, accused this pricing method of being collusive (NRA 1935, 60-1).[52] As early as 1923 in *The Economics of Overhead Costs*, Clark had underlined the increasing propensity of competitors to avoid price competition (Clark 1923, 417). De Chazeau, Daugherty and Stratton took Clark's overhead costs as a theoretical source for their empirical analysis of the BPS in the iron and steel industry. Like Clark, they considered the industry to be collusive. But a difference was the use they made of Chamberlin's oligopoly which allowed them to explain competitors' behaviour. They argued that the BPS was possible because of the recognition of mutual dependence. In addition, they did not defend the interruption of the BPS for two main reasons. First, the oligopolistic nature of the industry was explained by technological characteristics of the productive process. It was consequently vain to believe that public policy could restore "competition" (atomicity). Second, the ending of this practice would have caused heavy losses because of high investments already

[51] "Now I have worked it over more carefully and have reached a conclusion which, if true, is damaging to what I understand to be your central thesis – that stability is assured by the fact that 'the quantity sold by each is considered as a continuous function of the differences in price' (p. 44). It seems to me rather that it is assured by the fact that the supplies of the sellers are unlimited. […] Since you have assumed absolute inelasticity of demand, it would seem that the solution would be infinity for both p_1 and p_2, q_1 and q_2 being indeterminate, if the demand were taken to be elastic, the upper limit would be finite, and it would pay A to set his price at same point lower than B (instead of at infinity)." (Chamberlin to Hotelling, 25 July 1930, *Chamberlin Personal Papers*, Duke University).

[52] In its report, the National Recovery Administration recommended a "mill-base system" instead of the BPS. It consisted in the obligation for sellers to quote their prices f.o.b. or to impose a uniform price for all of them. This is what Fetter was to call the "mill-base rule" (Fetter 1937).

made in the industry and whose profitability relied on such a price system (Daugherty et al. 1937, 533, 558, 547).

In a comment on the report titled "The New Plea for Basing-Point Monopoly", Frank Fetter expressed his scepticism about the analysis in *The Economics of the Iron and Steel Industry*. Whereas the authors of the report asserted that, under oligopolistic conditions, the emergence of a BPS depends "neither on tacit understandings nor on collusive agreement",[53] Fetter was not convinced. There had to be explicit agreements behind such a pricing system, which Fetter condemned since prices in the BPS were generally assumed to be above the "competitive prices" taken as a normative benchmark (Fetter 1937, 585, 579). This controversy progressively developed into a discussion of the benefits and shortcomings of some alternative pricing system that could be more desirable than the BPS (De Chazeau 1938; Fetter 1938; Clark 1943; Mund 1942, 1943).

Partly due to the role played by Chamberlin's concept of tacit collusion in oligopoly, many participants in our Harvard group took part in the BPS controversies. In 1942, Arthur Smithies, another former student then professor at Harvard, added his brick to the wall.[54] As he recalled in a footnote, his article "Aspects of the Basing Point System" was corrected by Fritz Machlup and Isaiah L. Sharfman, a former professor of Chamberlin's.[55] Smithies confirmed De Chazeau and Clark's point of view against Fetter and Mund. But he wished to provide more details about price determination in the BPS. Like Chamberlin before him, Smithies studied the behaviour of two identical competitors and reached the same conclusion: there was a possibility of tacit collusion. If such a cooperative solution were reached, there was indeed no incentive for either competitor to change their policy. But asymmetry between producers might increase opportunities to depart from collusion.

In 1948, the Supreme Court concluded that quoting the delivered price was illegal, putting an end to the BPS practice. The decision was praised by Fetter (1948) and brought new impetus to this controversy. Machlup (1949) devoted an entire book to the subject: *The Basing-Point System: An Economic Analysis of a Controversial Pricing Practice.* Another latecomer to our group, Carl Kaysen, published three papers and a review of Machlup's in the *Quarterly Journal of Economics* and the *Review of Economics and Statistics*.[56] His "Basing Point Pricing and

[53] Daugherty et al. (1937, 572).
[54] He defended his PhD thesis, "Theory of Production", in 1934 under the supervision of Schumpeter.
[55] On the link between Chamberlin and Sharfman, see Chamberlin (1961) and Guicherd (2017).
[56] Kaysen (1949a, 1949b, 1950, 1951). During World War Two, he was hired in OSS by Mason in 1942, who convinced him to go to Harvard University. He received his M.A. in 1947 and defended his PhD in 1954 on anti-trust laws.

