

Empirical Industrial Organization: A Progress Report

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The field of industrial organization has made dramatic advances over the last few decades in developing empirical methods for analyzing imperfect competition and the organization of markets. These new methods have diffused widely: into merger reviews and antitrust litigation, regulatory decision making, price setting by retailers, the design of auctions and marketplaces, and into neighboring fields in economics, marketing, and engineering. Increasing access to firm-level data and in some cases the ability to cooperate with firms or governments in experimental research designs is offering new settings and opportunities to apply these ideas in empirical work.

This essay begins with a sketch of how the field has evolved to its current state, in particular how the field's emphasis has shifted over time from attempts to relate aggregate measures across industries toward more focused studies of individual industries. The second and primary part of the essay describes several active areas of inquiry. While this is in some sense a survey, our goal is not to be comprehensive but to highlight key ideas and illustrate how modern research has approached different problems. We also discuss some of the impacts of this research and specify topics where research efforts have been more or less successful. The last section steps back to offer a broader perspective. We address some current debates about research emphasis in the field, and more broadly about empirical methods, and offer some thoughts on where future research might go.

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What Got Us Here?

Industrial organization is concerned with the structure of industries in the economy and the behavior of firms and individuals in these industries. The field has historically focused on how markets depart from idealized conditions of perfect competition, whether because of scale economies, transaction costs, strategic behavior, or other factors. From an empirical perspective, this leads to questions about how competition plays out in different markets and how it relates to industry structure. Not surprisingly, these questions overlap with public policy issues, such as the appropriate antitrust stance toward concentrated industries or the design of regulatory mechanisms for industries with scale economies.

Modern research in industrial organization has evolved largely in response to two challenges that plagued the field into the 1970s. The first was a lack of compelling theoretical models for studying imperfectly competitive markets. This problem was largely reversed by the game theory revolution of the 1980s, which permitted much sharper modeling and analysis of problems such as product differentiation, network effects, barriers to entry, pricing strategies, and the effect of asymmetric information in product markets (Tirole, 1988). Indeed once this line of research began to advance, models proliferated so rapidly that some prominent members of the field were lamenting their overabundance (Fisher, 1989; Peltzman, 1991)!¹

The second challenge for empirical work was a lack of good data and of convincing empirical strategies for evaluating hypotheses about competition or industry structure. The most active strand of empirical research into the 1980s consisted of cross-industry regression analyses that attempted to link market structure across industries with market outcomes. This agenda can be traced back at least to Bain (1951, 1956) and is sometimes referred to as the “structure–conduct–performance” paradigm. In a typical study, a researcher might use data on a cross-section of industries to regress an outcome measure, such as accounting profit, on an industry concentration measure, such as the combined market share of the four largest firms. Researchers of course recognized that concentration might depend on many of the same factors that influenced profitability, creating an endogeneity problem. More sophisticated studies might try to “fix” this problem by assuming that industry concentration is derived from “exogenous” barriers to entry: for instance, technological scale economies or fixed costs such as advertising or research and development.

While the broad question of what makes industries more or less competitive is an important one, the cross-industry approach had notable limitations. Many studies relied on data consisting of a small number of industry aggregates with

¹ The central issue at the time was that many of these models seemed to be sensitive to hard-to-assess assumptions about consumer preferences, asymmetric information, and the ability of firms to make strategic commitments. This sensitivity turns out to have had an effect on subsequent empirical research, in that it made researchers appreciate the potential importance of even small details in the underlying strategic environment.

empirical measures that proxied imperfectly for the relevant economic quantities. For instance, a common concern was that accounting measures of profits available in the data might not be an accurate gauge of true economic returns. Even more troublesome, most studies in this literature lacked convincing strategies for econometric identification of causal effects. What is needed to identify the type of causal effect described above is exogenous variation in industry concentration. While cross-industry differences in advertising-to-sales ratios or capital intensity may correlate with industry concentration, it is hard to argue that they are unrelated to factors affecting firm profits and hence qualify as useful instrumental variables. This problem was well summarized by Schmalensee (1989, p. 954), who began his definitive review of the cross-industry literature by acknowledging that “essentially all variables that have been employed in such studies are logically endogenous.”

Both the concerns about cross-industry regression models and the development of clearer theoretical foundations for analyzing imperfect competition set the stage for a dramatic shift in the 1980s toward what Bresnahan (1989) coined the “New Empirical Industrial Organization.” Underlying this approach was the idea that individual industries are sufficiently distinct, and industry details sufficiently important, that cross-industry variation was often going to be problematic as a source of identification. Instead, the new wave of research set out to understand the institutional details of particular industries and to use this knowledge to test specific hypotheses about consumer or firm behavior, or to estimate models that could be used for counterfactual analysis, such as what would happen following a merger or regulatory change.

