THE STATE OF THE INDUSTRIAL ORGANIZATION FIELD

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Competition and monopoly stand at the center of market activity, and they are as important in China as they are in all other economies. Competition usually drives markets toward excellent performance, making them efficient and innovative, but monopoly power instead usually causes higher prices, inefficiency and less innovation. The research field about this has been called "Industrial Organization" in the United States and "Industrial Economics" in Europe. The field has great importance for China's markets and policy choices in the coming decades.

I've visited and taught in China several times since 1983, and I'll summarize here the field's recent content. It is easy to misunderstand the field's recent developments. The topic has been exciting; there are lots of tensions, strong debates, and many attempts to inject "new" ideas and methods. Those "new"

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ideas are sometimes valuable, sometimes not. The field's mainstream work is applied research, and that continues to be abundant and productive. But there have been attempts since the 1970s to make the field merely a mere matter of pure theory, mathematics, and game theory. There are several aggressive schools of thought, and there are a lot of long-running unresolved issues.

Along with competition and monopoly power, this field is about the endless policy efforts (using antitrust and regulation-deregulation) to promote **effective competition**. Often markets instead have weak and ineffective competition.[2] Sometimes there is "natural monopoly," and the need is then to apply economic regulation, so as to reach the many economic goals, especially innovation, efficiency and fairness. Figure 1

2 For summaries and background on the history, see William G. Shepherd, The Economics of Industrial Organization, 5th ed., Waveland Press, 2003, especially chapter 1; and F.M. Scherer and David Ross, Industrial Market Structure and Economic Performance, 3d ed., Houghton Mifflin, 1991; and Almarin Phillips and Rodney E. Stevenson, "The Historical Development of Industrial Organization," History of Political Economy 6 (Fall 1974), pp. 324-42.

presents the field's format, as it is usually summarized in the U.S. literature.

/ Figure 1 goes about here [it is Figure 1.2 on page 8] /

Since the 1880s, the field has developed four main concepts of markets, which range from high monopoly power to highly effective competition. They are summed up in Table 1, with a number of U.S. examples.

/ Table 1 goes about here [it is Table 1.1 on page 13] /

Each type of market has a structure, which primarily involves the market shares of all the firms. When a firm's market share is larger, it usually has more power over the market. Figure 2 illustrates the structure of a market that has one dominant firm among many smaller firms. An "entry barrier" may also be an element in the market's structure. It would be some impediment at the edge of the market, which might reduce the ability of new "potential" competitors to enter the market from outside.

/ Figure 2 goes about here [it is Figure 1.3 from page 11] /

Pressures and Distortions. This field is highly unusual in several ways. For one, it has always been like a pressure cooker, containing very large companies, huge legal cases, and intense debates about law and policy. The pressure goes up and down from time to time, as big waves of mergers and monopoly activities have risen and ebbed during the field's history.

Research and debate were particularly active during 1900-1915, the 1930s, and from the 1960s on.

After the 1960s, an array of major cases took place in the U.S. and Europe, and they had serious effects on the field. Examples include the big U.S. v. IBM case (which I personally helped to start); the FTC In Re Xerox case of 1974-76; the case that led to AT&T's astonishing break-up in 1984; two huge waves (even manias) of mergers, peaking in 1986 and 2000; the British privatization crusade after 1980, which spread to some other countries; and the rapid deregulation of banking, utilities and other large industries in the U.S., Britain and Europe.

Amid these spectacular events and rough debates, the field's mainstream work on real markets has continued to be strong and fruitful. There are many distinct layers of debate, including:

1. How "perfect" are actual markets? Are most or all markets virtually "perfect," or are most markets partly imperfect, while some leading markets have sharp imperfections?

2. Actual competition versus potential competition. Which matters most: **actual competition** (often embodied in firms' market shares) or **potential competition** (the possibility of new entry from outside the market)?

3. Research methodologies -- theory versus applied research. Is it pure theory (especially game theory) that creates knowledge best, or is applied research on real-market conditions the better method?

4. Policies. Which policy devices have worked best, during the last century in thousands of markets? The main tools are antitrust, regulation, deregulation, and public enterprise.

I'll comment here on the evolution of the field since the 1880s, including several newer ideas and debates. Then I'll focus on three important issues and on a colorful example, to summarize the current debates. Finally, I'll briefly compare the pro-competition (antitrust) policies in Europe and the U.K. with those in the U.S.. All these points may help Chinese scholars and officials understand the many complicated choices that they face.