Public Policy" came from a set of discussions on antitrust laws at Harvard. After expounding the main characteristics of the BPS analysis, he confirmed the existence of collusive behaviour and rooted it in an "oligopolistic rationality" (Kaysen 1949a, 289, 291, 294). The idea behind the expression was the same as Galbraith had put forward a decade earlier (1936). Because of recognition of mutual dependence, a seller in an oligopoly could have a preference for price rigidity. It reduced uncertainty regarding the behaviour of others.

From these debates there emerged a first line of cleavage between what George Stocking labelled the "conspiracy school" and the "spontaneous evolution" school (1950). Members of the "conspiracy school" were primarily Fetter, Machlup and Mund.[57] Since the BPS was seen as the product of explicit agreement and led to the waste that characterised monopolists' practices, it was considered to be illegal and had to be countered with the help of antitrust laws. On the contrary, members of the "spontaneous evolution" school thought the BPS could result from independent decision-making by sellers. The members of our group around Harvard mostly belonged to this latter. Stocking clearly showed that this divide could be viewed as two different interpretations of Chamberlin's theoretical contribution.

> Chamberlin, primarily a theorist, gave a powerful weapon to policy-makers. Unfortunately, it has proven a double-edge sword, wielded lustily both by those who favor and those who fear Big Business. Opponents of Big Business have argued that since oligopolists behave like monopolists and since oligopoly is inevitable, the government must regulate Business in the public interest. Proponents have used the Chamberlinian doctrine as a defense in anti-trust proceedings (Stocking 1950, 162).[58]

Acknowledging that the oligopoly market could result in a monopoly led some economists to praise the use of antitrust legislation and its adaptation to the oligopoly problem. But the potential tacit nature of collusion and the recognition of a private rationality behind such behaviour of sellers in oligopoly did not necessarily lead to the idea that any public intervention was to be proscribed. It rekindled the pleas for a social control of business. The tacit nature of collusion, the spontaneous nature of oligopoly markets and the practical difficulty of its implementation had, however, often reduced the confidence in antitrust legislation among

[57] See also Stigler's presentation of each side (1949). Dealing with the emergence of a BPS, Stigler did not tackle the possibility of a tacit oligopoly (1949), nor was it explored in his famous article "A Theory of Oligopoly" in which he analysed the stability of an explicit collusion (1964). In the 1950s, Stigler seemed to be an inheritor of this latter school as the leading figure of the Chicago Tradition in IO identified by Posner (1978).
[58] See also Stocking (1953, 439).

members of our group.[59] For instance, Mason advocated an application of antitrust legislation according to "rules of reason" rather than "rules *per se*" (1956). Clark argued that collusion was neither harmful nor good *per se* (1943, 296). Economists needed to go on using the tools they had built to carry out empirical investigations on the merits and shortcomings of the different branches of the industrial system.[60] By doing so, they stimulated works of a third kind. They attempted to reappraise the very nature of mid-century American Capitalism, since this latter appeared to have drifted far from its founding myth of free competition.

