June 2012

"On a level with dentists"? Reflections on the Evolution of Industrial Organization

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1. Introduction

In 1930, writing about "Economic Possibilities for our Grandchildren," Keynes (1930, p. 373) opined that "If economists could manage to get themselves thought of as humble, competent people on a level with dentists, that would be splendid!" In this essay I offer some observations on the evolution of industrial organization economics and consider at the end whether its practitioners have come to deserve to pass Keynes' test.

I use the word "evolution" rather than "progress" or "development" deliberately. Just as predator and prey evolve together in natural systems, and evolution there is about adaptation, not species-by-species improvement, so problems addressed and tools employed evolve together in intellectual fields. In fields of economics, methods of analysis are shaped over time both by developments in related fields – mutations, if you will – and in response to the changing problems with which the field is concerned. Those problems are similarly shaped both by events that are outside academia (often, but not always, in the public policy arena), by the capabilities of the available analytical methods, and by developments that are internal to the field. Problems that resist solution with the tools available are sometimes set aside, as are tools not suitable for the problems *du jour*. Often, new learning undoes old certainties. There is progress over time in industrial organization, as in other scientific fields, but the uphill path is not always straight.

Industrial organization is a young field, young enough so that a short biography/history of thought can potentially do it reasonable justice. I am only an intellectual grandson (via Morris Adelman) of Edward Mason, who is widely considered the father of industrial organization. Despite this field's youth, I hope this essay demonstrates that enough has happened since its emergence to make its biography at least mildly interesting.

* This paper is based on my keynote address at the International Industrial Organization Conference, March 17, 2012. I am indebted to Christopher Knittel, Jonathan Levin, Ariel Pakes, Nancy Rose, Stephen Ryan, and the editor, Lawrence White for extremely valuable comments and conversations, but I of course retain responsibility for all errors and opinions.

In what follows I somewhat artificially divide the literature of industrial organization into work of a few distinct styles and loosely associate those styles with particular time periods during which they were especially important, recognizing that at any time several styles of work were usually being done. I do not aim for bibliographic completeness and can only offer a blanket apology to the many whose important work I have not had the good sense or good taste to cite. Nor do I attempt to cover all of the problems that have been worked on by industrial organization economists or all of the theoretical and empirical tools that they have employed. Much good and important work on the determinants of innovation, productivity, firm organization, market structure, regulatory behavior, and other issues is thus ignored. My focus is on a central concern of the field since its emergence: What are the implications of departures of real markets from the perfectly competitive ideal for the conduct of sellers—in particular the nature and intensity of competition among them—and thus for the performance of markets?

2. In the Beginning

The term “industrial organization” may have originated with Alfred Marshall, who was a keen observer of real markets as well as an important theorist. Five chapters in his *Economics of Industry* (1899) have the phrase “industrial organization” in their titles. In those chapters, Marshall’s main focus was on how in competitive markets economies and diseconomies of various sorts, along with the advantages and disadvantages of various legal forms, shape what we would now call seller concentration. At the start of the 1930s, E.A.G. Robinson (1931) adopted exactly the same focus in his classic *The Structure of Competitive Industry*. While research on the determinants of concentration continues, it has not been the field’s central concern since the 1930s. Nor does research in “industrial organization” focus exclusively on industrial markets. Thus, “industrial organization,” despite its long history, is probably not the best name for the field to which it is attached.[1] To avoid that name and in the interest of brevity, I will instead generally use “IO” in what follows.

[1] For several years I used the English term “industrial economics,” which seems marginally more descriptive, but feeling lonely in this regard I gave up long ago. Mason (1957) often used “business organization,“ which is a slight improvement. As a logical matter “business economics” is much superior, but it sounds too much like a basic MBA course. “Enterprise economics”? Perhaps the Industrial Organization Society should award a prize for the best new name and promise to rename itself accordingly…

In his description of the emergence of the broader field of IO, Mason (1957, pp. 1-3) observed that in the 1920s, economists were confident that most of the economy could be analyzed using the competitive model, with monopoly "a distinct but less important phenomenon, and duopoly (or oligopoly) a textbook curiosity." He argued that this changed in the early 1930s, at least for some economists, with the appearance of Chamberlin's *Theory of Monopolistic Competition* (1933), Robinson's *The Economics of Imperfect Competition* (1933), and Berle and Means' *The Modern Corporation and Private Property* (1932).[2]

Some contemporary scholars reacted to all of this by attempting "to rescue the competitive model and to refurbish it for modern usage" (Mason 1957, p. 2). Others, to whom the "Harvard School" label is commonly attached, followed Mason and came to believe that the economy contained markets that Marshall's tools could not handle:

> Whatever the magnitude of the industrial segment adapted to a competitive analysis, that part that is not so adapted cannot, in the main, be usefully analyzed as monopoly. The Marshallian dichotomy between competition and monopoly is irretrievably broken (Mason 1957, p. 5).

The new theories did not much advance understanding of those markets; Mason found that the work of Robinson and Chamberlin "has provided a beautifully logical and consistent set of propositions without direct operational applicability" (Mason 1957, p. 4). Despite Mason's assessment, of course, the monopoly model that Robinson presented has become arguably the most important theoretical tool in IO.

A good deal of statistical work in the new field of IO sought to measure seller concentration in particular markets and to assess its changes over time. Other studies sought to understand the implications of departures from the assumptions of Marshallian competition for business behavior via theoretical analysis and empirical inquiry. As Mason (1939, p. 66) defined this latter project,

[2] It is interesting that Hotelling's (1929) roughly contemporaneous model of spatial competition, which is widely used today, appears, like Cournot's (1838) influential model of oligopoly, to have attracted very little attention for decades after its publication.

> The economic problem … is to explain through an examination of the structure of markets and the organization of firms, differences in competitive practices including price, production, and investment policies.

The influence of Berle and Means (1932) may explain Mason's view that "the organization of firms" could have a significant influence on market behavior. In any case, he asserted as fact (1939, p. 66) "…that firms are not, regardless of what economic theory may suppose, undifferentiated profit maximizing agencies which react to given market situations in ways which are independent of their organization." This view has long been out of fashion in economics, in part because economic theory has not had much to say about exactly how organization should matter. Non-economists in business schools generally think that organization matters, however, and if organizational economics continues to gain momentum, it would not be too surprising if Mason's view re-emerged in IO.