The current state of the field reflects this transition. Today, most of the influential research in empirical industrial organization looks extensively to economic theory for guidance, especially in modeling firm behavior. Studies frequently focus on a single industry or market, with careful attention paid to the institutional specifics, measurement of key variables, and econometric identification issues. Advocates of this approach argue that when successful, it combines the conceptual clarity of economic theory with convincing empirical measurement and that the focus on individual industries offers the best opportunity to understand the competitive mechanisms at work.

Of course, industry studies also have their drawbacks. They may lead to narrow analyses and sometimes leave researchers reluctant to generalize their findings to other contexts where the institutions may be different. As a result, the broader take-aways often tend to be either qualitative insights (for example, that it is possible for entry to have a substantial effect on prices or for asymmetric information to matter significantly in market outcomes) or empirical methods that can be applied in multiple settings. In addition, critics sometimes argue that the econometric models used in some studies, which often rely heavily on equilibrium assumptions, can lead to nontransparent analyses that obscure the link between the estimated parameters and the underlying variation in the data. We return to these concerns in the last section.

Where Has the Action Been? A Brief Tour

In this section, we describe a few active areas of empirical research in industrial organization. We start with the problem of estimating consumer demand, which is a key input for almost any study of market competition. We then discuss models of short-run price competition, both in traditional markets where firms post prices and in bidding markets where firms compete in auctions. Finally, we turn to the problem of longer-run competition, where entry, exit, and investment decisions can shape market structure and the competitive landscape.

In each case, we discuss some of the empirical methods that have been developed and a few applications that usefully illustrate specific points. One point to emphasize is that although there is no recipe for empirical research in industrial organization, many papers share common themes. Modern research in the field often begins with a theory of market equilibrium, which can be more or less explicit depending on the application. Often researchers rely explicitly on assumptions about optimization or equilibrium to draw inferences about underlying parameters. In other cases, the theory is used to derive testable predictions about the data or to interpret estimates of causal effects. Careful attention is paid to the institutional context and market details. One broad lesson from looking across industries is that these details can be very important.

Estimating Demand in Imperfectly Competitive Markets

Many studies of imperfect competition begin by describing consumer behavior, often by estimating a model of consumer demand for the products of the relevant industry. Consumer demand may be interesting in its own right—to understand what consumers value and how they substitute between products as prices or product offerings change, to assess the welfare effects of mergers or new products, or to measure how information or advertising affect consumer decisions. Demand elasticities are also a critical input for identifying the degree of market power that firms can exercise. Many of the focal advances in this area have come in the form of novel econometric models that in principle can be applied broadly across markets as well as in nonmarket environments.

The typical situation in most industries is that consumers face a choice of products that vary along different dimensions. Product differentiation bestows firms with a degree of market power. For instance, one factor weighing into Apple's pricing of the iPhone is that some consumers just prefer the iPhone to comparable phones made by Palm or Nokia. Moreover, consumers who value the iPhone interface may be different from those who value the Blackberry's ability to synch with corporate email servers. The problem becomes more subtle when we recognize that Apple sells several iPhone versions, and that consumers may be able to delay their purchase and wait for prices to fall. Thus, Apple's pricing incentives depend not just on a single demand elasticity with respect to a single Apple price, but on a set of elasticities that describe how customers will shift their purchasing as Apple changes its pricing schedule.

Assessing the demand for differentiated products, therefore, calls for an analysis that relates the prices of a set of competing products, and perhaps other strategic variables such as advertising, to the quantities sold of those products. This task poses an immediate challenge. Even a simple linear demand system for an industry with n products will have n^2 price coefficients, and estimating these coefficients will require distinct variation in each of the n prices. The more products, the greater the identification challenge, particularly because many potentially attractive sources of variation such as cost shocks may simultaneously affect all prices. Other idiosyncratic shocks may affect just a few prices, leaving the effect of other prices not identified.

Work on demand modeling, therefore, has centered on the trade-off between 1) allowing flexible substitution patterns and 2) the lack of variation in typical data that allows such substitution patterns to be flexibly identified. One strategy is to divide products into segments and estimate a model that restricts substitution patterns across segments but allows flexibility within segments. Hausman (1997) applies this approach in studying the demand for ready-to-eat cereals, dividing products into adult, family, and kids' cereals. Another strategy is to describe products in terms of a relatively small number of characteristics, and assume that consumers trade off price and other characteristics in a way that is uniform across products. Berry, Levinsohn, and Pakes (1995) take this approach in modeling the demand for autos by describing cars in terms of their size, horsepower, and gas mileage as well as a quality characteristic that is not captured by the three observable attributes.

