

Regulation and Deregulation After 25 Years: Lessons Learned for Research in Industrial Organization

PAUL L. JOSKOW

*Elizabeth and James Killian Professor of Economics, MIT, Cambridge, MA 02139 USA.
E-mail: pjoskow@mit.edu*

Abstract. This paper is based on a keynote address given at the 2004 International Industrial Organization Conference in Chicago, April 2004. I draw selectively on the literature from the past 25 years on regulation/deregulation to provide important lessons about the attributes of good research in empirical industrial organization.

I. Introduction

When I first began doing research on issues related to government regulation of industry in the early 1970s, a significant slice of the economy was subject to some form of government price and/or entry regulation. The affected industries included (a) airlines, (b) trucking, (c) railroads, (d) electric power, (e) natural gas production, (f) pipeline transportation and distribution of natural gas, (g) crude oil and refined petroleum products, (h) cable television, (i) automobile insurance, (j) hospital services, and others. The share of these industries in GDP, while substantial, understates their potential impact on overall economic performance of the economy since they generally supplied important intermediate goods and services to downstream commercial and industrial users.

Beginning about 25 years ago legislative and regulatory actions began a process of deregulation, vertical and horizontal industry restructuring and regulatory reform that has now affected, to varying degrees, all of these industries. Many of these industries have been completely transformed during this period of time (e.g. airlines, trucking, and telecommunications). Others are in the midst of more lengthy transitions that involve

restructuring and deregulation of major industry segments and the application of new regulatory mechanisms to those segments that continue to be regulated (e.g. electricity).

Technological change, changes in supply and demand conditions and interest group politics have certainly played a central role in fostering these dramatic changes. However, economic research that has examined the effects of government regulation, deregulation and regulatory reform on prices, costs, innovation and consumer welfare has had significant (generally constructive) effects on these policy initiatives. The research focused on detailed analysis of specific industries rather than the superficial analyses of cross-sections of hundred of census “industries” that characterized much of the research in industrial organization 25 years ago. It integrated theoretical modeling with empirical analysis. It provided a stimulus to the development of new econometric techniques for estimating demand and cost functions. It pioneered the use of structural econometric models to measure the effects of changes in governance arrangements on costs, prices, income distribution and welfare. It made important contributions to and supported developments in corporate finance, political economy, mechanism design and incomplete contracts. Finally, it provided a good platform for studying more traditional issues in industrial organization as the structure, behavior and performance of the “deregulated” industries responded to the relaxation of regulatory constraints on prices and entry. Economic research on regulation, deregulation, and regulatory reform has played an important role in the transformation of research in industrial organization and related fields in economics.

The purpose of this paper *is not* to provide a comprehensive overview of the relevant literature about the causes and consequences of regulation, regulatory reform and

deregulation, something that can be found elsewhere (e.g. Joskow and Noll 1981, Joskow and Rose 1989, Peltzman and Winston 2000). Instead, I will draw very selectively on this literature to make a number of general observations about empirical research in industrial organization more generally. There are important lessons to learn from these developments about the attributes of good research in industrial organization generally and the role it may play in the public policy arena. These lessons seem to me to be especially important today as the field seems to be dividing between scholars committed to so-called “structural approaches” and those embracing a wider range of research methods, a division that is likely to hinder the advancement of knowledge of the core issues in industrial organization. Accordingly, I will take the opportunity provided to me by the Industrial Organization Society to reflect on the last 25+ years of research and policy related to government regulation and deregulation of industry and its potential implications for research in industrial organization more broadly today. I will be so bold as to identify what I think are the most important “lessons learned” from the research accomplishments during the last 25 years. I will draw primarily on research related to government regulatory policies that directly control prices for goods and services and restrict entry of new competitors, as well as reforms that have relaxed or removed these regulations. I will not discuss health, safety and environmental regulation, nor the growing body of work on privatization, though much of the research on deregulation outside of the United States is closely linked with supporting privatization initiatives.

By way of introduction to the field let me begin with what in some ways is the most important lesson that I have learned after over 30 years of research on regulated industries and other topics in industrial organization:

Lesson #1: *Good empirical research in industrial organization has been driven by a set of basic questions about the structure, behavior and performance of markets for goods and services and the institutional environment in which the producers and consumers in these industries interact. A good research program starts with one or more of basic questions about firms, consumers, market and non-market institutions, and the interactions between them.*

In the research on regulation, regulatory reform and deregulation produced over the last 25 years the primary questions have been reasonably constant and involve both *positive* and *normative* lines of inquiry or questions:

- a. How does regulation affect costs, prices, the rate and direction of innovation, the distribution of income and wealth, and overall welfare?
- b. How will/have *changes* in regulatory mechanisms, including deregulation, affect these indicia of behavior and performance?
- c. If we are going to regulate what are the best regulatory mechanisms and methods to choose *given* relevant information asymmetries, political constraints, and transactions costs?
- d. What changes in industry structure (e.g. vertical separation, unbundling) and regulatory mechanisms (e.g. access fees, market power mitigation mechanisms) are needed to facilitate the introduction of competition into one or more segments of incumbent regulated industries?

- e. Why is regulation introduced, why does it take the form that it does, and why does it change? How does interest group politics and political and legal institutions affect the answer to this question?

This list of questions is not meant to be exhaustive and, there are many other questions of interest in industrial organization more generally. However, by focusing attention on a reasonably well-defined set of questions, scholars doing theoretical and empirical research on regulated industries using a wide range of methods have been able to make significant contributions to moving knowledge in the field forward in a coherent and constructive fashion and facilitated the application of that knowledge in the public policy arena.