I. MY PERSPECTIVE

I'm just one observer from distant America, with a professional viewpoint that may be idiosyncratic. But I do know the terrain. I've worked throughout this field since about 1955, and I've known personally many of the significant contributors and debaters in this field during this modern era.

My perspective these days reflects four kinds of experience. **First**, my past research has touched on many technical topics, in U.S. and European markets.

Second, in 1967-68 I was economic adviser to Donald F. Turner, then the brilliant head of the U.S. Antitrust Division in Washington DC. I got a lot of top-level experience and insight during that intense year of mergers, dominant firms (AT&T, IBM, General Motors, Xerox, etc.) and tight-oligopoly cases. Back then, these were enormous and very powerful companies; now of course they have finally been reduced by policy actions and competition, so that it is hard to remember their immense power. We considered suing the Big 3 auto firms for their shared monopoly, the AT&T company for its vertical monopoly, and more. But little new antitrust was actually done then, despite occasional claims by conservative ideologues that antitrust was way too active in the 1960s.

Third, for more than a decade, from 1990 to 2001, I was the editor of the Review of Industrial Organization. That journal promotes "applied" research on real markets and policies, ranging over research issues and policies of many schools and countries. I observed the people and trends in detail, and I tried to deal fairly with all authors and ideas.

Fourth, I recently completed a wide-ranging book that surveys the innovators who have added to the Industrial Organization field from its beginnings to the 1980s. I'm collaborating on it with Henry de Jong. We've been thinking hard about past innovations and controversies.

Further Background on the Issues

Almost all of the important ideas in this field were well-known and discussed as long ago as 1920. Such long-standing familiarity is true of many long-developing fields, where the reality is thoroughly familiar and the ideas have long been well known. Claims of "new" ideas in this field have often been false. Some other new ideas have been mere shifts in attention among the various long-familiar concepts. In some cases, old ideas are simply given new labels as a marketing ploy, to make them seem new.

Related to this, Industrial Organization's main real subject -- competition, monopoly power, and various abuses -- has been widely familiar for centuries, and famous for being important. People have seen and coped with real monopolies and market dominators since human economic life began, far back beyond the mists of antiquity. We're all experts, in some degree. For instance, it has been obvious to everyone that competition raises effort and efficiency, while controlling more of the market gives more monopoly power.

Partly because its subject is so familiar, this field has had no seismic "Eureka!" events or Big Discoveries during its history. The successive gains have mostly been marginal, not fundamental. In fact, many of the "innovations," including some of the most highly-publicized ones, have been intensely debatable, of questionable importance, or even harmful to knowledge by displacing solid concepts and measures.

Even the innovations that are positive have a finite period of influence. They add some value, are debated and whittled down, are absorbed as modest additions, and then may be displaced entirely by new innovations. Problems and damage to knowledge can occur when a modest innovation's authors and advocates push it too far, claiming that it has paramount and permanent importance. That can block other valuable ideas and methods.

Yet the career system creates a relentless pressure that often inflates the supposedly "new" ideas. Here, as in every field of knowledge, the rise of young scholars requires then to replace the old, by force if necessary. The rising generation casts about for different ideas and techniques to use as weapons to displace their seniors. The young researchers have to publish something and try to seem original, and the "powerful new techniques" can be claimed to be superior to the field's established ideas and to the wisdom and judgment of the older scholars. In fact, the senior scholars have learned wisdom and sophistication, but the young often deride that as mere woolly "judgment, inferior to their tight (but simple-minded) "rigor."

The process has a systematic bias toward using pure theory to displace complex real-world research. The young (and all theory-skilled scholars, for that matter) are drawn to pure theory because it can be done so quickly and with none of that tedious effort to learn about real conditions. Also, if the theory is "new" and technically difficult and there are no reliable data at hand, then the theory has a certain glamour and is hard to test or disprove.

The urge to replace established knowledge has been especially strong in this field, where new young experts since the 1960s could look forward to unusually large rewards for private consulting and testimony, using their "better" ideas. Some colleagues have grown rich from consulting and from testifying as “expert” witnesses. Some of them list over a hundred cases in their professional vitas. I often marvel that the serious research in the field has managed to be reasonably good, despite these powerful distracting temptations.