In his statement of the economic problem central to IO, Mason used the now-unfashionable term "market structure" to state an obvious point, not to make a substantive assertion. If one of the field's objectives is to explain or predict business behavior, those explanations or predictions must be based on quantities that are in principle observable—though it might take sophisticated econometrics to get a good look at them—and one might as well refer to all those observables as constituting market structure. Or, as Mason (1939, p. 69) put it, "The structure of a seller's market, then, includes all those considerations which he takes into account in determining his business policies and practices."

In light of the great importance concentration assumed in academic work and public policy debates beginning in the 1950s, it is interesting to note that in the late 1930s, Mason (1939, p. 66) believed that "…an adequate analysis of price and production policies requires consideration of … elements of market structure which include many more things than numbers and product differentiation." And through at least the late 1950s he remained convinced that "some substantial degree of seller concentration is a necessary condition to the exercise of monopoly power, but it is not a sufficient condition" (Mason 1957, p. 34).

Mason (1957, p. 8) was clear on the intimate connection of the "economic problem" he had defined to public policy:

> Only to the extent that we can infer or predict, however roughly, the performance of firms from observed elements of market structure, can we proceed to any sort of judgment relevant to public action in this area.

In most of the post-war period, arguably the most important area of "public action" related to market structure that IO economists have sought to inform has been horizontal merger policy. Accordingly, one test of whether IO economists deserve to be considered "on a level with dentists" might be whether we analyze proposed horizontal mergers as well as dentists deal with one of their most important problems – say toothaches in infected teeth. I return to this test at the end.

3. Initial Industry Studies

Mason (1939, pp. 65-66) was initially skeptical of inter-industry statistical analysis and advocated instead the performance of detailed case studies of particular industries:

> … the attempt to correlate measures of price behavior with other data such as industrial concentration and product durability on an economy-wide basis is apt to include irrelevant and exclude relevant determinants of price policy. It seems probable that empirical work will achieve better results by a more intensive examination of specific market situations.

Accordingly, beginning in the 1930s, Mason's students and others produced many careful book-length cases studies, often based on documents and other information made public in antitrust trials. The industries covered included retail groceries (Adelman 1959), aluminum (Wallace 1937), cement (Loescher 1959), tin cans and tin plate (McKie 1959), cigarettes (Nichols 1951), and, of course, shoe machinery (Kaysen 1956). Each of these and other studies provided the reader with a great deal of qualitative and quantitative information on seller conduct, and the best of them are still worth reading for the detailed and insightful descriptions that they provide of the operation of real markets. Many of these studies found evidence of collusive behavior, but, of course, many were based on materials generated in major antitrust cases.

Unfortunately, the heavy reliance of this first wave of industry studies on antitrust cases meant that they could not provide balanced or comprehensive coverage of the economy. In addition, during the heyday of this mode of inquiry, available theory did not provide much in the way of useful tools for dealing with oligopolies. Thus while each author provided explanations for the behavior he observed, these explanations were rarely grounded in rigorous theory, and, being based on samples of size one, they were inherently untestable. Some years later Stigler (1964), with an eye to the industry studies that still dominated the IO literature, contrasted his work on oligopoly theory with "the immortal theories that have been traditional in this area."

By the late 1950s, Mason (1957, p. 5) admitted that the focus on case studies had not done what he had hoped: "valid generalizations suitable as a basis for prediction or for public action … are hard to come by." And he seemed to have become more open to alternative methods of inquiry than he had been earlier:

> It is an open question to what extent a recognition of the scope and coverage of oligopoly markets forces us into "sheer empiricism" via an institutional study of particular industries. (Mason 1957, p. 5)

In any case, as I discuss below, by the end of the 1950s most IO economists were no longer doing traditional industry studies.

As Paul Joskow (2005) has reminded us, however, students of regulation did not follow this trend. Regulated industries differed from each other and from unregulated industries in obvious ways, so it rarely made sense to include them in the broad inter-industry studies that came to dominate the field during the 1950s and 1960s, and regulators often mandated publication of considerable disaggregated data that could be used for intra-industry analysis. Thus Stigler and Friedland's (1962) pioneering study of the effects of regulation could focus exclusively on electric utilities. Both the quality and quantity of work on specific regulated industries increased notably in the 1970s, and many of the research threads that began then were long-lived. Douglas and Miller's (1975) research on airline markets, for instance, launched what is now an enormous and growing intra-industry empirical literature that has continued long after those markets were deregulated.

Since the 1960s, IO economists have played a much more important role in antitrust litigation than they had earlier, and that work has often involved close exposure to qualitative as well as quantitative information on the workings of real firms and markets. Expert witnesses in major cases have regularly presented what amount to industry studies in support of broad assertions regarding market conduct and performance. But the historian-like skills that are involved in this work and that authors of the best early industry studies exhibited are not much valued outside the antitrust arena. Accordingly, the increase in the potential supply of academic industry studies based on antitrust cases has not generated an increase in their production. The landmark *AT&T* and *Microsoft* cases have not yielded book-length industry studies, for instance.[3] Though Kwoka and White (2009) have continued to commission and publish essay-length treatments of interesting antitrust cases that are useful in the classroom,[4] the most recent antitrust case to have generated book-length studies seems to have been *U.S. v IBM*, which was dropped by the government in 1982 (Fisher et al. 1983a, 1983b).

Perhaps ironically, well after the initial heyday of book-length industry studies that were based on antitrust cases, interesting generalizations have begun to emerge from the large and growing sample of cases involving cartels. In an important early study, Hay and Kelley (1974) examined the characteristics of industries in which the U.S. Department of Justice had brought price-fixing cases. More recently Levenstein and Suslow (2006) have been able to draw general lessons from a large number of studies of cartels in the U.S. and abroad.