These techniques have been highly influential. They are commonly used in antitrust work, both in merger reviews to define the scope of the market and as an input to simulations, and in antitrust litigation to determine damages from excessively high prices. In recent years, these techniques have diffused into neighboring fields in economics, including trade, education, housing, health, and environmental economics. Because they allow researchers to map demand estimates to welfare, they also have been influential in debates over adjusting the Consumer Price Index to account for new goods and substitution bias.

In practice, these models of consumer demand commonly are estimated using either individual choice data or aggregate data on prices and market shares. Estimation requires variation that will identify the demand response to price and product changes. A standard concern is that because prices in a market tend to respond to demand shifts, there can be a simultaneity problem in trying to infer how purchasing decisions respond to price changes. This is the imperfect competition analogue to the textbook demand and supply simultaneity problem. Just as in the textbook setting, researchers frequently adopt an instrumental variables strategy.

In part because it is hard to find independent movement of many product prices, some of the most popular identification strategies rely on restrictions across equations in the demand system. One such approach is to use a product's price in other markets as an instrumental variable, under the theory that cross-market correlation in the price of a given product, conditional on observed demand

characteristics, will be due to common cost factors rather than unobserved features of demand. An alternative is to instrument for prices using the nonprice characteristics of competing products, which proxy for the degree of competition.

Neither solution is perfect, and the source of price variation and its power has to be evaluated in each application. Take the latter approach involving the nonprice characteristics of competing products. In the case of the iPhone, the relevant variation might arise from competing firms introducing touch screens, creating additional competition for Apple and leading it to reduce the iPhone price. While the story is not implausible, caution is clearly warranted. For instance, if competitors were themselves responding to the surprisingly high demand for iPhones, the exclusion restriction required for a valid instrument would be violated.

Our own view is that many applications of these methods—while they are often very careful in clarifying the statistical conditions under which their identification strategy is valid—tend to be rather thin in explaining the precise source of identifying variation, in arguing why the required statistical condition is likely to hold, or in providing first-stage regressions and other diagnostics.

An important development in this regard is the increased availability of proprietary data on consumer behavior and the opportunity to engage in experiments sanctioned by firms. For example, a recent paper by Lewis and Reiley (2009) examines a large-scale experiment that randomized the exposure of consumers to Internet advertising from a large retailer and tracked subsequent purchasing behavior. Although the focus is not on price elasticities, their study demonstrates the data-gathering capabilities of new technologies and the increased willingness of firms to cooperate in academic research. These trends are likely to create many opportunities for future research.

One point to recognize, however, is that experimental measurement is not a panacea. In a typical demand setting, while experimental measurements of the consumer response to a specific change in prices or information are informative, they are only a starting point. Explicitly modeling consumer choices provides a way to link different behavioral responses and connect them to underlying parameters of interest,² as well as a way to translate estimated behavioral responses into statements about consumer welfare. So while we imagine that better and richer data will allow researchers to obtain more precise and convincing estimates of consumer responses to prices, such improved estimates will complement rather than substitute for the demand techniques described above. We return to these points in the final section.

² One example of this is Hendel and Nevo (2006), who point out that short-run estimates of demand elasticities can be misleading if consumers respond to sales by stockpiling nonperishables. Hendel and Nevo develop a model that relates estimated short-run elasticities to long-run elasticities, which are more relevant for the price effect of mergers. Another example is Cohen and Einav (2007), who show how data on insurance choices can be used to estimate risk preferences under the assumption that consumers maximize expected utility from wealth.

Market Power and Price Competition

A long-standing and central problem in industrial organization is the extent to which market outcomes reflect the exercise of market power or some form of implicit or explicit collusion. Porter's (1983) study of nineteenth-century railroad cartels, Bresnahan's (1987) study of the automobile industry, and Nevo's (2001) study of the breakfast cereal industry are classic analyses of market power. The approaches taken in these and related papers provide the basic toolkit for marrying demand and supply in an equilibrium analysis of imperfectly competitive markets.

Underlying each of these studies is a model of market equilibrium. The most common model involves Bertrand–Nash price competition with differentiated products. In equilibrium, each firm sets its price to equal marginal cost plus a mark-up that depends on the semi-elasticity of the firm's demand curve, taking the prices of competing products as given. These equilibrium conditions allow a researcher who has obtained estimates of consumer demand and firm costs to compute equilibrium prices, or to test hypotheses about noncooperative pricing behavior, perhaps against alternative behavioral assumptions such as collusive pricing.