The "new" weapons in the debates have included the use of math, of pure theory, of entry barriers, of the efficient structure hypothesis, and of such items as the H Index, Tobin's q, the "SSNIP" 5 percent basis for defining markets, and other new terms -- all used as bullets and bombs in the battles.

Of course, the "market for ideas" has long promoted healthy strife among competing ideas in all fields, including this one. But the outcomes can often go seriously astray. The market process can be biased and distorted, with a tendency toward extremism as new people try to jump in with a big impact. The biases can yield ideas which actually blank out superior ideas and valid evidence. That can reduce knowledge and understanding.

One important lesson: the field does NOT necessarily progress steadily. There have been backward slides and reductions in knowledge.

Finally, it is important to recognize that there are many goals for the economy, including innovation, static allocational "efficiency," fairness in distribution, freedom of choice, and others. Table 2 sums them up. Static efficiency is just one, and it may be only modestly important.

/ Table 2 about here [it is Table 2.1 in page 32] /

II. THE LONGER HISTORY

Problems of Data and Short Memories

Regrettably, the field has long suffered from poor information, especially about such crucial things as market shares, profits, innovations and efficiency. Firms relentlessly hide this sensitive information about themselves. Moreover, any actual data about market shares and profits are often distorted. Huge dollar interests depend on the data, creating high pressures for secrecy and distortion. In such a vacuum of reliable data, anybody -- economist, lawyer, officials, politicians -- can feel free to urge almost any "new" idea, because testing those ideas is extremely difficult or impossible.

And even the strongest concepts can be frivolously denied.

Even more than most fields, the Industrial Organization field requires special care and great sophistication in combining pure **logic** with real-world **amounts**. Logic is important: we always need new ideas, which can often clarify reality. But because the testing of ideas is hard to do, the following definition of theory may come true: "Theory is going wrong with confidence." A mere possibility (for example, that markets are "perfect" or that monopoly is always weak) is often said to be "conceivable" or "interesting," as a mere “insight.” Then somebody will claim it to be "quite possible." After that, it may be asserted to be "often true" and important. Then it’s "usually true." Then "always true." Such progressive overstatements have been only too true about the "efficient structure" hypothesis and about entry barriers compared to market shares. I discuss both issues below.

Meanwhile, the rise of pure theorists since 1970 has greatly reduced any awareness of the field's long, rich history. "Modern" research actually began far back in the 1880s, though some observers have dated it back only to the 1930s, when the theory of oligopoly became popular. But many current theorists have a tinier perspective. They think that almost nothing was done in the field before 1970, except loose talk. Before game theory's “brilliant” rise in the 1970s, they say, there was nothing but a Stone Age. No theory, no rigor, no "serious" work, everything obsolete, according to them.

Such ignorance is breath-taking and destructive. Instead, important research began in the 1880s, most of the important ideas were discussed by 1920, and a wide array of concepts and methods had come far by the 1960s.

Some Features of the Field's History

The field's development in the U.S. is sketched in Table 3. The post-Civil-War boom of 1865-1890 was followed by waves of mergers, causing rising concentration in many heavy industries. The emergence of neoclassical theory during the 1880s and of economics as a profession was marked by hot debates about the dangers of monopoly despite the happy lessons of neoclassical analysis.

/ Table 3 goes about here [it is Table 1.2… at pages 24-5] /

The Great Merger Wave of 1897-1901 created near-monopolies in many scores of the largest U.S. industries, ranging from such old trades as twine, leather and cigarettes to the heavy industries like metals and railroads.[3] Nearly all the recent justifications for rampant mergers -- scale economies,

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[3] See John Moody, The Trust About the Trusts, Moody Publishing, 1904.

consolidation, market stability, etc. -- were gravely proclaimed then, and many exaggerations about them were vigorously debated and deflated.[4] The fledgling U.S. Antitrust Division attacked no less than six of the ten largest U.S. corporations during 1904-1915. There was extensive factual research into industrial monopoly power by the Bureau of Corporations.[5] Financial power was also studied intensively.

In sum, the early field was very rich and effective. It focused tightly on the really important core problems, such as entrenched market dominance, monopoly profits, the damage to efficiency and innovation, the resistance to new competition, and the need for strict action. Major cases were brought and won, and the U.S. showed that firm antitrust policies could cure the worst problems. Meanwhile, progress began toward fairly effective public regulation of privately-owned utility

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4 See Charles J. Bullock, "Trust Literature: A Survey and Criticism," Quarterly Journal of Economics, February 1901, pp. 167-217, among an array of articles and books.