4. Inter-Industry Studies

If Edward Mason was the prophet of case studies in the 1930s, Joe Bain was the prophet of inter-industry studies in the early 1950s. In a pair of influential studies, he led the way to a style of work in which researchers elected “to treat much of the rich detail as random noise, and to evaluate hypotheses by statistical tests of an interfirm or interindustry nature” (Weiss 1971) and, in particular, to attempt to find determinants of monopoly profits by statistical analyses in which accounting measures of profitability served as the dependent variables.

In the first of his influential studies, Bain (1951) carefully assembled a sample of Census industries that he felt most closely approximated economic markets and linked them to profitability data from the Securities and Exchange Commission. He didn’t run a regression (not the easiest thing to do in those days, of course) but instead produced a table showing that the

[3] Temin’s (1989) valuable study of the fall of the Bell System is more a work of economic/business history than a classic IO industry study.
[4] In addition, James Brock (2008) is continuing the series of useful collections of essay-length industry studies that Walter Adams began in 1950.

industries in his sample with four-firm concentration ratios above 70 percent had distinctly higher accounting profit rates than did the others.

In the second and even more influential study, Bain (1956) laid out the classic structure-conduct-performance framework; argued that concentration, product differentiation, and barriers to entry are the main structural variables; and asserted that economies of scale, product differentiation advantages, and absolute cost advantages are the main sources of entry barriers. He then very carefully assessed all of these variables, along with measures of profitability, for a sample of 20 manufacturing industries. It is clear that Bain had studied each of these industries in some depth, presumably along with others that he concluded did not correspond to well-defined markets. He drew on results from surveys using industry-specific survey instruments as well as a variety of industry studies, trade publications, and government data.

Bain (1956, p. 204) found that "the main culprit in establishing excessive or very high barriers to entry would appear to be product differentiation," but noted (p. 203) that half of the industries in the high-barriers category did not have particularly high advertising/sales ratios. Bain's main conclusions, based again on tabular presentations of raw data rather than regressions, and expressed as "extremely tentative implications" (p. 203) were that high entry barriers are associated with high rates of profit, particularly if concentration is also high, but that concentration alone "does not appear to be an adequate criterion of the workability of competition" (p. 204).

Few subsequent authors of cross-section studies were as careful in preparing their datasets as Bain was in these two studies, and most used much larger samples. Greater availability of industry-level data and statistical software packages made it easy to run inter-industry regressions with accounting measures of profitability or average price-cost margins on the left. The positive relation between concentration and profitability that Bain had found generally emerged, at least in published studies, though, particularly in later studies, it was seldom very strong (Schmalensee 1989).

Rather than assess overall entry barriers subjectively as Bain did, most subsequent inter-industry studies simply included independent variables that—their authors argued—measured particular sources of barriers in linear specifications along with measures of concentration. Thus, notably, Comanor and Wilson (1967) found that the coefficient of the advertising/sales ratio, which they took to be a measure of product differentiation, was positive and highly significant, and this was also observed in subsequent studies. The nonlinear, interactive relationship involving concentration and entry barriers that Bain had found was rarely explicitly tested in later studies.

In his widely-used text, Bain (1968, p. 458) concluded that on the basis of (mainly verbal) theory and the accumulated empirical evidence that "monopolistic pricing and profits are favored both by high seller concentration and high entry barriers, and are especially likely to be acute if these two structural conditions exist together in the same industry." It is clear in context that by "monopolistic pricing," Bain meant overtly or tacitly collusive pricing (Bain 1968, ch. 9); he was not talking about markups over marginal cost in single-period Cournot or Bertrand equilibria.

Merger policy in the 1960s also focused on collusion and placed even more stress on concentration than Bain had. In discussing horizontal mergers, the U.S. Department of Justice's (1968) first Merger Guidelines stated that "The larger the market share held by the acquiring firm, the more likely it is that an acquisition will move it toward, or further entrench it in, a position of dominance or shared market power." The phrase "shared market power" clearly expresses a concern with what we would now call *coordinated effects*: adverse changes in (expected) market performance that occur because changes in market structure make collusive behavior more likely. Apart from the mention of dominant firms, there is no discussion of what we now call *unilateral effects*: adverse changes in market performance that occur because changes in market structure affect the consequences of non-collusive behavior. The standards presented for enforcement in the Guidelines involved only market shares.

The same year the Department of Justice issued its first Merger Guidelines, the (Johnson) White House Task Force on Antitrust Policy issued its report (Neal et al. 1968). The Task Force was chaired by the then-dean of the University of Chicago Law School, Phil Neal,[5] and included economists from MIT, Vanderbilt, and the State University of New York at Buffalo. The Task Force's report, generally called the Neal Report, called for (and produced a draft of) "no-fault" deconcentration legislation, which would enable the government to break up leading firms in industries that were more concentrated than efficiency required. This proposal was endorsed by

[5] Dean Neal's affiliation makes it clear that "Chicago School" was an imprecise label even then.

11 of the Task Force's 13 members, including all three economists. (Robert Bork was one of the two dissenters.) Inspired by this report, which attracted serious attention in academic and policy circles, Senator Philip Hart introduced no-fault deconcentration legislation in 1971 and 1972.

In 1969, another Task Force, this one commissioned by the incoming Nixon Administration and chaired by George Stigler, issued its report on productivity and competition (Stigler et al. 1969), generally known as the Stigler Report. In addition to Stigler, the authors of the Stigler Report included economists from Yale, Michigan, and Chicago. In contrast to the Neal Report, the Stigler Report (Stigler et al. 1969, p. 846) explicitly declined to endorse "no-fault" deconcentration, but it was only somewhat less hostile to concentration. It recommended "a policy of strict and unremitting scrutiny of the highly oligopolistic industries" and opined that

> …collusion that can be incontrovertibly inferred from behavior (such as persistent, stable price discrimination in the economist's sense) should not bring immunity from the Sherman Act, and we are confident that structural remedies will be sanctioned by the courts in cases where … lesser remedies are likely to be unavailing.

Most IO economists no longer think that price discrimination by itself is indicative of significant competitive problems, let alone problems that might justify breaking up established firms.