5. The nature of post-war American Capitalism

The spreading of Chamberlin's theory, the building of market classifications and the debates around the BPS and antitrust laws pervaded the works of almost all the members of Harvard's Department of Economics. In 1948 the American Economic Association sponsored the publication of a *Survey of Contemporary Economics*. Two members of our group, Galbraith and Bain, wrote chapters on "Monopoly and the Concentration of Economic Power" and "Prices and Production Policies", respectively. Both shared the conviction that oligopoly was then the dominant form of competition in American industry.[61] Both papers attested that, whatever the authors' assumptions regarding the causes leading to the emergence of oligopoly and the nature of collusion, all the protagonists of the emergence of IO at Harvard had discussed the pros and cons of their operations. Whereas theoretical and empirical investigations usually focused on specific industries, some economists in the group had attempted to put oligopoly theory into a systemic analysis of capitalism, providing a reappraisal of the nature of the entire American economy. Since oligopoly was the new distinctive characteristic and the new driving force in the economy, the dynamic of capitalism was changing. As a primary observer of the effect of Chamberlin's influence at Harvard, Joseph Schumpeter considered such a prevalence of the oligopolistic and monopolistic nature of competition and tried to include it in a systemic analysis of the dynamics of capitalism.

When he arrived at Harvard in 1932, Schumpeter was already an accomplished economist, with his own research agenda on the Business Cycle. But as early as December 1933, he headed a session at the meeting of the American Economic Association on "imperfect competition" in

[59] See Mason (1956), Schumpeter (1942), Kaysen (1949a), Galbraith (1948).
[60] These investigations conducted through the "industrial section" of the National Resources Planning Board (1939, 1940) headed by Gardiner Means and in the numerous TNEC monographs. For a survey of these empirical studies, see Bain (1948).
[61] Galbraith (1948, 127) and Bain (1948, 136). Stigler's put forward their convergence in his review of the survey (1949, 95-6).

which Chamberlin took part. The following year, Schumpeter published a paper with A. Nichol linking the question of oligopoly to the elasticity of demand (1934). At Cambridge, he was also close to Mason and Chamberlin (Mason and Lamont 1982). In his *History of the Economic Analysis*, he argued that Chamberlin's theory deserved its success. However, his first book published at Harvard, *Business Cycle*, did not show obvious marks of any influence (Schumpeter 1939). Chamberlin himself noted that the book still presented the "basic defense" of "perfect competition as the theoretical norm" (1951a, 137). Had Schumpeter's presence influenced the early thoughts of the other members of our group? Schumpeter's disdain for original institutional economics, the New Deal, and the instrumental stance of American economics were notorious divergences. His *Theory of Economic Development* was, however, translated in 1934. In a letter Mason sent to Galbraith in 1977, he recalled that this book had the merit, "in the middle of the Marshallian era with this complete concern for static analysis", of emphasising dynamic analysis, though it concentrated on the entrepreneur who progressively disappeared because of the rise of Big Business.[62] This exchange originated from a review by Galbraith. Galbraith's judgment on the obsolescence of *The Theory of Economic Development* aimed at contrasting with the relevance he found in *Capitalism, Socialism and Democracy* (1942).[63]

In this book, Schumpeter articulated the theory of monopolistic competition – oligopoly and product differentiation – with his analysis of the historical development of capitalism. Even if the scope of the book went beyond the reappraisal of competition in the line of Chamberlin, the theory of monopolistic competition had a crucial place. It directly preceded the famous chapter "On the process of creative destruction" (Schumpeter 1942, 79).[64] Schumpeter argued that "as soon as the prevalence of monopolistic competition or of oligopoly or of a combination of the two is recognized", the result of price theory in the "schema of perfect competition" was "either inapplicable or much more difficult to prove".[65] As we have seen and if perfect competition were taken as a normative benchmark, Chamberlin's theory could have led to the condemnation of oligopolists since they potentially behaved as monopolists. However, Schumpeter argued that the reality of the economic system had to be examined from a dynamic

[62] Letter of 27 March 1977. John Kenneth Galbraith Personal Papers (JKGPP), JFK Library, Series 9, Box 947. See also Mason (1951, 143).
[63] Galbraith to Mason, letter of 7 April 1977. JKGPP, JFK Library, Series 9, Box 947. Galbraith's review of *Capitalism, Socialism and Democracy* appears in *New Society*, 14 April 1977. Galbraith wrote a highly critical review of Schumpeter's *History* for *The Reporter*, 17 August 1954. JKGPP, JFK Library, Series 9, Box 940.
[64] Even Schumpeter's theory of democracy proposed in the final part of the book is influenced by the economics of monopolistic competition, especially his treatment of the effect of advertising.
[65] The recognition of a taste for diversity that characterised the monopolistic competition theory also made the result of welfare economics more complex. See Galbraith (1938) and Chamberlin (1950).