II. Industry Specific Studies

The pre-deregulation research done on the airline industry regarding the effects of Civil Aeronautics Board (CAB) price and entry regulation on it, provides a good example of how advances in our knowledge about industrial organization can proceed productively. Even looking back after 30 years, the research on airline regulation produced in the early 1970s is extremely impressive. I have in mind here studies by Douglas and Miller (1974, 1975), Eads (1975), and DeVany (1975),¹ all published around 1975 and all developing similar theoretical models and related empirical analysis.² I will focus here on Douglas and Miller's pioneering research.

The purpose of Douglas and Miller's research was not to assess or promote "deregulation," though it was eventually used by both economists and policymakers for

¹ See also Jordon (1970) and Keeler (1972).

² See also the volume edited by Almarin Phillips at about the same time (Phillips 1975).

that purpose. Rather it provides a theoretical model and empirical analysis to assess the welfare consequences of the regulatory mechanism used by the CAB to set airfares and its policies to restrict entry. Douglas and Miller's work was motivated by the goal of identifying some potential improvements in the CAB's price setting formula to improve the performance of the airline industry. It only later turned out to be the intellectual stimulus for deregulation. Douglas and Miller's analysis relies on the integration of a stochastic model of passenger demand, a model of airline costs, a model of CAB pricing, and a behavioral model that allows for quality competition in the presence of fixed regulated prices, a fixed number of competitors on each route, combined with opportunities for those competitors to compete based on service "quality." The central engine of their analysis is a non-cooperative oligopoly model where each firm takes the regulated CAB price as given and competes with a fixed number of other firms certified to offer service on the same route by varying the "quality" of service. Quality here is measured (roughly) as the expected difference in time between a traveler's actual departure time and her preferred departure time multiplied by the value of the traveler's time.

The model captures important aspects of a CAB regulation. The CAB set the fares that airlines could charge on particular city-pair routes based on a relatively simple fare formula of the general form

$$FARE_i = F + \alpha_i DISTANCE_i$$

where $FARE_i$ is the regulated fare for city-pair i and $DISTANCE_i$ is the distance between the cities that make up city-pair i . In addition, the CAB determined the number of airlines (N) that were certified to fly planes on each route and it rarely approved requests

by new firms to enter a route. However, the CAB did not regulate the number of flights that a certified airline could fly on a route or the size of the planes and total seats they could offer for sale on each route once an airline was certified to serve it.

By offering additional flights on a route, competing airlines perceive that they would attract more passengers due to the associated increase in quality; since passengers value flights more highly the closer they are scheduled to their preferred departure time, the more flights an airline offers, the closer will be actual and preferred departure time and the more likely a seat will be available on a convenient flight. Adding more flights was assumed, based on empirical evidence, to have little effect on aggregate passenger demand to travel on the route (recall, regulated prices are fixed here as well), so the primary effect of adding flights/seats was to lead to more empty seats on the route. Douglas and Miller's imperfect quality competition model led to the result that the larger the number of competitors that are certified to serve a route, the more intense this kind of non-price competition would be. That is, the route's load factor, the number of passengers on the route divided by the number of seats supplied, denoted $LOAD_i$, will be larger when there are more competitors on that route. Moreover, since the CAB's then-prevailing fare setting formula failed to take into account some important factors that influence the cost of transporting a passenger from one city to another, price/cost margins vary from route to route for any given value of $DISTANCE_i$ and $LOAD_i$. As a result, for any given N competitors on a route, competition is hypothesized to be more intense on routes where the implicit regulated price/cost margins are higher. This leads to higher quality and lower load factors for any given value of N , other things equal.

This theoretical model leads to a set of comparative statics results regarding the relationships between load factors, the number of competitors on a route, and other route-specific attributes affecting costs, a related set of testable hypotheses about price-constrained airline competition.³ Estimates of the parameters of this model can be integrated with complementary demand and cost models to provide a framework for evaluating the welfare consequences of CAB regulation compared to various norms and alternatives. Douglas and Miller use cross-sectional data on city-pair routes to test whether on how average load factors vary with the number of competitors and route attributes and measure the associated elasticities. As predicted, load factors decline as the number of competitors increase and as price/cost margins (holding L_i constant) increase across routes. Douglas and Miller integrate these results with stochastic models of passengers' preferred departure times, measures of airline costs, and alternative assumptions about the costs incurred by passengers as their actual departure times differ from their preferred departure times, and then proceed to perform a set of simulations to measure the change in welfare that would result from moving from the industry equilibria that emerge under then-prevailing CAB regulation to an "efficient" outcome.

In my view, this early work by Douglas and Miller provides an outstanding model of fine research in industrial organization, in this case motivated by an interest in understanding the effects of government regulation. Development of simple theoretical models whose structure is well grounded in the important attributes of the industry; the use of these models to generate testable hypothesis about the effects of CAB regulatory practice and competition on market equilibria; clever use of available data to test these hypothesis; the use of these empirical relationships along with models of consumer

³ See also Schmalensee (1977).

demand and quality valuation and supplier costs to simulate the welfare effects of changes in CAB regulatory practice. This work and the related research on airlines that was produced at that time supports a number of “lessons learned” that I have tried to apply in my own research and to impress upon my students.