In an impressive survey of "Quantitative Studies of Industrial Organization," based on an invited lecture delivered at the 1969 North American Meetings of the Econometric Society, Leonard Weiss (1971) focused almost exclusively on inter-industry econometric studies. Commenting approvingly on that survey, William Comanor (1971, pp. 403-4) provided a crisp description of IO at the end of the 1960s:

> Despite the original prescription of Edward Mason, practitioners in this area have moved from an early reliance on case studies toward the use of econometric methods of analysis. To a large extent, therefore, a review of econometric studies of industrial organization is a review of much of the content of the field.

IO began to shift away from inter-industry econometric studies, and academic support for no-fault deconcentration began to evaporate shortly after Weiss and Comanor wrote. In a particularly important critique, Harold Demsetz (1973) noted that as an empirical matter high seller concentration generally reflected highly unequal market shares rather than small numbers of sellers. He then argued that if in some industry a few firms were persistently more efficient than others, one would expect those firms both to grow relative to the others, thus to obtain high market shares, and to earn rents on the sources of their differential efficiency, thus to contribute to high industry-average profits. Thus, Demsetz concluded, even if there were no collusion anywhere in the economy, concentration and profitability might nonetheless be significantly correlated—both high in markets with substantial efficiency differences and low where those differences were smaller.

Similarly, basic theory makes clear that high price-cost margins, whatever their source, would tend to make high advertising/sales ratios optimal, all else equal (Schmalensee 1972, ch. 7). In fact, almost all of the variables that are used in inter-industry profitability studies were logically endogenous; they depended to at least some extent on seller conduct (Schmalensee 1989). Moreover, Weiss (1969) and others showed that if advertising spending has durable effects but is expensed (as is conventionally done) rather than being treated as an investment, a positive relation between advertising/sales ratios and accounting profitability could arise as an accounting artifact. By the early 1980s when Fisher and McGowan (1983) launched their powerful assault against the use of accounting measures of profitability to infer monopoly profits, they met little resistance. During the 1970s and increasingly in the 1980s Leonard Weiss (1990) and others attempted to deal with some of these problems by studying variations in price-cost margins across local markets for the same industry, but this approach has also not endured.

The era of inter-industry profitability regressions was largely over by the early 1980s, and the idea, derived in large part from those regressions, that concentration was a uniquely powerful determinant of market conduct had lost its hold on the field. This, of course, was progress, though of the sort that Mason (1957, p. 3) ruefully ascribed to the field's earlier years: "...progress—if it may be called that—in this area has largely been the development of striking theses that have later disintegrated under the corrosive action of factual criticism."

The first wave of inter-industry studies (pre-1980, roughly) is rarely read these days. To some extent this is unfortunate. Unlike case studies, well-designed inter-industry studies have the potential to establish stylized facts of general validity and, even if they involve reduced-form correlations among endogenous variables, of some utility. I believe such facts did emerge from the early inter-industry literature (Schmalensee 1989). Even though they may often lack clear causal interpretations, such facts may be productive sources of hypotheses for empirical work and of assumptions for theorizing.

5. Theory Development

When I took my Ph.D. general examinations at MIT in June 1967, two of my elective fields were industrial organization and "advanced theory." (Micro theory, presumably less advanced, was a required field.) A number of my peers told me that I was making a mistake, since "there is no theory in IO." This was not quite true for the field as a whole, as I discuss below, but it was a perfectly accurate description of MIT circa 1967. As Stigler (1968, p. 1) noted at about the same time, much of the literature in industrial organization had been "so nontheoretical, or even antitheoretical, that few economic theorists were attracted to it."

At least to somebody who has never lived in Hyde Park, the rise of theory in IO seems to have been begun by George Stigler in the 1960s. (Chicagoans tend also to give considerable credit to Stigler's colleague Aaron Director, but Director wrote little, and his influence was thus highly localized.) Stigler's landmark 1964 paper on oligopoly theory (Stigler 1964) portrayed oligopolies as aspiring cartels that were more or less likely to succeed depending on the ease of detecting and punishing cheating. It embodied a two-regime view of concentrated industries, in which conduct was either competitive or monopolistic at any point in time, a view that has been enormously influential ever since.

Stigler's (1968) later collection of essays demonstrated the potential of rigorous theoretical analysis to address a variety of IO issues. Moreover, while most of the sources he cited were case studies, statistical compilations, antitrust decisions, and assorted other sources, Stigler was able to point to a good deal of relevant pre-existing theory beyond Chamberlin (1933) and Robinson (1933). In addition to Cournot (1838), he cited theoretical literatures on limit-pricing (e.g., Modigliani 1958), basing-point pricing (e.g., Machlup 1942), and kinked demand curves (e.g., Sweezy 1939), as well as Dorfman and Steiner (1954) on the choice of advertising and product quality by a monopolist.[6] He might also have cited the influential work

[6] In contrast, Bain's (1968) contemporaneous text, written in the Harvard tradition and more than twice as long as Stigler's (1968) collection, cited only the limit pricing literature and Stigler (1964) on oligopoly theory.

of Bowman (1957) on tying as price discrimination and the so-called single monopoly profit theorem of Director and Levi (1956).

Beginning in the 1970s, Michael Spence (e.g., Spence 1975) and others began to develop formal models of seller conduct in imperfectly competitive markets. Spence (1976) and Dixit and Stiglitz (1977) formalized Chamberlinian monopolistic competition, for instance; Salop (1979) revived Hotelling's (1929) model of horizontal product differentiation, and Shaked and Sutton (1982) introduced the notion of vertical differentiation. In the early 1980s, thanks to Kreps and Wilson (1982), Milgrom and Roberts (1982), and others, the use of the tools of extensive-form game theory transformed theoretical work in IO. Notably, Green and Porter (1984) used those tools to produce an influential formalization of Stigler's (1964) aspiring-cartel model of oligopoly, and subsequent work has continued to employ game-theoretic tools in the study of collusion (Feuerstein 2005) and other dimensions of conduct.

Writing a decade after Weiss (1971), I could confidently assert that "[j]ust as industrial organization economists began to become econometricians in the 1960s, many began to become theorists in the 1970s" (Schmalensee 1982, p. 256). A large number of theoretical papers on traditional IO topics had appeared in major journals, and by 1980 one could speak with a straight face of "theoretical IO".