and systemic perspective. Yet the increasing concentration of industry had not led to output restrictions since the beginning of the century. The classical result that monopoly or oligopoly led to capitalist sabotage had to be proved. Moreover, some restriction in the short run could be required for long-run expansion (Schumpeter 1942, 78-88, 52).

Like other members of our group arguing that the emergence of oligopoly resulted from a "spontaneous evolution", especially due to technological requirements, Schumpeter saw growing concentration as a normal result of the competitive process. He claimed that this process was not a "necessary evil". It had proved very successful from an economic viewpoint.[66] He put forward four main arguments. First, this capitalism of great units had proved to be more stable than nineteenth-century capitalism. More precisely, "tacit understandings about price competition may be effective remedies under conditions of depression" (Schumpeter 1942, 91). Second, because of specialisation and rationalisation, great corporations were often more effective than small ones. He dismissed the idea of a productive superiority of perfect competition that hinged on the dubious assumption that firms, whatever their size, had the same method of production. Third, great corporations were those that generated innovations and fostered the technological development accounting for rising living standards. Fourth, monopoly positions were always temporary and not absolute, because of the creative and destructive nature of the technological process and potential competition (1942, 87-106).

Schumpeter's plea for capitalism through the defence of Big Business achievements did not pass unnoticed at Harvard. Carl Kaysen put it forward in the debate over the BPS to argue that the higher margins provided by this price system "might then act to stimulate a more rapid increase in capacity" (1949a, 302). Even if Schumpeter used to work in the framework of perfect competition, Chamberlin highlighted how his colleague saw the "overwhelming importance" of product heterogeneity and oligopoly (1951a, 138). Edward Mason bemoaned the lack of operationality of Schumpeter's theory of competition. It could hardly be a guide to implementing public policy. But he nonetheless praised his "drastic and effective" critique of antitrust policy, since Schumpeter also contested the idea that "the exclusion of market power" would necessarily "assure the efficient use of resources" (Mason 1951, 139). Finally, John Kenneth Galbraith argued in *American Capitalism* that, while his analysis belonged to another

[66] If Schumpeter predicted the disappearance of capitalism, it must be recalled that it was not due to its economic failures. On this issue and its link with debates on secular stagnation at Harvard, see Dockès (2015), Potier (2015) and Dal-Pont Legrand and Hagemann (2017).

tradition of economics than Schumpeter's, he reached "similar conclusions" regarding the role played by oligopoly for inducing technical change (1952a, 86-8).

As stated before, Galbraith's *American Capitalism* could be read as a syncretism of the writings of the authors we have identified, since he explicitly referred to the analysis of Bain, Chamberlin, Clark, Mason, Machlup and Schumpeter. Understanding Galbraith's main thesis implied presenting one last reappropriation of Chamberlin's oligopoly theory. This latter could not only be applied to the buyer's side of the goods market (oligopsony), but also to one very specific market, namely the labour market.[67] This was particularly visible in the report *The Impact of the Unio*n, which followed two days of meetings organised by the Institute on the Structure of the Labor Market. Among the well-known participants were two members of our group, Clark and Chamberlin.[68] As for price policies, Clark clearly distinguished the theoretical and the normative issues: "What determines wages?" and "What wages are economically sound?" (1951, 1). In his paper "Monopoly power of labor", Chamberlin applied the same tool he had used to analyse the product market. This led him to an analysis in terms of "bilateral monopoly" of the labour market, where unions and oligopoly competed for the appropriation of monopoly profits (1951b, 179). If debates turned on the real importance of unions' power and the consequences of their claims regarding unemployment and inflation, all participants, save Milton Friedman, recognised their growing power. This interaction between all kinds of oligopoly characteristic of the American economy was precisely the subject of Galbraith's book.