Lesson #2: *Industry-specific studies have provided important insights into issues that are both specific to the industry being studied as well as more general insights into more general issues of concern in industrial organization and the methods used to analyze them. Research on regulated industries starting in the 1970s led the transformation of industrial organization in this and other important dimensions.*

This second lesson is unlikely to be controversial today. However, this was not the case when the pioneering work on airline regulation was published in the early 1970s. The primary focus of empirical research in industrial organization at that time and the previous decade (there was little in the way of theoretical research at that time) was the application of cross-sectional econometric analyses of the attributes of hundreds of four-digit Census “industries” to apply and draw inference about the so-called Structure-Conduct-Performance (S-C-P) paradigm. A typical study involved multivariate regressions of some measure of prices or price-cost margins⁴ (often confusing average variable cost with marginal cost) for a large number of census industries as the dependent variable and various measures of market structure, such as the four-firm concentration ratio for domestic producers on a national basis, capital intensity, advertising intensity and other variables for each census industry as independent variables (e.g. Weiss 1989). This work often reflected as well an implicit normative assumption that any departure

⁴ Or R&D spending, advertising expenditure, etc.

from perfect competition was problematic, that vertical integration, long-term contracts and other non-standard vertical relationships were generally evil. It also led to policy prescriptions that assumed that a reasonable goal of antitrust policy was to ensure that all industries were perfectly competitive and that this could best be achieved by restricting horizontal mergers, adopting industry deconcentration laws, and placing severe restrictions on vertical integration, long term supplier contracts and other vertical restraints which were viewed as being inconsistent with competition (Joskow 2004).⁵

This is not the place to critique this research, but let me just indicate that I view it as a dark period for empirical research and policy in industrial organization. I believe that the infatuation with multi-industry cross-sectional regression analyses was in part a reaction to the earlier focus of industrial organization research on industry specific case studies. It also reflected an infatuation with the new technology for running multivariate regressions cheaply and quickly and a desire to “modernize” industrial organization in the context of the rapidly increasing use of econometric methods in other applied fields during the 1960s. Industry-specific case studies were often viewed at that time as providing analyses that could not be generalized and which failed to exploit the availability of technology that made it easy to do multivariate regressions with large data sets. So S-C-P research at that time largely rejected earlier case study work that endeavored to learn a lot about the detailed attributes of specific industries in favor of “general” analysis of data drawn from many industries about which the analyst typically knew nothing. This in turn embodied the misguided assumption that the estimated

⁵ These views were not, of course, universal. See for example Demsetz (1973). Those were the days when there were big differences between the “Chicago” view and the “Harvard-Berkeley” views on antitrust policy.

coefficients from these “reduced form” regression equations could be readily used to do policy simulations.

The research on regulated industries that began in the 1970s represented the first efforts to take the best of the older tradition of industry specific research and to integrate it with analytical theoretical models and evolving econometric methods both to understand the behavior and performance of specific industries and, through the accumulation of knowledge about many specific industries, to develop more general insights about issues in industrial organization more broadly. Industry-specific studies were facilitated in regulated industries because (a) research was often stimulated by policy questions arising in particular industries, and (b) at least in the United States, regulation typically implied as well the collection and public availability of detailed firm-specific information about prices, costs, demand, capital structure, profitability, and other data that were not available (or not as easily available as Census of Manufacturers data) for many other industries. So, it is not a surprise that a great deal of the pioneering work on cost function estimation (e.g. Christenson and Green 1976), demand function estimation (e.g. Dubin and McFadden 1984, Hausman 1985, Taylor 1975), the diffusion of innovations (e.g. Rose and Joskow 1990), the cost of capital (e.g. Miller and Modigliani 1966), and other topics, relied on data drawn from regulated industries, often the electric power sector. These observations lead to another lesson learned.

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.*

The value of industry-specific studies is now well-understood and reflected in much of the contemporary research in industrial organization. This is a good development. Multi-industry-studies are now done much more carefully than was once the case as well. This is also a good thing. However, simply acquiring what looks like an interesting data set for a specific industry is unlikely to lead to good industrial organization research without a more detailed exploration of the industry and “quality” of the data. The “have data and methods, will travel” approach to research is unlikely to provide useful insights into important industrial organization questions and is quite likely to lead to incorrect or misleading conclusions about these questions. In addition to interesting data and the ability to have an up-to-date toolkit of analytical methods available for application to it, it is also very important to understand the basic technical and institutional attributes of the industry being studied: How is it organized horizontally and vertically? Are the markets localized geographically? What kinds of vertical relationships exist? What are the costs of transportation and storage? What product attributes appear to be important factors influencing consumer demand? Are firms single or multi-product enterprises? How are complementary products marketed? What are the attributes of production technologies available to suppliers and how are they changing? What kinds of government regulations affect the way prices are determined, affect the costs of production and the way firms compete?

So, it would be silly to study price formation in the milk industry without also understanding how state and federal regulations affect milk prices, recognizing the existence of government restrictions on who and where milk can be distributed, government support programs and the existence of cooperative agreements among milk

producers. Nor would a study of competition in, say, the soft-drink industry, be particularly useful without reflecting a good understanding of the institutions governing the retail distribution of soft-drinks, state laws governing franchising, the nature of contractual arrangements between syrup suppliers, bottlers, distributors and retailers, and other factors.