In this literature "the rich detail" ignored in inter-industry studies often had a qualitatively important impact on predicted outcomes, sometimes along with modeling details that seemed hard to connect with features of real markets. It often mattered whether competition involved prices or quantities, for instance, whether time was continuous or discrete, whether moves were simultaneous or sequential, and how beliefs were assumed to be altered by events off the equilibrium path. Common knowledge assumptions seemed unrealistic to some, and the need for sophisticated refinements to prune multiple Nash equilibria was disturbing. Increasingly general versions of the Folk Theorem (e.g., Fudenberg and Maskin 1986) made it increasingly less likely that game theory was going to solve the oligopoly problem with which IO began.

In a review of Volumes 1 and 2 of the *Handbook of Industrial Organization* (Schmalensee and Willig 1989), Peltzman (1991, pp. 203) calculated that 78 percent of its pages were devoted to theory and noted that

> No one in or near the field in recent years can deny the increased prominence of formal theory, especially game theory. It is also undeniable that work on the ostensible frontier of the field—the sort done in the most prestigious graduate programs and then propagated by their best graduates—has leaned increasingly toward formal theory. (Peltzman 1991, p. 206)

But he went on to complain about the field's emphasis on theory:

> The main reason for my skepticism is the seeming inability of the recent theory to lead to any powerful generalizations. This is especially true in the area of game theory, where the problem seems beyond remediation. The impression one tends to come away with from many of the theory chapters in the Handbook is of an almost interminable series of special cases. The conclusions drawn from these cases tend to be very sensitive to the way problems are defined and to the assumptions that follow. (Peltzman 1991, p. 206)

As Fisher (1989) put this point somewhat more gently, the new theories were mainly exemplifying, describing what *could* happen, rather than generalizing, describing what *must* happen.

There is an important methodological issue here. If one believes, as Peltzman, along with Mason and Bain, seemed to, that there exist a few "powerful generalizations" that would provide a more or less complete understanding of conduct and performance in any real market, then work that produces "an interminable series of special cases" can only be a distraction from the search for those generalizations. On the other hand IO theory suggests that reality is just not so kind. It suggests that material differences among real markets are sufficiently complex that a few sufficient "powerful generalizations" simply do not exist, so that the search them is futile. If this is true, the logical approach would seem to be to treat that "interminable series of special cases" (or at least the ones that seem a priori sensible) as a set of conceptual tools that can be used to pose informative questions and guide market analysis, not to yield definite predictions..

At the end of his survey of models of oligopoly, Shapiro (1989, pp. 408-09) provided a clear statement of this second point of view, which I share:

> I would emphasize my view that the variety of models of oligopolistic interactions is a virtue, not a defect. … I view these game-theoretic models as providing the industry analyst with a bag of tools. In other words, we have been able to identify quite a large number of strategic considerations that come into play when there are large firms and enough sunk costs so that threats of entry are not the primary determinants of industry behavior… After learning the basic facts about an industry, the analyst with a working understanding of oligopoly theory should be able to use those tools to identify the main strategic aspects present in that industry.

I would add that the available models are also useful in helping the analyst decide what are "the basic facts" in any particular case. One can interpret the fact that the models in our tool kit "tend to be very sensitive to the way problems are defined" as vindicating Mason's belief that the list of relevant structural variables goes well beyond seller concentration and product differentiation. The basic idea is then to use available models and the tools used to construct them to find or build industry-specific models that are consistent with "the basic facts" and that provide useful insights into conduct and performance in the industry under study.

While this general approach to industry analysis is in fact still widely employed, particularly in antitrust matters, there is little doubt that the relative importance of theory development in IO research has receded considerably since the 1980s. Jean Tirole's (1988) superb exposition of IO theory circa the late 1980s is still a widely-used text almost a quarter-century after its publication. In the Preface to an undergraduate textbook published at the start of this century, Cabral (2000, p. xii) observes without further discussion that "most of the developments in [IO] occurred during the 1980s."

Of course, theory development has not stopped.[7] Armstrong and Porter (2007), describing the evolution of the field since the first two volumes of the *Handbook of Industrial Organization* were published, note that, "Since then the empirical literature has flourished, while the theoretical literature has continued to grow, although probably not at the pace of the preceding years." Among interesting recent developments, I believe the work on multi-sided

[7] A comparison of Belleflamme and Peitz (2010) with Tirole (1988) provides a useful view of post-1980s developments in theoretical IO.

platform businesses, or two-sided markets as they are often called (e.g., Armstrong 1996, Rochet and Tirole 1995, Weyl 2010) is likely to have a substantial impact on the field. In addition, a good deal of the most influential theoretical work in the last few decades has been concerned with developing frameworks for use in applied analysis rather than deriving specific predictions (e.g., Erickson and Pakes 1995, Doraszelski and Pakes 2007).

At the end of the day, however, while the development of rigorous theory since Stigler (1964) has provided IO economists with a large box of useful tools, those tools have not enabled us to predict the nature and intensity of competition in oligopolies on the basis of observables. That is, theory development has not solved the oligopoly problem that Mason set out to solve, and it has been clear to most in the field since the 1980s that further theorizing using available analytical methods is unlikely to solve it.

6. Econometric Industry Studies

When work on IO theory began to wane during the 1980s, empirical work waxed. Both trends may have been reactions to the apparent inability of theory to generate clear predictions in many interesting situations. At any rate, by late the late 1980s Tim Bresnahan and I (1987) felt confident in proclaiming that a set of papers that we had collected was illustrative of an "empirical renaissance in industrial economics." We argued that the new wave of empirical work in the 1980s differed from the inter-industry wave of the 1960s and 1970s in three important ways. First, most of the new studies used specially constructed data sets, rather than Census-provided industry-level data. Second, many of the new studies employed formal theory as more than a source of candidate independent variables and made appropriate use of post-OLS econometric methods. Third, there was a shift toward the firm rather than the industry as the unit of analysis and, indeed, in many cases toward the brand. Thus, it was as if many IO economists had finally responded to Weiss's (1971, p. 398) observation a decade earlier that "perhaps the right next step is back to the industry study, but this time with regression in hand."