As in 1936, Galbraith put forward a dual view of the American system. If one side operated much as classical competitive markets, another side was dominated by a few firms. Like his Harvard colleagues, he argued that oligopolistic situations emerged because of technological requirements. Great firms operating in these markets tended to avoid price competition and antitrust laws were not necessarily efficient and even relevant to fighting oligopoly. Following Clark and criticising the old liberal anti-monopoly tradition, he argued that the consequences of this monopoly power were not as bad as suggested by classical price theory. Following Schumpeter, he put forward the efficiency of these big corporations and the transitory dimension of monopoly power, since "to maintain a convention against innovation requires a remarkably comprehensive form of collusion" (1952a, 89). But he went further by arguing that

[67] As early as 1942, the Harvard economist John Dunlop analysed the situation of labour by combining classifications of product markets and labour markets. Dunlop (1942) and Dunlop and Higgins (1942).
[68] The others were Haberler, Knight, Boulding, Friedman, Samuelson and McCord Wright.

the private monopoly power of oligopoly automatically tended to give rise to a "countervailing power" on the other side of the market.

The power of buyers – that is, of firms, consumer cooperatives, unions or the State – countervailed the power of the sellers. It led to some equilibrium, as in classical the model, but by another mechanism. Galbraith thought this mechanism of power and countervailing power accounted for the success of the American system in fostering opulence. It made it possible to benefit from the productive capacity of great corporations without suffering all the inconveniences put forward by the economists who took pure competition as their benchmark. He gave several examples. Referring to Machlup's work on the BPS (1949), he depicted how the power of the large firms of the automobile industry in Detroit countervailed the power of the steel producers (1952a, 123). He also depicted the unions, which were generally strong in concentrated industries, as a countervailing power to big employers. In this respect, like Chamberlin (1951b), he thought that unions and management of great corporations mostly quarrelled over the distribution of these monopoly profits and pointed out the risk of oligopoly markets generating an inflationary process under conditions of excessive demand.[69]

Even if he recognised the role of the state in helping the emergence of a countervailing power, Galbraith presented it as a "self-generating force" that had replaced "competition" (1952a, 113). Some colleagues pointed out that a countervailing power could indeed be seen as a form of competitive behaviour. Galbraith also dismissed the concept of "bilateral monopoly" used by Chamberlin and others. He argued that it led to some incomprehension. In a letter dated 18 March 1954, John Maurice Clark told him that he "couldn't accept" his view of "the completely passive role of buyers under competition". He added that the concept of countervailing power should precisely lead to reinvestigating the "sadly neglected concept of bargaining power", since "it is neither competition nor monopoly" that operated but "a variety". This was why he questioned him about the reason why he asserted in his "provocative book" that "bilateral monopoly is a blind alley". In his reply on 25 March, Galbraith gave a straight answer:

> My reference to bilateral monopoly's being a blind alley for economists grows out of the fact that it was considered in the past to be accidental or adventitious rather than self-generating. So long as economists assume that bilateral monopoly occurred when a position of strength on one side of the market 'happened' to be opposed by another position of

[69] See also Galbraith (1952b) and Clark (1951).

> strength across the market it was hardly worth worrying about. My argument of course, is that a position of strength tends to beget a protective answering position. To the extent that this leads to bilateral monopoly or bilateral oligopoly the latter is not adventitious, but like the tendency toward competition (or monopoly) an organic phenomenon (Galbraith to Clark).[70]