In a typical study, a researcher might start with prices and quantities from a cross section or panel of markets. Often the first step is to estimate market demand, for instance using one of the strategies described in the previous section. On the supply side, researchers sometimes have access to accounting data on costs, although as emphasized by Bresnahan (1989), accounting practices generally are not geared toward reporting the economic notion of marginal cost that the theory implies should be relevant. An alternative is to infer costs from observed prices by relying on an assumption of profit maximization (Rosse, 1970). To do this, the researcher computes the optimal markup using estimated demand elasticities and subtracts it from the observed price to obtain an estimate of marginal cost. This approach relies on the strong assumption that firms are setting profit-maximizing prices, but it is often employed due to its elegance and the fact that it does not require direct cost data. Our view is that at times this approach can be a bit too seductive, in the sense that researchers sometimes shun available cost data on the grounds that it is imperfect, even when it could be quite informative or at least complementary. A few studies, including Nevo (2001) and Hortaçsu and Puller (2008), combine direct and indirect cost measurement to cross-check their analyses and test the hypothesis of equilibrium pricing.

The basic framework of imperfect competition that we have just described provides a starting point for a wide variety of analyses. Many of the early industry studies focused on distinguishing the unilateral exercise of market power from collusive alternatives or measuring the price and welfare effects of mergers, taxes, or new goods. More recent work has incorporated and studied the effects of search costs, price discrimination, retail sales, and consumer stockpiling; adverse selection effects; and the use of nonprice strategies to attract customers. This work can be highly diverse, linked by the conceptual framework of imperfect competition but not necessarily constrained to "traditional" product markets.

One example is a recent paper by Ellison and Ellison (2009), which analyzes how Internet price comparison engines affect competition among a set of Internet retailers. They use carefully documented price variation to show that the price ranking provided by the search engine leads to dramatic consumer price sensitivity. This sensitivity gives firms an incentive to engage in “obfuscation” to manipulate consumer search and in this way to relax what would otherwise be cut-throat price competition. A second example is the work by Gentzkow and Shapiro (2010), who construct a measure of newspaper “slant” and estimate consumer demand for “slant” using variation in the distribution areas of local newspapers. They then use the estimated demand as an input in a model of optimal choice of “slant” by newspaper owners. A final example comes from our own work on consumer credit markets (Einav, Jenkins, and Levin, 2010). In that work, we start by using discrete shifts in pricing to estimate credit demand, in this case for subprime auto loans. We then incorporate the demand estimates into a model of loan pricing in order to study screening and credit terms as set by a lender with market power.

A key point to emphasize about these applications is that while the use of detailed data and relatively transparent identification are important, the results themselves are most interesting when viewed through the lens of a particular theoretical model. As with any empirical study, the exact results apply to a particular setting at a particular time, and we have already argued that across the broader economy there is a great deal of heterogeneity in the way markets and industries operate. Thus, what makes the analyses meaningful beyond a narrow context is that they illuminate theoretical mechanisms—manipulation of consumer search, catering to ideological preferences, or pricing under asymmetric information—that will also operate in other markets at other times.

While we have focused on academic studies, one notable effect of the methods described above has been to shift the standards for empirical evidence in merger reviews and antitrust litigation. Thirty years ago, it was common for antitrust arguments to rest on simple summary measures of industry structure such as concentration ratios and Herfindahl–Hirschman indices. Nowadays, the Department of Justice and the Federal Trade Commission, which are tasked with reviewing proposed mergers, commonly undertake sophisticated econometric studies to define industry boundaries and to assess the likelihood of price increases or collusive behavior following a merger. These exercises often draw on academic research, and in turn have motivated the development of new empirical models.

Competition in Auction Markets

The section above described models of imperfect competition that apply most directly to traditional consumer markets where firms post prices. Many intermediate goods markets are bidding markets. For example, firms and governments often solicit bids to supply goods or services, or use auctions to sell resources in limited supply. Though not always appreciated, the connection between bidding markets and traditional price competition is very close. In recent years, economists have developed a set of empirical methods for analyzing auction markets that in

many ways parallel the imperfect competition framework discussed above (Athey and Haile, 2006; Hendricks and Porter, 2007).

To see the connection, observe that in an auction context, firms bidding to supply a product or service set prices in a way that trades off a higher probability of winning against a higher profit margin if they do win, just as a firm setting the price of its product in a consumer market trades off the prospect of higher sales against having a higher margin. In an important contribution, Guerre, Perrigne, and Vuong (2000) drew on this connection to suggest how a researcher might use bidding data to infer the underlying values or costs of bidders. Roughly, the idea is to use data from a sequence of auctions to estimate the probability, from an individual bidder's perspective, that a given bid will win. This is sufficient to calculate the optimal markup, and hence to estimate the values underlying individual bids, just as Rosse (1970) proposed to estimate costs from observed prices.³

Athey, Levin, and Seira (2008) use this approach to study whether competition is affected by whether bidders are asked to submit sealed bids or instead compete in an open ascending auction. They analyze data from the U.S. Forest Service, which in the 1980s used both sealed bid and open auctions to sell timber, in certain cases randomizing how sales would be run. Regressions exploiting this variation reveal that sealed bid auctions tended to attract more bidders and sometimes generated higher prices. Athey, Levin, and Seira show that a model of equilibrium bidding that incorporates differences in the size and technologies of potential competitors can match the empirical patterns. They use the model to test the hypothesis that certain auctions may have been prone to bidder collusion.