Some interesting econometric industry studies had been done before the 1980s, of course. As an undergraduate I was attracted to IO in part by Paul MacAvoy's (1962) early use of econometric methods to detect the presence of monopoly or monopsony power in natural gas fields and his analysis of cartel stability in the 19th century railroad industry (MacAvoy 1965).

Iwata (1974) estimated conjectural variation parameters for the Japanese plate glass industry, and Gollop and Roberts (1979) performed a similar study on U.S. coffee roasting.

The 1980s saw a wave of econometric industry studies. Some followed Iwata (1974) and Gollop and Roberts (1979) and estimated conjectural variation parameters. Others, notably including Bresnahan (1987), computed parameter estimates under alternative behavioral assumptions and used non-nested hypothesis testing to choose among them. Porter (1983) used a switching regressions approach to revisit cartel stability and instability in 19th century railroads, applying a rigorous version of Stigler's (1964) two-regime view of oligopoly. Many studies, particularly later ones, followed Rosse (1970) and used sellers' first-order conditions to infer cost functions, typically because brand-level cost data were unavailable. Almost all this work focused exclusively on price or output choices, but Roberts and Samuelson (1988) began a small but interesting literature that modeled decisions about advertising spending, an important competitive weapon in many differentiated-product industries. A variety of strategies were adopted in this literature to identify conduct variables or indicators, many derived from Bresnahan's (1982) basic insight about the impacts of demand curve rotations under different competitive conditions.

In an influential survey of this first wave of econometric industry studies, Bresnahan (1989) christened their basic approach the "New Empirical Industrial Organization" or NEIO. At the end of that paper, Bresnahan (1989, p. 1053) observed that "some of the studies appear to be finding conduct well toward the collusive end of the spectrum." and noted that (p. 1051) "the integration of different case studies to give a unified picture of the whole map [from structure to conduct and performance] is an obviously attractive prospect." Almost a quarter-century later, however, we still lack the array of studies that would be necessary to attempt such an integration. One obvious and important reason for this is the difficulty of obtaining suitable data for many industries. But I think two other reasons are also important.

The first of these reasons is that the methods used in many of the NEIO studies were clearly in need of improvement. As Reiss and Wolak (2007) have made clear, for instance, one cannot readily assign a behavioral interpretation to estimated "conjectural variations," and Corts (1999) has demonstrated that the classical "conjectural variations" specification can fail badly as an "as if" measure of the extent of market power. Moreover, the NEIO studies typically imposed implausibly strong restrictions on demand functions in order to avoid having to estimate large numbers of cross-elasticities. For instance, Bresnahan's (1987) study of pricing in the U.S. automobile industry assumed pure vertical differentiation, and Roberts and Samuelson (1988) used firm-level demand functions for cigarettes that were simply linear functions of goodwill stocks and numbers of brands. In addition, identification problems and the endogeneity of prices and quantities were sometimes not adequately handled. Naturally, then, rather than applying the first-wave NEIO methods to a broader sample of markets, many researchers turned their attention to the development of better techniques.

For instance, Jerry Hausman (1997) and others developed nested demand structures that imposed restrictions on substitution effects between brands in different investigator-specified segments. And Ariel Pakes and Stephen Berry, along with a number of co-authors, developed demand models in which observations on product characteristics are used to restrict substation patterns, unobserved characteristics are explicitly allowed for, and buyer heterogeneity can be accommodated.[8] Under the general banner of "structural IO," a great deal of subsequent effort has gone into refining these and related techniques and developing alternative identification strategies and approaches to price/quantity endogeneity.[9] This technically impressive work has enabled IO economists to produce detailed models of a variety of industries and to answer questions about the welfare implications of policy changes that could not have been quantitatively addressed a decade earlier. And structural IO has rather dramatically removed the last traces of the low-brow, low-tech stigma that IO had long endured!

The development of clever new modeling and estimation methods almost always has a higher professional payoff than producing one more application of known techniques. Thus, while recent years have also seen a wave of applied structural IO studies of particular industries, one side-effect of the wave of methodological work is that many of these studies seems to be more about the method used than about the industry studied. As a result, the substantive findings in some technically impressive papers are not very persuasive. In addition, it is natural that those who have invested in mastery of structural IO techniques are interested in getting a return on that investment and thus tend to focus on contexts and problems where those techniques can be most

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[8] Foundational papers in this literature include Pakes (1986), Berry (1994), and Berry, Levinsohn and Pakes (1995).
[9] Useful surveys of structural IO methods and their applications have been provided by Ackerberg et al (2007) and Reiss and Wolak (2007). Knittle and Metaxoglou (2008) discuss the challenges and difficulties involved in estimating the highly nonlinear models that dominate this literature.

usefully employed. While it is a lot to ask that those with deep technical expertise also study in depth each industry they study, there is much to be said for serious collaboration between those with technical facility and those with deep industry knowledge.

A second reason why the last quarter-century of IO research has not produced a wide array of industry studies that bear on Mason's "economic problem" is simpler: that problem has not been much studied. Most researchers seemed to find the problem of diagnosing the nature of seller behavior either uninteresting or, more likely, too hard. Thus, in striking contrast to the earlier NEIO literature, few structural IO papers attempt to estimate parameters describing seller conduct or to test alternative conduct hypotheses. Almost all simply assume single-period Nash equilibria (typically in prices) without defending that assumption and use the implied first-order conditions in estimation.[10] This simplifies estimation, of course, and the assumption that the nature of equilibrium never changes makes counterfactual analysis cleaner. But this approach does represent something of an inversion of the field's historic priorities: instead of trying to understand how sellers behave in oligopolies in general or in some particular industry, it is now almost standard to make a convenient assumption about seller behavior and to concentrate instead on estimating cost and demand parameters.