Galbraith's thesis about the organic nature of oligopoly in modern capitalism unleashed controversies. The same authors who were opposed to the members of our group on the BPS issue were those who wrote the more critical reviews.[71] Recognising some quality to the book, Vernon Mund thought that Galbraith "fails to recognize the injury to consumers which results from the collusive action of labor groups and organized producers" and minimised the importance of concentration that did not respond to technological requirements but rather to financial interests (Mund 1952, 576). In a session of the annual meeting of the American Economic Association devoted to his concept of countervailing power, George Stigler went much further. In addition to some technical details regarding the conditions when the countervailing power operated and some counterexamples, he reproached Galbraith for providing a "dogma" rather than a "theory". He considered it too difficult to validate Galbraith's hypothesis that "bilateral oligopoly generally leads to socially tolerable results" (Stigler 1954, 10, 13). In a letter to Stigler before the meeting, Galbraith tried to clarify his normative stance, that is to say, what he meant by "tolerable result". He explained that his "value criteria involve minimization of social tensions rather than maximization of consumer real income".[72]

As this reconstruction of the emergence of IO at Harvard comes to an end, it brings us back to the starting point. It was Edward Chamberlin who chaired the session on "Countervailing Power" at which Galbraith claimed that economists owed him "a debt" for having shown that "economic power in the economy is pervasive" and "goes far beyond the limits set by the classical concepts of monopoly" (Galbraith 1954, 6). Whereas Edward Mason's role in the emergence of IO was always acknowledged, the impact of Chamberlin's theory on the Harvard Tradition in IO received far less recognition. Yet, none other than Joe Bain argued that it was crucial. "The *Theory of Monopolistic Competition*, by enunciating in express form a novel theoretical construct and implementing it to provide the essentials of an empirically relevant

[70] JKGPP, JFK Library, Series 3, Box 9.
[71] Adolph Berle and Joan Robinson praised the book.
[72] Letter from Galbraith to Stigler, 24 November 1953. JKGPP, JFK Library, Series 3, Box 55. See also Galbraith (1954, 2-3). Galbraith's criticism of neoclassical welfare economics dated back to Galbraith (1938) and continued in his trilogy (Chirat 2020).

and sophisticated theory of markets, was *the* major contribution to price theory which set in motion the systematic development of the modern field in industrial organization" (Bain 1964, 32).

6. Conclusion

This paper has traced the emergence of a group of economists at Harvard from the 1930s to the early 1950s. The main difference with the existing literature lies in the identification of concepts uniting actors whose intellectual ties are traditionally overlooked because of their political divergences or their different conceptions of the nature of economics. We believe that, unless importance is attached to "recognised mutual dependence" and "tacit collusion" in oligopoly theory, one misses a part of the story of the emergence of IO at Harvard and the various pathways by which it developed. The spreading, reformulations and reappropriations of these notions are also crucial in accounting for the close links between monopolistic competition theory, Industrial Organisation and public interventions by members of Harvard. We have reappraised the impact of Chamberlin's book on his colleagues. Lastly, the paper provides a detailed account of the emergence of the "Harvard Tradition" before the SCP paradigm became dominant at Harvard. We do not claim that the identification of these ideas of "recognised mutual dependence" and "tacit collusion" are an answer in themselves to all the theoretical and practical motives for Harvard economists' participation in the emergence of IO. Moreover, we have said nothing about the spread of the notion outside Harvard and the exchanges with economists from Cambridge (UK) and the controversies around the full costs approach.[73] We have also put aside the reception of the works of our group of authors by other American economists.

In this respect, the late 1940s marked the beginning of a turning point in IO history. The spreading of game theory seems to challenge the methodological relevance of Harvard's market structure analysis. Kurt Rothschild (1947) began his paper "Price Theory and Oligopoly" as a rejoinder to the idea of recognised mutual dependence among a few sellers. Like Bain, he criticised price theories built on given and exogenous conditions of demand and supply. However, he regretted that many IO contributors did not further examine competitors'

[73] Two important articles are R. F. Harrod's "Price and Cost in Entrepreneur's Policy" and R. L. Hall and C. J. Hitch's "Price Theory and Business Behaviour", both published in *Oxford Economic Papers* in 1939. Besides, Hall and Hitch not only quoted Chamberlin and recognised him as a pioneer in enquiries into oligopoly, but also used a Chamberlinian classification of markets (Hall and Hitch 1939, 14). Exchanges between the two Cambridge scholars on these issues are, however, another story, even if important. For instance, Galbraith's (1936) early contribution referred to Chamberlin (1929, 1933) but also to both Robinson (1933) and Kaldor (1934).

propensity to modify market structures according to their possible interactions and behaviours. Consequently, he proposed to turn to pure strategic approaches such as the one proposed by John Von Neuman and Oskar Morgenstern in the *Theory of Games and Economic Behaviour* (1944). As game theory progressively became the core of industrial organisation, it seems appropriate to conclude this paper by briefly setting out how some members of the Harvard tradition reacted to its early dissemination.