An important part of becoming educated about an industry also involves understanding how the data that are to be used for analysis are generated, how they relate to the industry attributes that are being studied, and what their strengths and weaknesses are likely to be. I am continually surprised with the frequency with which researchers rely on data sets obtained from third-parties without having much of an understanding of how the data were generated or how they can be applied given any underlying imperfections of what they actually measure. For example, data on coal transactions prices reported by various agencies combine prices under long term contracts with prices from spot market sales. The failure to understand that the price series have been generated in this way can lead to a misleading picture of how coal prices change with changing supply and demand conditions (e.g. Joskow 1988). Price data drawn from trade publications on various commodities may or may not reflect actual transactions and may or may not be representative of transactions generally, and based on recent experience with electricity and gas price series, may even be manipulated for private gain. While I am not of the view that accounting cost data are useless,⁶ I do believe that when accounting data are used it is important to understand the accounting system that generated them. For example, accounting data drawn from regulatory accounting

⁶ Indeed, well known issues with accounting data have in my view too often become an excuse for avoiding the task of collecting any cost data at all, in favor of making unverified assumptions about the functional form of potentially observable but unobserved cost information. This is unfortunate.

statements can and have been used to measure the effectiveness of regulatory mechanisms in terms of profitability or “rent extraction.” (Greene and Smiley 1984, Smiley and Greene 1983). Similar data can and have been misused in comparing costs across regulated firms as a consequence of a failure to understand the underlying capital cost accounting principles. A good understanding of regulatory accounting systems and how they are integrated with regulatory mechanisms for setting prices over time are necessary to use such data effectively.

Douglas and Miller’s work on airlines was influential both from an academic perspective and from a policy perspective, in part, because the authors made the effort thoroughly to educate themselves about the airline industry, its cost structure, demand patterns, how it was regulated, and how firms behaved and used this knowledge effectively to develop industry-relevant theoretical models and supporting empirical analyses. This is true of the best research examining other regulated industries that followed D&M. Does one have to become an “expert” on every industry one studies? Probably not, but serious efforts should be made to understand the important attributes of the industry being studied, how the data being used were generated and what the strengths and weaknesses of the data are likely to be. Actually talking to people who know the industry well (e.g. trade association staff, industry economists, securities analysts) is always a good idea.

III. Structural Models

For reasons that remain a bit of a mystery to me, over the last several years divisions have emerged between industrial organization scholars who focus on “structural models” and

those who rely more (or as well) on what I suppose would be called “non-structural” approaches. The origins of this division are a mystery to me for several reasons. First, the application of structural models in industrial organization is not new; in particular, structural approaches have been used in research on regulated industries for many years. Second, the kind of modeling approach that is likely to be most effective necessarily depends on the questions one is seeking to answer; questions first, methods second! Third, the most effective approach to addressing these questions necessarily is also constrained by the data that are available at a point in time. Finally, and perhaps most importantly, I have learned a lot about important industrial organization issues from scholars working with structural models, those working with reduced form models, those working with experimental techniques and those producing comprehensive case studies that rely on no formal modeling or sophisticated econometric analysis at all.

This kind of division has previously infected other fields in economics and, in my view, these fields are weaker as a consequence of intolerance for diverse approaches to answering fundamental questions. Choosing what research is worth reading and what is not based on methods used rather than questions asked is an unfortunate development. I am drawn to research that is motivated by interesting questions, that uses appropriate data, theoretical models and empirical techniques to shed convincing light on these questions. In addition, I find empirical results to be most compelling when the research demonstrates extensive knowledge of the industry or industries being analyzed and the data being used, regardless of the particular methodological approach that happens to be employed.

Structural models of various types of varying levels of complexity have been used particularly extensively in analyzing the effects of regulation and deregulation on prices, costs, entry and innovation in the airline, surface freight transportation, and energy sectors. In addition to the work on airlines that I have already discussed, Friedlander and Spady (1981) (on trucks and trains), Levin (1978, 1981) (on trucks and trains), MacAvoy and Pindyck (1973) (on natural gas), Hausman (1997, 1999) (on telecommunications) and many others have produced pioneering work on the effects of regulation and regulatory reform on these other sectors using structural econometric models and integrated econometric and engineering models. This leads me to another lesson learned.

Lesson #4: *The use of structural models in industrial organization has been around for a long time. Structural models have been used most effectively by practitioners whose work is consistent with the first three lessons discussed above.*

Most good research in economics starts with a structural model of some kind. The papers cited above all seek to estimate the parameters of demand functions, the parameters of cost functions, make assumptions about the nature of behavioral interactions between incumbents and (in some cases) potential entrants. Now, I suppose that one can then argue about how “structural” one needs to be. And surely, better data and improved analytical methods now make it possible recover, for example, a richer set of parameters that characterize consumer demand and that these in turn can be usefully applied to problems that could not be previously addressed. However, many modern applications of structural methods require making assumptions that knowledgeable students of the industries from which the data have been drawn find questionable. So, for

example, using retail scanner data to infer elasticities at the manufacturing level requires a set of (often unstated) assumptions about the nature of vertical relationships between manufacturers, wholesalers and retailers which are difficult to justify. Ignoring entry and exit of new firms and/or products may be convenient for specifying a structural demand model, but if entry and exit have important effects on incumbent supplier and consumer behavior and the distribution of products available to consumers failing to account for them is likely to undermine the credibility of the structural parameters being estimated and any welfare simulations and conclusions that rely upon them. Most research is burdened by less than ideal data and models that necessarily reflect simplified characterizations of reality. Accordingly, there is much to learn from a wide range of approaches and there is much to lose if we pick and choose how we learn about questions in industrial organization primarily based on the methods that are being applied.