There have been some exceptions to that rule, most of which have relied on accounting cost data.[11] Genesove and Mullin (1998) were able to use very reliable cost data on sugar refining to estimate conjectural variations, and Wolfram (1999), Sweeting (2007), and Hortaçsu and Puller (2008), among others, used data on marginal costs of electricity generation to test hypotheses about behavior in wholesale power markets. Nevo (2001) estimated a demand system for ready-to-eat cereal brands, for which cost data were not available, under alternative behavioral hypotheses. He was able to choose among those hypotheses by comparing the industry-average price-cost markup they implied with industry-average accounting data.

Wang (2009) and others have compared the dynamics of oligopoly pricing to Edgeworth price cycle equilibria. A number of authors, including Hendricks et al (2003) have tested behavioral hypotheses in auctions, and the literature on detecting collusion continues to grow,

[10] In the last several years a number of dynamic models have been estimated (e.g., Ryan (forthcoming)), generally under the assumption of noncooperative Nash behavior.

[11] In addition to the studies mentioned in this paragraph and the next, Berry et al (1999) estimate their model under a variety of behavioral assumptions, but they do so as a robustness check on their estimates of the effects of a particular trade policy; they do not attempt to choose among those assumptions.

though not at a rapid pace (Porter (2005). Finally, Gasmi et al. (1992) use non-nested hypothesis testing to evaluate price and advertising behavior by Coca-Cola and Pepsi-Cola.[12] This is of course not a complete list of recent studies concerned with estimating conduct parameters or testing hypotheses about conduct, but it does not omit large numbers of such studies.

Some might argue that the single-period Nash assumption, which rules out any sort of collusion, is in fact generally appropriate. And, indeed, there are plenty of examples of concentrated industries in which rivalry seems sufficiently intense as to make any sort of collusion implausible. On the other hand, Bain (1968, p. 308), a close observer of real markets, considered market conduct between competition and monopoly to be common: "The great bulk of all observed conduct patterns lie somewhere within the in-between range of incomplete collusion—with elements of collusion and elements of independence." If Bain was right, if only to the extent that such patterns exist in some industries sometimes (surely their prevalence is an interesting empirical question), the question of how to model such behavior deserves serious attention.

Following Stigler (1964), one might attempt to model sellers as playing a complex dynamic game of imperfect information, in which cheating on a (possibly incomplete) collusive understanding is at least sometimes deterred by the threat of punishment of one sort or another. But this approach becomes very complex very quickly, and econometric tests of hypotheses involving such games seems beyond us in most cases at this point (Doraszelski and Pakes 2007), and the hyper-rationality needed to play such games optimally seems substantially to exceed the capabilities of most if not all of our species. In some situations, particularly in relatively stable settings, it may be appropriate to work with games that are easier to play optimally (Fershtman and Pakes (forthcoming)). Alternatively, it may be possible explicitly to model behavioral and organizational influences on conduct. Or, the best way forward may be to attempt to develop and employ parsimonious parameterizations in the spirit of the "conjectural variations" approach that can provide reliable reduced-form estimates of the location of conduct along "the in-between range of incomplete collusion."[13]

[12] Slade (1995) seems to be the only other published study to model competition in both price and advertising, but non-collusive behavior is simply assumed.
[13] The recent discussion of discriminating between oligopoly models by Berry and Haile (2010) may point in this direction.

Whatever approach proves most productive, there is simply not strong empirical support for the assumption that all market outcomes are well-modeled as noncollusive Nash equilibria—any more than there is support for the Rotemberg-Saloner (1986) vision of ubiquitous collusion. Accordingly, I believe it is critical for the progress of IO—as a field that is concerned with the real world—that empirical work should develop sound, broadly applicable methods to relax or test the single-period Nash assumption and that those methods be systematically applied to the Mason-NEIO agenda of constructing a mapping between market structure, broadly defined, and the nature and intensity of competition.

7. Evolution and Dentistry

I noted at the outset that evolution in natural systems is not well-described as a process of species-by-species improvement but rather as a process of adaptation. By becoming larger, for instance, a species might become better able to survive in environments that put a premium on strength, but it might be disadvantaged where speed is particularly important. Similarly, I believe that as IO has de-emphasized first book-length industry studies, then inter-industry econometric studies, then formal theorizing, and finally econometric tests of seller behavior, it has both gained and lost at each stage, though I am persuaded that over time the gains have far outweighed the losses.

To produce a good book-length industry study, for instance, one must exercise some of the traditional skills of an historian. One must be able to assemble information of various sorts, quantitative and qualitative, from diverse sources into a compelling, consistent picture. As the professional reward for doing this sort of work has diminished, less of it is done, and the average IO economist has become less good at it. I believe this has reduced persuasiveness and thus the impact of some recent econometric industry studies. I agree wholeheartedly with Joskow's (2005, p. 176) Lesson #3:

> A necessary but certainly not sufficient condition for good empirical industrial organization research is that the analyst should know the basic structural and institutional attributes of the industry well and should understand fully the data that are available to study it.

It is frustrating to read or listen to an impressive empirical paper and conclude that the author has a good understanding of the methods employed and the quantitative properties of the data but knows little about the industry that those data describe or how they were generated.

The persuasiveness of substantive conclusions that are derived from econometric work generally depends critically on the plausibility of one or more modeling assumptions. Without detailed industry-specific knowledge, it is usually difficult to mount a convincing defense of such assumptions. Some have argued that this is inevitable, that the demands of estimating complex models seem often to require a level of abstraction that cannot easily be related to "basic structural and institutional attributes" of many real markets. As I noted above, however, collaboration between technical and industry experts is a natural way to deal with this problem. Moreover, Asker's (2010) work on a bidding cartel and Ho's (2009) study of hospital-insurer networks are counter-examples indicating that even without such collaboration it is possible to combine deep knowledge of institutional facts with rigorous use of modern modeling and estimation techniques.

As I noted above, the skills required to produce a good industry study are still important and rewarded in antitrust work, where an involved economist is typically confronted with the range of information employed by the authors of the first wave of industry studies. Many IO economists have found involvement in antitrust litigation to be an excellent way to sharpen these skills and to broaden their educations by learning about "the rich detail" of real industries.