In 1948 Oskar Morgenstern studied links between imperfect competition, oligopoly and monopoly on one side and game theory on the other. He made several assertions, establishing an implicit continuity with Rothschild's paper. He proposed abandoning different market approaches and pleaded "to know how the individual, pursuing his maximum interest, *should* behave on *all types of market*" (Morgenstern 1948, 11).[74] He was in search of a general normative theory, asserting that "economic theory must therefore indicate how the firm or the individual should behave under all conceivable conditions". He spoke in favour of the Robinson Crusoe duopolistic thinking, in other words the abandonment of the empirical basis for theoretical purposes (1948, 13). Morgenstern's paper was followed by a discussion by William Jaffé who rejected all preceding duopoly and oligopoly theories. He lamented their failure to find a definite solution. Nevertheless, he also voiced scepticism about Morgenstern's ability to provide a solution (Jaffé et al. 1948, 19).[75]

Some members of our group also expressed their opinions regarding the emergence of game theory as a new tool for the study of oligopoly and industrial organisation. First, Joe Bain suggested that this "well-developed and new theoretical system" could influence IO in a way that "will be interesting to observe", since it provided "a new approach to the formation of business decisions under conditions of recognized mutual independence" (1948, 162). In his course on "Business Organization and Public Regulation", which he taught at Harvard in the academic year 1951–1952, Galbraith presented von Neuman and Morgenstern's book as a separate attempt from Bain's, but sharing the same aim of building a general theory of oligopoly.[76] Lastly, Carl Kaysen was the member of the Harvard Tradition in IO who paid most attention to *Game Theory of Economic Behavior*. He wrote an extensive review in the *Review*

[74] We highlight the normative stance of Morgenstern's assertion.
[75] "Perhaps the nature of duopolistic behavior is not amenable to systematic analysis, but I doubt that. At best the theory of games or war can only be part of such analysis." (Jaffé et al. 1948 21). In a similar way, Martin Bronfenbrenner accused the theory of imperfect competition (or monopolistic competition) of dividing price theory instead of looking for a unifying principle. He recognized Morgenstern's merit but was cautious about its capacity to put an end to this problem.
[76] JKGPP, Series 5, Box 519.

of Economics and Statistics asking whether game theory would bring about "A Revolution in Economic Theory?"

The first aim of Kaysen's review was to present a tool which was new to the vast majority of the profession. After highlighting the relevance of von Neuman and Morgenstern's framework for dealing with two-person zero-sum games, Kaysen explained why the study of three- or *n*-person games had to become "a study of coalitions" to avoid indeterminacy. He illustrated his former explanation with economic applications such as bilateral monopoly (two-person games) or the case where two buyers faced a single seller (three-person games). He then concluded on the case of a duopoly, which originally fostered the analysis of oligopoly. He argued that "traditional theory" in economics, like game theory, provided no solution to the duopoly problem which did "not depend on the use of arbitrary elements". In other words, to reach a solution, economists were forced to postulate the reaction function of one of the rivals. He thought that "the study of actual markets" undertaken by Harvard IO economists reduced the "arbitrariness" of the assumption regarding the reaction function of rivals. He thus wondered whether game theory, thanks to the incorporation of these facts described by market structure analysis, could also provide a "solution" that could be "free of indeterminacy". In other words, he suggested that rather than being supplanted by game theory, market classification and empirical study that characterised the Harvard tradition in IO might help identify some pattern in the formation of market coalitions (Kaysen 1946, 14-5).

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