Happily, the research community working on regulated industries over the last 25 years has not been plagued by these methodological debates and divisions. Significant progress was made because the community encouraged diverse approaches and, indeed, embraced scholars from political science, law and history who looked at the issues in different ways and brought new methods to address them. The field has been enriched by this intellectual cross-fertilization. The industrial organization field more broadly would benefit from following a similar course.

IV. Natural Experiments

A great deal of research in industrial organization is motivated by or purports to have useful implications for public policy—antitrust policy, regulatory policy, patent and

copyright policies, trade policies, etc. Research in industrial organization certainly need not be motivated by public policy issues or even have direct implications for public policy. Indeed, I think that the mainstream cross-section regression industry that characterized much of the empirical work in industrial organization in the 1960s and 1970s was too heavily motivated by antitrust policy issues and probably contributed to bad policies in the areas of mergers, vertical integration and vertical contractual arrangements. Nevertheless, public policy issues continue to be associated with a significant fraction of the research in industrial organization. Accordingly, it is natural to ask whether, how and why this research has affected public policy in these areas.

Douglas and Miller's work was motivated by policy issues arising from the effects of the CAB's pricing formula and entry rules on the behavior of regulated firms. As I indicated earlier, their work was focused not on deregulation but on improving the regulatory mechanism used by the CAB at that time to set fares. Was this work on the effects of CAB regulation of the airline industry influential in the policy arena that led to airline deregulation? Clearly it was. The CAB ultimately changed the pricing formula to better reflect route-specific costs and to limit some dimensions of non-price competition. More importantly, however, the work made the point that regulation led to significant inefficiencies and that "competition mattered" even when prices were controlled by regulation. This in turn led to more serious inquiry into the question "why regulate airline prices as all?" This and related research played an important role in the Senate hearings on airline regulation initiated by Senator Edward Kennedy in 1975 as well as in a set of Senate hearings on deregulation and regulatory reform more broadly sponsored by Senator Abraham Ribicoff in 1978. These hearings involved the development and

presentation of a lot of substantive analyses of government regulatory policies and engaged a diverse group of scholars in the process.

However, I think that it is fair to say that if the only empirical evidence available had been the analyses by Douglas and Miller and others which were soon being used to simulate the effects of deregulation on prices and welfare, airline deregulation would have been a tough sell politically. Policymakers were more heavily influenced by the results from what was regarded as a natural experiment. In the early 1970s, prices and entry of interstate airlines were regulated by the CAB as described above. However, the states retained regulatory authority over intra-state airlines. Texas and California (large states from both geographic and population perspectives) had intra-state commercial airlines that were exempt from CAB regulation. These states had decided not to regulate intra-state airline prices or entry (aside from safety consideration). Thus, Texas and California were viewed as a “natural experiment” with deregulation. Policymakers were able to compare airfares between CAB regulated city-pairs and comparable fares in arguably unregulated intra-state routes in Texas and California.

The comparisons between fares on comparable regulated and unregulated city-pairs indicated clearly that fares were significantly lower in “deregulated” markets than in comparable regulated markets. Thus, relatively simple to understand comparisons of outcomes under different governance arrangements, helped to confirm the credibility of the more sophisticated analyses that led to the conclusion that deregulation would lead to lower airfares. Similar types of “natural experiment-like” comparisons were made to better understand the effects of trucking regulation (by comparing costs for regulated and exempt carriers and prices for regulated and exempt commodities) and natural gas

pipeline regulation (regulated interstate natural gas transactions compared to unregulated intra-state natural gas transactions in Texas). More recently, government initiatives in many countries to liberalize electricity, natural gas, and telephone industries were heavily influenced by favorable experience with liberalization in other countries. And, of course, it can go in the other direction as well. The California electricity crisis of 2000-2001 has slowed down or even reversed liberalization initiatives in many U.S. states and some other countries.

Most people (I am not referring to economists in particular here) are drawn to what appear to be natural experiments because they seem to provide more credible evidence for the likely effects of changes in governance arrangements than do simulations of a model whose parameters may simply be difficult to understand. Of course, these simple comparisons can also be quite misleading since there may be significant differences in important economic and institutional variables that make “natural experiment like” inferences problematic. The World Bank and related international organizations have repeatedly fallen into this trap as they have tried to apply lessons learned from institutional changes in highly developed countries to developing countries with very different institutional infrastructures. These observations, lead to a fifth lesson.

Lesson #5: *Natural or near-natural experiments that produce cross-section and time series variations in the nature or intensity of regulatory mechanisms—can and have provided very useful opportunities to measure the effects of regulation, the effects of variations in the structure of regulatory mechanisms and the impacts of deregulation*

initiatives. However ensuring that one really has a meaningful natural experiment is always a challenge.

The father of the natural experiment approach to evaluating the impact of regulation, regulatory reform, and deregulation was George Stigler. His 1962 paper with Claire Friedland “What Can Regulators Regulate?” (Stigler and Friedland 1962) is very important for at least two reasons. First, this is perhaps the first academic paper on regulation to suggest that one way to assess regulatory effects is to compare the performance of firms in states subject to commission regulation with the performance of comparable firms in states without state commission regulation—turning the U.S. federal system of diverse state regulation into a laboratory for studying variations in government regulation. Stigler and Friedland did this by comparing electricity prices in states with and without commission regulation in the first three decades of the 20th century, controlling for differences in production costs and demand attributes across states. Second, this paper began the process of transforming the ethos of students of regulation from one that presumed that government regulated in the public interest to one that assumed that government regulators acted in response to interest group pressures and where often not very effective in achieving widely articulated public interest goals (Stigler 1971, Posner 1974). Stigler pursued that latter line of work much further in subsequent work and in many ways his work on the political economy of regulation helped to create the modern field of “rational choice” political economy as well (Noll 1989).