In addition to providing a structured environment to analyze traditional questions about imperfect competition, auctions also provide an empirical laboratory for studying strategic behavior in the presence of asymmetric information. A pioneering example is Hendricks and Porter's (1988) study of government auctions for offshore oil drilling rights. In these auctions, firms try to gauge the potential oil reserves by doing seismic studies. Hendricks and Porter compare auctions for newly opened territory with sales of territory that adjoins developed areas. In the former, no bidder has a particular advantage. In the latter, the owner of an adjacent lease may have extra information. The auction outcomes differ dramatically in these two cases, with adjacent lease-holders profiting in a way that corresponds closely to predictions from the theory of asymmetric information. This study and subsequent research by Hendricks, Pinkse, and Porter (2003) provides some of the sharpest empirical support for equilibrium models of asymmetric information.

³Specifically, if bidder j has cost c_j of supplying a good and believes the lowest competing bid will follow a distribution G (with density g), its optimal bid b_j satisfies $b_j = c_j + [1 - G(b_j)] / g(b_j)$. This suggests an empirical strategy in which data from a sequence of auctions is used to estimate a statistical model of bids. The estimate then proxies for G (and g), permitting the researcher to associate each observed bid with an underlying cost of supplying that makes the bid profit-maximizing. The connection to product pricing is that a profit-maximizing firm sets its price p_j so that $p_j = c_j + Q(p_j) / Q'(p_j)$, where c_j is marginal cost and Q is the firm's residual demand taking as given the prices of the other firms. In the product market case, the estimated demand system allows the researcher to proxy for Q , and hence infer costs from prices.

A recent line of research on the advertising auctions conducted by Google and other Internet platforms illustrates the power of combining auction methods with large-scale datasets and field experiments. For instance, Varian (2009) uses data from Google to estimate the division of surplus in their sponsored search auctions by inferring values under an assumption of optimal bidding. In related work, Ostrovsky and Schwarz (2009) describe the design of a reserve price mechanism for Yahoo!'s sponsored search auctions. They use a calibrated model of equilibrium bidding to derive optimal reserve prices. They then implement reserve prices suggested by the model in a large-scale field experiment and track the resulting increase in platform revenue.

These papers at the intersection of industrial organization and auction theory highlight the value of the interplay between theory and research design. For example, in the Athey, Levin, and Seira (2008) paper, the authors use theory to translate the observed data (bids) into structural parameters (estimates of bidder values) that allow out-of-sample predictions. The variation in the data, however, is what allows a sharp test of the model. In the Ostrovsky and Schwarz (2009) paper, insights from optimal auction theory guided the modeling and calibration and the experimental design provides confirmation of the usefulness of the theory. Economic theory has an important role to play in empirical work, facilitating predictions "before the fact" or "outside the data."

Academic research on auction markets has also played a key role in regulatory policy, for example in the case of restructured wholesale electricity markets. These markets frequently involve electricity generation facilities bidding to supply power, and analyses along the lines described above have been influential in highlighting potential problems associated with the exercise of market power as well as the relationship between daily spot markets and long-term forward contracts between electricity generators and distributors. For example, Borenstein, Bushnell, and Wolak (2002) analyze data from the California wholesale electricity market and argue that the dramatic price spike during the summer of 2000 was due in large part to the exercise of market power, rather than an increase in production costs.

Determinants of Market Structure

A classic question dating back at least to Bain's work in the 1950s is how the set of firms in an industry and their relative capabilities affect competition, innovation, and other market outcomes. This question leads to asking what determines, over time, the set of firms present in an industry. In particular, what barriers to entry serve to keep markets more or less imperfectly competitive? Questions about market structure have been the focus of sustained research from a number of different directions.

One way to tackle the question of how market structure affects competition is by focusing on specific episodes of entry or exit from a market. For example, Goolsbee and Syverson (2008) study how prices are affected by the entry of Southwest Airlines into new routes. They find that prices fall sharply and the decline begins in advance of the actual date of entry. Prices begin to fall once Southwest

starts operating at both endpoints of a route, making entry into the route probable. In another example that focuses on strategic theories of entry deterrence, Ellison and Ellison (2007) look at the behavior of pharmaceutical firms just prior to patent expiration. They use variation in market size to identify settings where incumbent firms might profitably engage in entry deterrence strategies, and present evidence that these strategies are being used.