5 See Hans B. Thorelli, The Federal Antitrust Policy, University of Chicago Press, 1954, and William Letwin, Law and Economic Policy in America, Random House, 1965, for detailed discussion of the wide array of economic concepts that were extensively discussed.

monopolies (especially in electricity, telephones and western railroads).

Even at its beginnings in the 1880s, the field's ideas, research and policies were surprisingly extensive, focused and relevant -- including market definition, economies of scale, competitive processes, and collusive behavior. Since then, there has been progress in some areas, especially in getting more data. But true intellectual progress has been fitful, and odd claims have been frequent. For reasons I noted above, some newer ideas have tended to replace or to confuse valid core ideas and methods.

An example is oligopoly theory, starting with Chamberlin's and Robinson's contributions in the 1930s.[6] Though the topic was interesting and significant, the fevered, obsessive focus on oligopoly displaced almost all attention from the more acute problems of market dominance by single firms, such as AT&T, IBM and Xerox in the 1960s. The new study of potential competition and entry barriers added to this distraction from the central problems after 1956. It redirected attention away from real

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6 Edward H. Chamberlin, The Theory of Monopolistic Competition, Harvard University Press, 1932, and Joan Robinson, Economics of Imperfect Competition, Macmillan, 1933; also William J. Fellner, Competition Among the Few, Knopf, 1949.

competition and monopoly at the core of the market. Instead, the market's edges became the fixation, along with "potential entrants" who might somehow come in from entirely outside the market.

Oligopoly theory in the 1930s and game theory after 1944 certainly stimulated a lot of technical activity -- the sense of excitement and discovery about game theory was much like the advent of seemingly magical nuclear power at the same time. But the theorizing also moved attention away from single-firm dominance and toward the secondary issue of multiple-firm cooperation – and then, only by firms who don't directly talk with each other. Concentration data were developed and then mined extensively, on into the 1960s and 1970s. But again, the focus was away from single-firm dominance and instead toward the lesser problem of oligopoly.

Since the 1970s, some additional theories about entry conditions have continued to divert the field away from the important core issues. The clearest example is Baumol's "contestable markets" theory of 1982. In some cases, the focus and clarity of established research have been replaced by a proliferation of vague "new" ideas, which are actually just confusing. For a parallel kind of sterility, think of academic "music," which exists in its own isolated world. Usually such arcane, cerebral "music" has no harmony, rhythm or melody –

nothing at all that centuries of genius have created as genuine music. Academic music is written only for other abstract academic specialists, in a kind of ingrown, hothouse group-think. In the same way, abstract paintings with plain monotone canvases have been like over-abstract economic theory: empty and meaningless. Exotic economic theory can be equally extreme and sterile.

As for the rise of free-market neoliberal Chicago School 2 ideas in the 1960s-1970s, their proponents actually did little practical research on the basic conditions. They simply assumed that markets contained only perfect conditions. On that imaginary basis, the patterns which strongly linked market shares with profit rates were simply claimed instead to "prove" their "efficient structure hypothesis," and to disprove the presence of market power. To Chicago School 2 writers, firms could gain and hold high market shares only because they were “superior”: more efficient and innovative. That claim was logically possible, but only if the markets were quite perfect. Even moderate imperfections could let firms "win" by using anti-competitive tactics that were deceptive or unfair. Indeed, a high market share inevitably creates or increases imperfections in the market.

Before the free-market overstatements gained currency in the 1980s, the research field had properly given extensive study to the possibilities of market imperfections. Table 4 summarizes 19 categories of market imperfections that have been covered in the literature. They are many, familiar, and important. But the free-market analysts simply declared perfection, without providing evidence to prove it. In the conservative Reagan politics of the 1980s, this optimism fitted the political trends well.

/ Table 4 goes about here [it is Table 3.2 at page 69-70] /

As a result, antitrust policies and staffing in both agencies (the Antitrust Division of the Department of Justice, and the Federal Trade Commission) were cut back deeply after 1980, and some policy areas were eliminated (e.g., conglomerate mergers, price discrimination, vertical restrictions). Though theorists and free-market advocates praised this for reflecting "superior" economics, antitrust was severely weakened.