The basic approach used by Stigler and Friedland was to estimate hedonic price equations for the price of electricity for a panel of states in several different years

beginning in 1907 and ending in the early 1930s. Regulated electricity prices are driven by input prices (prices for fuel and the availability of hydroelectric resources), attributes of the geographic area in which electricity is supplied (e.g. population density, weather) which effects the ability of the supplier to exploit economies of scale associated with the distribution and (at that time anyway) the generation of electricity, and the attributes of the customers served (residential, commercial and industrial and their associated demands for electricity). Stigler and Friedland included measures of these variables in their hedonic price equations. They allowed the price equation to shift up or down by including a dummy variable indicating whether electricity prices were regulated by state commission or were not regulated by state commission—states where electricity prices were characterized as being “unregulated.” The most notable result from this analysis was that the presence of state commission regulation had a very small negative and generally statistically insignificant effect on electricity prices. This suggested that regulation was not an effective governance arrangement for controlling electricity prices and natural raised the question “why bother?”

There were some significant flaws in Stigler and Friedland (1962). As is now widely known (Peltzman 1993), the dummy variable in the primary hedonic price equation was coded as a 10 rather than as a 1. The famous result that regulation had only a very small constraining effect on electricity prices (less than two percent) now turned into a large effect of reducing prices by roughly 25 percent, consistent with results from simple structural models published years later (Joskow and Rose 1989).⁷ Moreover, the

⁷ A simple structural model in the presence of a regulated monopoly involves estimating a demand function for electricity, which with good price data and appliance choice information can be quite granular, estimating a cost function given abundant public firm-specific cost data and then simulating what

work proceeded under the assumption that the variable indicating the presence or absence state commission regulation represented the choice between regulation and no regulation. This assumption was not generally correct. A review of the historical evolution of state commission regulation in the United States makes it fairly clear that it was a *substitute* for municipal franchise regulation and franchise contracts, not the introduction of price and entry regulation where none existed at all. Indeed, it was a substitute promoted by John R. Commons and other contemporary economists. Finally, one might question the implicit assumption that the incidence of regulation was exogenous, perhaps calling for a model of why states chose to shift to commission regulation.

These observations reinforce the first three lessons discussed earlier. However, despite its shortcoming, this is an extremely important and influential paper because of the questions it asked and the questions it subsequently stimulated many other scholars to ask in many other regulated (and state-owned) industries (Joskow and Rose 1989), the skepticism it brings to questions about the motivations for and the effects of regulation, and its introduction of systematic empirical methods to answer them. This leads to my sixth lesson learned:

Lesson #6: *Research that addresses interesting problems by adopting new and interesting ways of thinking about them can easily compensate for the absence of rigorous theoretical models and reliance on very simple empirical analysis and have important impacts on the advancement of knowledge in industrial organization. Good new ideas accompanied by excellent analytical models and empirical analysis are certainly better. But the quest for the perfect can be the enemy of intellectual progress.*

unregulated retail prices would look like (Smiley and Greene 1983, Greene and Smiley 1984). The analysis can be expanded to include the effects of potential entry.

One must take care in applying this lesson. It is really hard to come up with exciting new questions and new ways of looking at new and well-established questions. So, we can only expect a few papers to meet this high standard. At the same time, I sometimes wonder if important papers by Stigler on regulation and interest group politics (Stigler 1971), oligopoly (Stigler 1964), and information economics (Stigler 1961) and the “fragments of evidence” that often accompanied them, or Coase’s papers on the nature of the firm (Coase 1937), property rights (Coase 1960), and Federal Communications Commission regulation of the radio spectrum (Coase 1959), or even Williamson’s early papers on organizations (Williamson 1971, 1979), could pass through the refereeing process and get published in the same journals today. Advancements in economic knowledge have been driven by new insights, whether or not that are accompanied or revealed by the application of sophisticated methods.

V. Deregulation and Industrial Organization

For many years the study of “regulated industries” was viewed as a sort of cousin to the broader field of industrial organization. While I have always believed that this characterization took an extremely narrow view of the field of industrial organization,⁸ clearly the historical focus of research on regulated industries and the historical focus of most research in industrial organization on competition issues were quite different. However, the transformation of these important regulated industries as a consequence of restructuring, deregulation and regulatory reform has turned these industries into among

⁸ Also reflected in the then prevailing view that work on organizations and contracts were at best parts of “cousin” fields.

the best laboratories for understanding the behavior and performance of imperfectly competitive markets and many of the central questions in industrial organization. The extensive experience with deregulation in the last twenty-five years has created enormous opportunities both to re-examine what we thought we knew about the effects of regulation as well as to provide opportunities to examine the attributes of imperfectly competitive industries after they have been “shocked” by the relaxation or removal of price and entry constraints. The resulting transitions from one equilibrium to another (assuming that the airline industry for one has yet reached an equilibrium!) provides a lot of information that can be used effectively to examine many questions of broader interest in industrial organization. Moreover, good data often continue to be available for some period of time during the transition of these industries (though not as much as many of us would like).