An alternative approach, pioneered by Bresnahan and Reiss (1991), also relies on variation in market size but combines this variation with an assumption that markets are in a state of long-run equilibrium. The basic idea, which also appears in Sutton (1991), is to think of variable profits as scaling with the size of the market.⁴ In the specific markets considered by Bresnahan and Reiss, namely service providers such as dentists, pharmacists, and plumbers in small isolated towns, the idea can be stated simply: If it takes 800 residents to support a single dentist and the entry of a second dentist does not result in lowered margins or affect entry costs, a town of 1,600 residents should be able to support two dentists. However, if the presence of a second dentist intensifies competition, it will take *more* than 1,600 residents to support two dentists. In this way, variation in market size can be used to draw inferences about the rate at which competition causes prices to fall toward unit cost.

Bresnahan and Reiss (1991) estimate the size threshold required to support different numbers of firms in a given industry. Their surprising finding is that compared to the population required to support a single firm, the *incremental* population required to support a second firm is much larger than that for the first firm, sometimes even double, but the incremental population required to support a third and a fourth firm is about the same as for the second. Interpreted through the lens of their theory, competition appears to kick in relatively quickly and lead to lower margins with just a few firms, but then does not ramp up with additional entrants.

The Bresnahan and Reiss (1991) work illustrates how even relatively sparse data, used creatively in conjunction with economic theory and clean research design, can be applied to tackle an important question—in their case, how competition varies with the number of firms in a market. This work has generated a substantial literature, although in a direction that has focused more heavily on the econometric methods than on the original questions. One might have expected researchers to have followed up by looking for other sorts of identifying variation (say in fixed costs or entry restrictions) or by gathering data on prices and costs (which is largely absent in the original work). Instead, attention has tended to focus on developing more general econometric methods that allow for firm heterogeneity or relax parametric restrictions, often applying the same Bresnahan and Reiss identifying assumptions in situations where they are less compelling. These extensions have led to a rich literature on identification and estimation methods

⁴ If variable profits scale with the size of the market, then $\pi(n) = Sq(p_n)(p_n - c)$ where p_n is the price charged if there are n competitors, c is the unit cost of production, $q(p)$ is the share of potential customers who purchase, and S is the market size. If there is a fixed cost F to being in the market, long-run equilibrium should exhibit the property that $\pi(n) > F > \pi(n + 1)$. The innovation of using long-run equilibrium conditions as a basis for estimation also appears in Berry (1992).

(Berry and Tamer, 2006), but one can argue that they haven't necessarily taken us much closer to understanding the original question of interest.

Industry Dynamics

Many questions about market structure are most naturally viewed in a dynamic context. For example, one may be interested in whether new industries follow a common "life cycle" of entry and consolidation, or how firm and industry growth patterns vary with market characteristics, or whether mergers are likely to be followed quickly by new entry, or how industries adapt to booms and recessions. A dynamic perspective can also temper conventional intuition on standard problems. For example, higher market concentration can reduce static welfare due to less competition and higher prices, but the prospect of gaining market power can provide strong incentives for innovation with consequent welfare benefits.

The literature that addresses these types of questions can be separated roughly into two strands. One strand has looked across industries in search of robust patterns of survivorship, turnover, and firm growth. One important take-away documented in this literature is that even within relatively narrow industries, there is a great deal of heterogeneity across establishments. This observation has proven very influential. It has played an important role in the development of theoretical models of industry dynamics (for example, Hopenhayn, 1992), and these models in turn have helped to touch off a vast amount of recent work in international trade on the role of firm heterogeneity (Melitz, 2003). In addition, the recognition that entry and exit patterns are systematically related to firm characteristics has been incorporated in the literature on industrial productivity, following the influential work of Olley and Pakes (1996).

A second strand of work on industry dynamics has focused on models of dynamic equilibrium in individual industries, often adopting the framework proposed by Ericson and Pakes (1995). For example, Benkard (2004) applies such a model to the commercial aircraft industry, where he documents dramatic cost effects due to learning-by-doing. Benkard (2004) calibrates a dynamic oligopoly model using industry data and uses the quantitative model to illustrate the (dynamic) pricing incentives. One insight is that equilibrium can involve prices below marginal cost as a way to speed up production and reduce future costs; and the prospect of market power is what provides an incentive to get down the learning curve. The effects get passed on to consumers in the form of lower prices, both early in the product life cycle (to speed up learning) and later on (due to reduced costs). More recent work in this area has suggested empirical strategies that allow estimation of sunk costs, learning, or adjustment frictions, using panel data on firms or local markets (Aguirregabiria and Mira, 2007; Bajari, Benkard, and Levin, 2007; Pakes, Ostrovsky, and Berry, 2007).