Europeans have been more skeptical of the "new IO theory" ideas, and antitrust in Europe and the United Kingdom has remained more centrist and practical. So Europe and the U.K. now have, on the whole, the most thorough and effective policies treating market power (see section V below). Some U.S. "new IO" scholars claim that Europe and the U.K. are merely "obsolete," but Europeans in particular are largely correct in keeping the focus on real markets, market shares, and the real costs of monopoly. Of course, some European and U.K. colleagues may disagree with this view.

III. LEADING EXAMPLES OF ONGOING DEBATES

Among the points and debates in Table 3, several are of large long-run importance and continue to be unresolved. They define much of the current content and state of the field. I will discuss four of them.

Issue #1: Perfect Markets and "Efficient Structure." Since the 1970s.

The idea that most markets are essentially perfect began with Aaron Director, a law professor (not an economist) at the University of Chicago. He converted George Stigler from skepticism to this rosy optimism, after Stigler arrived at Chicago in 1957.[7] Stigler and Director developed other conservative colleagues, and Stigler created a well-funded atelier at Chicago where new young scholars made their mark by attacking all kinds of public policies for "distorting" markets rather than improving them. The original skeptical Chicago School became the uncritical Chicago 2. Their claim: high market shares are good, because they are won in perfect markets.

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7 See also George J. Stigler, "Perfect Competition, Historically Contemplated," Journal of Political Economy, February 1957, pp. 1-17.

"Perfect" markets actually must meet extreme requirements of perfection:

1. total information about current and future conditions, known completely by all participants, including all producers, all consumers, and all potential entrants,
2. no lags or frictions,
3. all participants are always rational, including consumers,
4. a high number of relatively equal competitors,
5. stable technology, so equilibrium can be reached.

The perfection must also hold for all capital markets, which are crucial; all firms must have equal access to ample investment funds. That requirement for perfection is particularly stringent. If capital markets have imperfections, that contaminates all other markets and dominant positions.[8]

Chicago 2 writers thus sought to eliminate the long, deep debates about imperfections in real markets, by simply pretending that perfection could be assumed. But the writers did little real-world research to validate those perfect-market

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8 My own main critique of the theory is in William G. Shepherd, " 'Contestability' versus Competition," American Economic Review, September 1984, pp. 572-87; see also my "Contestability vs. Competition -- Once More," Land Economics, August 1995, pp. 299-309.

beliefs. It is true that some trends since the 1950s may tend to reduce imperfections in many markets. For example, better telecommunications and the rise of the Internet have spread information more widely. Some markets have grown more transparent and open to quick actions.

But there are some important limits on this. For instance, the 1990s brought claims that the Internet would replace most of the wholesale purchasing activity and costs of inputs for major industries, such as automobiles, metals and machinery. Those hopes turned out to be mostly empty, and the Internet purchasing organizations either folded quickly or are struggling. Markets were not drastically changed. As always, a "radical" new idea and method turned out to have an impact that was much smaller than its "expert" advocates claimed.

Chicago 2 writers also claimed, narrowly, that static allocation is the only relevant goal for competitive markets. Such a focus on just allocative efficiency is far too narrow and shallow. Allocational efficiency is only one among many goals; innovation in particular is widely recognized to be more important.

Chicago 2 writers freely accepted the strong, proven correlation of market shares with profit rates, as it is illustrated in Figure 4. They then flatly denied the most obvious lesson, that the correlation shows some role for monopoly power, perhaps a large role. Chicagoans declared instead that all markets are perfect, then the regression line merely proves that dominant firms are more efficient.

/ Figure 4 goes about here [it is Figure 3 on page 118] /

This issue persists as sharp and divisive. Think for yourself if markets are commonly "perfect" and whether static allocation is the only economic goal.

Issue # 2: Should Research and Policy Focus On the Market's Center (the leading firms' market shares), or On Its Edges (its potential-entry conditions)?

This has been a leading issue for half a century, and it still is. Entry might matter, but it's likely to be a minor problem, at the edges of the market.

In 1956 Joe S. Bain tried to make it a big issue, aiming to show that barriers could increase the force of collusion among the leading firms. He tried to measure the "causes" of barriers, including large size, cost advantages, advertising intensity, and other "capital barriers."