Lesson #7: *“Deregulated” industries have become important targets of opportunity for studying problems in industrial organization more generally.*

The airline industry has been an especially important target of opportunity. As a result of work by Borenstein (1983, 1989), Berry (1992), Morrison and Winston (1995), Graham, Kaplan and Sibley (1983) and many others, airline markets have become perhaps the most studied imperfectly competitive markets around. The research provides new insights into entry and exit strategies, the effects of financial constraints on pricing behavior, impressive examples of price discrimination, and numerous interesting static and dynamic examples of firms exercising market power in a context other than pure monopoly.

The deregulation experience has also provided an opportunity to perform “before and after” tests of the models and empirical work used in the pre-deregulation era to predict what would happen post-deregulation. How well did we do in predicting changes in behavior and performance? It is surprising that there is not more of this kind of before and after testing done in industrial organization. For example, structural models are now routinely used to simulate the effects of mergers on prices and consumer welfare. Indeed, one of the great selling points of structural models is that they can be used more effectively than alternative approaches to simulate the effects of economic (e.g. input price) or policy changes (e.g. trade restrictions). Why isn’t there more work that looks at the post-merger experience to benchmark the performance of the models relied upon to simulate merger consequences? This kind of before and after analysis is routinely performed by students of industries subject to structural and regulatory reforms.

For example, looking back at what was predicted and what has actually happened in the airline industry as a consequence of deregulation it is clear that the researchers of the 1970s got some things right and some things wrong. There are lessons to learn from both.

- a. The 1970s research predicted that quality would fall. It is hard to argue with that prediction. Load factors have risen from an average of about 52 percent to an average of 72 percent in 2003. And other dimensions of quality have declined as well. So, this fundamental prediction of Douglas and Miller and others appears to have been correct.
- b. The research predicted that prices would fall when CAB price controls were removed, and price competition and competitive entry drive prices down. Studies

suggest that *on average* prices are significantly lower than they would have been if CAB regulation had continued applying traditional regulatory mechanisms (Morrison and Winston 1995). They got this one right too.

- c. Many of the *ex ante* simulations of the consequences of airline deregulation assumed that the “competitive” world would be perfect competition or contestable. So, little consideration was given to how fares and entry incentives might vary with the structure of the market after deregulation. They got that one wrong (Borenstein 1989, Berry 1992).
- d. And while more intra-route variation in fares was predicted to be realized after deregulation, it was thought that it would reflect primarily “peak load pricing” considerations. There was little if any discussion of price discrimination. As Borenstein and Rose (1994) have shown, the extensive amount of price dispersion post-deregulation, is mostly price discrimination not peak load pricing based on variations in marginal costs.
- e. The predictions for the post deregulation era focused on individual city-pair “markets.” As a result it missed the importance of network effects, the growth of hub and spoke systems, and the associated implications for consumer welfare, costs, and competition (Borenstein 1989, Mayer and Sinai 2003). Problems caused by limited access to major airports by new entrants and by CRS systems controlled by major incumbents were also not foreseen.

The research on airline deregulation has changed the way we think of issues in microeconomics more broadly. Most microeconomics textbooks available in the 1970s and 1980s treated price discrimination as something associated with pure monopoly. We

now know that it is a much more pervasive phenomenon associated with imperfect competition. Network considerations now play a more central role in the study of many industries. So, too, does the interaction between competitive industry segments, supporting infrastructure that has natural monopoly attributes, and the associated effects of vertical integration and potential foreclosure strategies. Airline industry financial problems have also stimulated more research on the interactions between the way firms are financed and their behavior (Busse 2002). Research on the post-deregulation airline industry helped to bring these issues to the fore.

The evolving of deregulated wholesale power markets, with organized auction markets for power and network support services, supported by regulated monopoly transmission and system operations infrastructures, are emerging as another fruitful area for studying mainstream issues in industrial organization. Work by Green and Newbery (1992), Wolfram (1998, 1999), Bornstein, Bushnell, and Wolak (2002), Joskow and Kahn (2002) and many others has made it possible for evolving wholesale power markets with different market structures, varying level of forward contractual cover and other important institutional differences to serve as a laboratory for understanding competitive interactions in a spatial competition setting. It is an area where it is feasible to measure costs rather than infer them from first order conditions and functional form assumptions. It provides opportunities to integrate the effects of environmental programs on firm behavior and market performance. It is one of the few areas of industrial organization where international experience is being used extensively to advance our understanding of issues of market organization, firm behavior and market performance.

One of the most interesting results that has emerged from the work on surface freight transportation (trucks and trains) since deregulation has been the importance of product and process innovation. Service quality improvements and service quality differentiation has been a key feature of the evolution of these transportation sectors post-deregulation. Recent work by Thomas Hubbard has shown how new technologies adopted by trucking firms have both served to improve service quality and to improve productivity and lower costs (Hubbard 2001, 2003). Regulation inhibited the diffusion of these kinds of technologies in a number of different ways

Work that I did with Nancy Rose and Dick Schmalensee on the diffusion, construction costs and operating performance of new electric generating technologies shows how regulation and ownership form can significantly affect these performance indicators (Joskow and Rose 1985, Rose and Joskow 1990, Joskow and Schmalensee 1987). Despite all of the controversies about electricity “deregulation,” the adoption and rapid diffusion of efficient CCGT generating technology was stimulated by allowing competitive entry into electricity generation. And when merchant generating firms experience cost overruns when they build new generating plants, as has been the case in Boston, they are the residual claimants on the impacts of these overruns on their profits not me as an electricity consumer. The effects of regulation and deregulation on the speed of introduction of new services and technologies in telecommunications also make it clear that these dynamic considerations are extremely important from a social welfare perspective (Hausman 1997, Crandall and Hausman 2000). These results lead to an eighth lesson learned.