The rejection of inter-industry studies of accounting profitability initially had two adverse consequences, both of which I believe have now largely been overcome. First, many IO economists seemed to believe that accounting data could not be safely used for any purpose. Contributors to the large and rapidly growing literature on productivity have generally had little choice but to rely on such data all along (Syverson 2011), of course, and, as noted above, some accounting data in some industries are clearly informative. I believe that the knee-jerk aversion to using accounting data has generally been replaced by a proper recognition of the need for care when doing so. This is a welcome development, since, as noted above, the recent econometric industry studies literature suggests that it may be necessary after all to use accounting data in order to perform reliable industry-specific assessments of seller conduct.

Second, for a time the rejection of inter-industry profitability studies moved the field away from the use of inter-industry studies to search for any empirical regularities or to test any broad hypotheses. The right lesson was that it is very difficult to do inter-industry studies well, not that they can never be useful, and subsequent authors, including Evans (1987), Dunne et al (1988), and Sutton (1991),[14] have shown that well-designed inter-industry studies can make significant additions to our knowledge. While such studies are likely for the foreseeable future to account for a small share of the field's research effort, I am confident that they will continue to be undertaken.

The main potential loss from the de-emphasis on theorizing after the 1980s seems to me that new entrants to the field may be less able on average than some earlier generations to use the available theoretical tools to structure empirical inquiries. We now have an extraordinarily rich set of rigorous models available, and while they do indeed say more about what might happen under certain conditions than what must happen, the questions that they raise are often the keys to insightful market analysis. There certainly are areas in which more theoretical work seems likely to yield significant insights, and theorizing will no doubt continue. But, absent truly profound breakthroughs, it seems very unlikely that such work will enable economists to make confident predictions about the nature and intensity of competition based on observable elements of market structure. More narrowly, it will not enable us to predict whether an oligopoly's prices will be collusive, single-period Bertrand, or somewhere in Bain's "in-between range of imperfect collusion."

If that is correct, then, as I argued above, a natural way forward is to return to Mason's original program of industry studies, this time using modern econometric techniques to assess competitive behavior in particular industries, and then to look across industries for general rules and useful diagnostic tests to complement the predictions of theory—just as physicians rely on clinical experience as well as general models of the human body. But this has not yet been attempted. As Einav and Levin (2010, p. 160) put it at the end of their useful overview of recent empirical work in IO:

[14] It is worth noting that Sutton (1991) is one of the few inter-industry studies with a dataset prepared with care that is comparable to that employed by Bain (1956).

> After 20 years of industry studies, we know *a lot* about how specific industries work, but this knowledge is extremely disaggregated. We have detailed analysis of [many markets]. But this knowledge does not easily accumulate across industries. As a result, industrial organization has ceded many of the interesting and important questions about the overall organization of production in the economy to other fields such as trade and macroeconomics. [emphasis in original]

Moreover, turning away from the Mason-NEIO program of generalizing from a set of industry-specific conduct assessments and instead basing most empirical work on assumptions about the nature of conduct means giving up hope of producing either useful empirical generalizations about the impact of market structure on conduct or, for that matter, diagnostic tests than could be used in policy settings where structural variables are at issue. While I am hopeful that in the future researchers will be both willing and able to use improved structural IO methods to assess the nature of conduct in a wide set of markets and that this may contribute to a solution to Mason's problem, the jury on that question has yet even to be empanelled.

So, at the end of the day, after IO has traveled down this long and winding road, are its practitioners now entitled to be thought of as "humble, competent people on a level with dentists?" Even putting aside the always difficult matter of humility, I think the verdict is mixed. Like today's dentists, today's IO economists can address and sometimes even solve problems that were utterly intractable until quite recently. But dentists have gotten much better at dealing with the problem with which their profession began – toothache – while we are only slightly better at dealing with the oligopoly problem that triggered the emergence of IO as a distinct field.

Merger policy illustrates this last point well.[15] As Shapiro (2010) demonstrates, U.S. policy toward horizontal mergers is enormously more sophisticated now than it was when the 1968 *Guidelines* were issued. Market shares are no longer the only structural indicator considered. Drawing mainly on post-1968 advances in theory, as well as enforcement experience (but not, as far as I can tell, on empirical findings), the 2010 *Guidelines* outline a much more complex analytical framework than did the 1968 *Guidelines*.

[15] White (2010) provides a useful overview of the evolution of IO economics focusing on its influence on U.S. antitrust enforcement and policy.

But there has been a change in emphasis as well as an increase in sophistication. As noted above, apart from worries about the creation of dominant firms, the focus in 1968 was exclusively on changes in the likelihood of collusion, on coordinated effects. As Shapiro (2010) notes, since 1992 the Guidelines have also considered unilateral effects, the performance impacts of changing structure assuming no change in the nature or intensity of competition. The tools available to analyze unilateral effects have become much more powerful. Merger simulation models (Budzinski and Ruhmer 2010) can be employed to integrate information from a variety of sources, and the newly introduced Upward Pricing Pressure (UPP) test (Farrell and Shapiro 2010) is a great improvement over the traditional market definition/market share approach in many settings that involve differentiated products.

But these new tools shed no light whatever on coordinated effects. Merger simulation models, for instance, generally assume single-period Bertrand competition (Budzinski and Ruhmer 2010),[16] and the usual version of the UPP test employs what is basically a monopoly model: it assumes that the demand curves facing the merging firms do not change as a consequence of their merger or their post-merger price changes (Jaffee and Weyl 2012). It is thus perhaps not surprising that, as Shapiro (2010) notes, "In recent years a sizeable majority of [Department of Justice] merger investigations have focused on unilateral effects." Is this because the authorities now know that mergers rarely if ever substantially increase the likelihood or effectiveness of collusion? Or do the enforcement authorities focus on unilateral effects because they, like almost all IO economists and the courts, are generally more comfortable looking under the unilateral effects lamppost, where the light is indeed *much* brighter than it used to be, rather than looking into the darkness that still largely envelops coordinated effects?

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[16] Peters (2006) finds that merger simulation models did a poor job of predicting the consequences of a set of airline mergers and argues that this is mainly because those mergers induced changes in market conduct.

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