One difficulty in estimating and working with these types of dynamic industry models is that the information available in the data often pales in comparison to the ambitious questions that are being asked, forcing the researcher to fill in the gaps with strong modeling assumptions. Our own view is that such work is best viewed as

a quantitative theory exercise. Its primary role is to shed light on certain aspects of dynamic competition in the context of a particular model with reasonably chosen parameter values. Is this a problem? We don't think so. While results from such exercises should be taken with the appropriate caution, it seems hard to imagine a clean research design that, say, would nail the long-run effect on innovation attributable to a change in the patent system or a new research and development subsidy. When a single data point is the entire time series of an industry, opportunities to utilize quasi-experimental variation are rare, but this does not mean economists should give up on attempts to investigate important questions about, say, the trade-off between market power and innovation incentives.

Stepping Back and Looking Forward

Thirty years ago, empirical work in industrial organization was perhaps broadly similar to other applied fields. At about the same time that industrial organization economists were running cross-industry regressions and coming to terms with the problems of endogeneity, omitted variables, and reverse causality, labor economists, for example, were running wage regressions that faced a similar set of issues. Since then, however, empirical work in industrial organization has evolved dramatically, and in a distinctive direction: toward analyses of individual industries where one can obtain cleaner measurement and identification, and toward studies that frame their empirical analysis in terms of an economic theory of the relevant industry or a set of competing theories.

In an essay that appears in this issue, Angrist and Pischke describe a parallel development in applied microeconomics toward analyses that use randomized experiments or “quasi-experiments” to identify causal effects. In the course of doing this, they launch what strikes us as a somewhat misguided attack on empirical research in industrial organization. In part to clear up potential confusion, and in part because we have our own views about fruitful directions for research in industrial organization, it seems useful to discuss how their arguments relate to the type of work we have discussed above.

One issue relates to what constitutes acceptable identifying variation in a given study. Here, there is really no disagreement about the ideal. Every researcher would like to first define an object of interest and then design the perfect experiment to measure it. But in the absence of this ideal, researchers generally face trade-offs. A common trade-off is between how clean a measurement is obtained versus how well the measured object proxies for the original object of interest.

Suppose a researcher is interested in consumer demand for cereal, and ideally would like to know the n -by- n matrix of cross-price elasticities. As we described above, a common approach in industrial organization has been to control for possible shifts in demand, and then to rely on an assumption that the residual correlation in prices across cities is due to changes in costs (for example, Nevo, 2001). An alternative might be to look for specific episodes where particular prices

were shifted for explicable but plausibly exogenous reasons, say a week when the Froot Loops pricing algorithm malfunctioned and there was an unintended sale, and use these episodes to estimate demand elasticities with respect to the price of Froot Loops. The researcher might then extrapolate by assuming that a change in the price of Frosted Flakes, or perhaps any cereal, would have analogous effects. In this context, neither approach is the ideal. The former relies on a potentially questionable identification assumption, but frames the empirical exercise directly in terms of the object of interest. The latter may provide a more compelling measurement of a few entries in the matrix, but getting to the object of interest involves a questionable extrapolation.

One problem with attempts to evaluate entire research approaches in the abstract (such as Angrist and Pischke's essay) is that they paint too simple a picture. The resolution to the type of trade-off described above almost certainly depends on the research question that is being asked, the data that is available to answer it, and the degree to which economic theory provides a compelling rationale for making assumptions about relationships in the data. Our own view about industrial organization is that there is room for a variety of approaches, even for tackling the same question. Indeed, because any research can only come so close to the requisite answer, careful work from different angles is often complementary.

A second issue about how industrial organization has evolved relates to the use of economic theory in empirical research. It seems safe to assume that professional economists, or at least the vast majority of them, believe that economic theory provides a powerful lens for thinking about the world. So what is striking about Angrist and Pischke's picture of empirical research is that it seems to involve very little role for economic theory in thinking about or analyzing data. Instead, and in contrast to many of the studies described in this essay, they favor measurement strategies that are uncontaminated by restrictions that arise from an economic model. One of their justifications is transparency; they seem to conflate the use of economic theory with complex modeling that obscures the data. This strikes us as a false equivalence, however, in the sense that one can have a perfectly clear exposition of a model derived from economic theory and a perfectly obscure exposition of a linear regression.