Lesson #8: *The results of research on the performance of industries after price and entry regulations are removed/relaxed suggests that research in industrial organization and related public policy prescription has placed too much emphasis on static efficiency gains or losses and not enough emphasis on the factors influencing the rate and direction of product and process innovation which are likely to have much larger consumer welfare effects.*

The word “deregulation” is typically used loosely to refer to a package of reforms that involve industry restructuring, deregulation of prices and entry in one or more formerly regulated segments, and regulatory reforms applied to residual regulated segments --- electric power networks, natural gas transportation networks, and local telecom networks --- that have important implications for the performance of the competitive segments that rely on them. Indeed, improvements in the performance of the regulated distribution and transmission networks in England and Wales accounts for a large share of the estimated benefits of “deregulation” (Newbery and Pollitt 1997). “Deregulation” has actually stimulated greatly increased interest in the design and effects of alternative regulatory mechanisms governing the residual regulated network segments, mechanisms for pricing access to these network segments to support competition using these platforms efficiently, and the behavior of regulatory agencies which continue to affect important segments of so-called regulated industries.

My earliest and longest standing interest in regulation has focused on the behavior of regulatory agencies and the effects of that behavior on the incentives that regulated firms have to control costs and to introduce new process and product technologies

(Joskow 1972a, 1972b, 1974; Joskow and Schmalensee 1986). In my view, students of regulation of legal monopolies wasted at least 15 years extending the Averch-Johnson model of regulatory behavior and trying to test it empirically without much success (Joskow and Rose 1989). The Averch-Johnson model and its progeny have been replaced with a richer set of models of regulation, both normative and positive, that consider asymmetric information, political economy considerations, legal constraints on agency behavior and their effects on the incentive properties of regulatory mechanisms and ultimately on the behavior and performance of regulated firms. I have in mind here in particular work by Laffont and Tirole (1986, 1993), Baron and Besanko (1984), Sappington (1988) and many others. Work on regulatory mechanisms also has become nicely integrated with closely related work on contracts, government procurement, and organizational behavior stimulated by the integration of considerations uncertainty, asymmetric information, incomplete contracts and specific investments into microeconomics.

Ironically, despite the extensive amount of research done by economists in the United States on mechanism design issues, the application of incentive regulation theories, including benchmarking and yardstick competition, have been much more extensive in Europe, Australia, New Zealand and several Latin American countries than it has been in the United States. Why this should be the case is unclear, but it is at least partially the legacy of a century of living with formal regulatory rules that may appear to restrict what regulators can do. Regulators in the United States have no problem cutting deals through informal regulatory arrangements, but the formal introduction of well designed incentive-based regulatory contracts has been difficult. Maybe the regulators

are insulted by the notion that there is asymmetric information. They do appear to like it better if you call it performance based regulation rather than incentive regulation, so maybe some linguistic acrobatics will help.

Interest in understanding the behavior of regulatory agencies has also forged important linkages with contemporary developments in political science. Stigler's famous paper "The Economic Theory of Regulation" marked the beginning of the end of the widely accepted assumption that regulation was introduced and applied to remedy market imperfections and to pursue widely accepted public interest goals (Stigler 1971). The important role of interest groups, interest group politics, and linkages between branches of government (e.g. McCubbins, Noll and Weingast 1987) in understanding regulatory behavior is now widely accepted and appreciated. Work on regulated industries and regulatory reform has then provided an excellent platform for understanding issues central to political scientists who have increasingly used analytical tools drawn from economics. Research by economists, political scientists, legal scholars, and students of organizations has all been enriched by common interests in understanding the behavior of government regulatory agencies and the understanding that we have something to learn from each other. This leads to the final lesson learned for this paper.

Lesson #9: *Research in industrial organization has been and will continue to be enriched by reaching out to economists working in related areas (e.g. contracts, corporate finance, organization) and in related disciplines (e.g. political science and law). Drawing the lines between what is "in" and what is "out" of the field of industrial organization too narrowly will diminish our ability to understand many important issues in industrial organization.*

VI. Conclusion

Looking back on the progress of research on regulation, deregulation and regulatory reform over the last 25 years makes me very pleased that I have been able to be part of it. A lot has been accomplished in all relevant dimensions—theory, empirical methods, empirical results, and policy applications. What started as a sort of subfield of industrial organization has become fully integrated into it and I believe there is much to learn about scholarly research in industrial organization generally from this experience. I believe firmly that research on regulation and deregulation progressed nicely because the people working on these problems recognized that useful contributions to knowledge could be made using a range of methodological approaches and drawing on knowledge from other fields of social science and law. Research on regulation and deregulation involved the interaction between theoretical and empirical analyses, structural models, reduced form models, and natural experiments as well as institutional analysis drawing on political science, law, and organizational behavior. Scholars working with different methods worked well together and shared their work constructively. This has made this a fun field to work in. Of course, there is still a lot more interesting work to do on regulated and recently deregulated industries and it will continue to have broader positive impacts on knowledge about industrial organization more generally. Let's call this the tenth lesson learned.

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