But it quickly became evident that barriers are a vague and ethereal idea, impossible to define clearly. As for practical measures, it's possible to make only vague estimates of the "height" of barriers. The best-known example is Bain's guesses (in his 1956 book) of "absolute, high, moderate or low" barriers in 20 industries. Also, the larger literature soon developed at least 20 possible “causes” of possible barriers, and virtually all of them are also impossible to measure reliably. Worse, “potential competition” is itself even harder than that to measure. Which “potential entrants” might enter the market? How rapidly, and with what new market shares? The whole effort to define and measure barriers turns out to be nothing more than sheer guesswork.

Nevertheless, some economists grew increasingly excited by the idea of barriers in the 1960s. By the 1970s, Chicago writers had reversed Bain’s viewpoint. They claimed that entry was virtually free and quick in most markets. The resulting free entry, they said, eliminated any efforts of oligopolists to collude or even of dominant firms to raise prices at all. In their extreme view, potential competition somehow mattered more than actual competition or monopoly. This claim served the interests of such powerful 1960s-1970s dominant firms as IBM, AT&T, General Motors and Xerox.

In fact, reality is the opposite. Real businesses are tightly centered on the endless struggle to get more market share. Almost always, higher profit yields come from controlling higher shares of the market. Entry barriers are, by contrast, little more than an esoteric topic for airy scholarly speculation in chatty theoretical seminars. They are rarely even discussed in real business events and reporting.

"Contestability"

In 1982 the fetish for entry and barriers was taken to the extreme by a handful of theorists, who had been working for the AT&T company, which was then the colossus in the telecommunications industry. William Baumol and several colleagues announced what they said was a radically superior pure theory of entry and exit. "Contestability" (their label for it) was a theoretical notion, in which a new firm could enter an entire market instantly, control it all, and then leave instantly. That action could entirely nullify even a complete, powerful monopoly.

To these theorists, pure contestability now replaced real competition entirely! The whole theory of competition was, they said, now obsolete. Such wild claims were not immediately shown to be empty, because Baumol was then the President of the American Economic Association.

The Baumol group freely admitted that they have no real-market examples of perfectly-contestable markets. It's just an interesting idea, they said, which give "insight." Yet they used it frequently and emphatically in policy discussions and in sworn testimony about large corporate mergers and dominant-firm positions. This "powerful" and "widely-accepted theory," they would say, proves that there can be no monopoly effects whatever. My own main critique of the theory is in my "Contestability versus Competition," American Economic Review, September 1984; see also my "Contestability vs. Competition Once More," Land Economics, August 1995.

In addition, the Baumol group offered the idea of "uncommitted entrants." Those are hypothetical firms in adjacent markets whose products and costs are not much different from those already in the market. The firms might shift "easily" into the market, and so the theorists treat them as if, somehow, they were as significant as firms that are already in the market.

Yet, despite the possibility, these firms are certainly not yet in the market. The theory seeks to convert a mere possibility of entry into a definite fact of large, successful entry.

The wider research debate continues, and you can consult your own viewpoint to see how you might decide important real cases. Has Microsoft held a virtual monopoly, extending it into other markets? Or is Microsoft a paper tiger, beset by powerful potential entry? The same question applies to electricity suppliers in newly deregulated power markets, to telecommunications firms that are merging to get high market shares, to Intel the dominant chip maker, to pharmaceuticals makers, and to many others.

<u>Issue #3:</u> Pure Theory and Game Theory versus Applied Research. Since the 1970s.

From 1932 on, Chamberlin, Fellner and Bain made oligopoly a complex topic, and they stressed that oligopoly would tend toward collusion. By 1952, Bain developed fairly thorough empirical tests that concentration yielded higher profits, and Weiss's major collaborative study of 1989 later affirmed it. But Neumann-Morgenstern's landmark book of 1944 inspired pure game theory, and the field soon focused on just two players who don't collude and who play a brief game. During the 1950s, Shubik made extensive efforts to apply the theory to real markets but concluded in 1961 that the theory did not have practical research possibilities.

But in the 1970s, like the mythical Phoenix, game theory came back to life, especially among young theorists who used it as a major device for professional success. This boom in game theory related also to the markets-are-perfect viewpoint; without perfection, game analyses often grow too complicated to resolve. The theorists said -- as of course they still do – that game theory provided great rigor and power; it was, they said, the only logically correct basis for thinking about market power.

This viewpoint became prevalent in teaching Industrial Organization on many U.S. campuses during the 1980s.

But since then a better balance has been reached in the field. By the 1990s, real-market research had revived and improved, and the free-market ideologues were less influential.