From our perspective, it seems more natural to start with a research question and then ask to what extent economic theory helps to shed light on the problem. For instance, Angrist and Pischke focus on the effect of class size on student learning outcomes. Standard economic theory does not have a great deal to say about student behavior in third-grade classrooms. So while one could start with an equilibrium model of student learning and use it to structure empirical work, there is good reason to adopt a more statistical approach, observing that there are thousands of third-grade classrooms engaged in roughly parallel activities and many opportunities to find attractive variation in class size to assess its effect on learning.

For industrial organization research, however, this paradigm is frequently not the right one. Industrial organization is largely about the operation of firms and markets, where economic theory has a lot to say and, when appropriately used,

generally serves to *clarify* rather than to complicate one's understanding of a market. Moreover, the interesting question in many studies is not just a causal effect *per se*, but understanding the mechanisms at work. As we have emphasized throughout this essay, markets differ greatly, and importing particular numbers (about demand elasticities, production costs, or policy effects) across markets often does not seem compelling. Instead, if one hopes to generalize, it is often more appealing to view empirical research as building up support for principles of strategic interaction or market functioning that are broadly applicable across industries.

The particular example chosen by Angrist and Pischke to critique empirical work in industrial organization—the evaluation of mergers—illustrates some of these points. As we discussed above, it is common in assessing a proposed merger to frame the problem in terms of a theoretical model of industry competition. Researchers have spent considerable time developing econometric tools to quantify the potential effects in the context of such models. Angrist and Pischke criticize this work and dismiss it as needlessly indirect. Instead, they ask why there hasn't been more retrospective analysis of past mergers. At one level, their point is a good one—why not more retrospective analysis of past mergers? At another level, it entirely misses the point. Do they seriously think that if the Department of Justice had to review a merger of Microsoft and Yahoo! it should rely on the price effect of past airline or office supply company mergers, or better yet the subset that resulted from chance meetings of chief executive officers or from lunar eclipses? It seems far more useful to lay out a clear conceptual framework to think through the potential effects, adding the best available evidence in a sensible way.

The bottom line, however, is that the use of economic theory and the search for compelling sources of identifying variation are not enemies. Indeed, we hope to have conveyed that the applied work we often find most exciting relies on careful measurement based on data with good underlying variation, but then continues by framing the empirical exercise in terms of a coherent economic model. The model can then provide a way to think about the operation of the industry and potentially to draw conclusions about policy or general principles.

To the extent that we have a concern about the current state of industrial organization research, it is that there is insufficient emphasis on these kinds of applications, relative to, say, expanding the set of econometric methods. Of course, better methods are valuable, provided they eventually get used in compelling ways and do not become an end in themselves. If we return again to the demand estimation literature, it is possible that one reason researchers have been willing to tolerate less than ideal price variation is that in some cases the main contribution is not the estimated price elasticities *per se* but the econometric method, which can be applied more broadly. While this is not terribly objectionable, it is important that the field at large strikes a balance between building tools and using them convincingly. Whether the field has tipped too far is debatable, but the fact that one might engage in a serious debate suggests some grounds for concern.

From a historical perspective, one way to understand the current balance relates to data limitations. Some of the most influential methodological advances

in industrial organization were, in fact, responses to problems of limited data. The entry model of Bresnahan and Reiss (1991) gets around missing data on prices and quantities. The demand estimation method introduced by Berry, Levinsohn, and Pakes (1995) has been so influential in part because it requires only market-level rather than individual-level data. We have tried to stress throughout this essay that the situation is changing rapidly. Virtually every major firm now collects vast amounts of data on their customers, their employees, and other aspects of their business. It is increasingly easy to collect data on prices and quantities, entry and exit, firm locations, and accounting information. The advent of vastly richer data may substitute for methods and encourage industrial organization economists to shift some of the focus toward applying existing methods rather than developing new ones.

There is a related concern—and to be fair, one that applies equally to the empirical strategies favored by Angrist and Pischke—that focusing on the elegance of the solution can lead one to gravitate toward less-important questions. For example, suppose we were to think about the research avenues one could pursue related to the Internet platform eBay. If we started with the view that it was a potential laboratory for applying elegant empirical auction methods, it would be natural to focus on narrower and narrower submarkets to isolate specific features of the auction format. While this approach could be useful, it might take one away from broader issues, such as why eBay as an institution has been so successful or how it competes with Amazon and other platforms for buyers and sellers. Indeed, economic researchers of all varieties should resist the temptation of grabbing a hammer and letting it pull them toward a limited set of nails.

A final and important issue for the future of industrial organization relates to the shift from cross-industry analysis to industry studies. In his post-mortem on the cross-industry literature in industrial organization, Schmalensee (1989) pointed out that it had not taught us much about how markets actually work. 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 on automobiles, airlines, electricity, and cement and concrete plants (which are not the same!). 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. It may be time to reclaim them.

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