

# **History in the Study of Industrial Organization**

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## I. Introduction

In studying Industrial Organization, economists have at times turned to the past to illustrate and test its theories. This includes some of the seminal papers of the new empiricism (e.g., Porter, 1983). This readiness to cull from the historical record has neither been examined critically, nor accompanied by much of an attempt to follow the industrial organization of markets over time. This paper asks how history can help us understand markets, by posing the following dual questions: (a) what are the advantages and disadvantages of using old markets to illuminate our understanding of current ones, and (b) is a historical approach to the study of Industrial Organization possible and worth pursuing?

We are talking about history in two different ways: as the past, and as an analytical approach. History as the past means using old markets in empirical work in the same way one uses contemporary markets, whether that is inductively learning about markets in the “theory-development role of applied econometrics” (Morgan, cited by Snooks, 1993), estimating parameters of interest, or “using historical episodes to test economic models for their generality” (Kindleberger, 1990, p. 3). History as an analytic approach means describing a sequence of events as a logical progression informed by economic theory but unencumbered by it, with room for personalities and errors, and perhaps emphasis on certain events with overwhelming importance. It often uses narrative that is not primarily quantitative and does not necessarily impose equilibrium conditions everywhere.

These two different ways of using history are broadly applicable to any kind of social science. The distinction between the two in Economics has been discussed before by, e.g., Coleman (1972), Arrow (1985) and Solow (1985), Hicks (1969), and McCloskey (1976)). This paper will consider how Industrial Organization uses, and should use, history in each of these two ways, and the sense in which they are complementary. It will argue that using evidence on firm strategic interactions from the study of past markets in understanding contemporary markets, without appropriate adjustment, is illegitimate, as it ignores underlying, but generally un-modeled, changes in the environment in which

firms operate, such as changes in firm hierarchies or managerial knowledge of strategic options. The lessons learned from undertaking history as analytic approach should, however, give us the necessary tools to adjust those findings.

Others have written about how Industrial Organization is, or should be, done, such as Fisher (1989,1991) Shapiro (1989), Bresnahan (1992), Einav and Levin (2010), Nevo and Whinston (2010), Schmalensee (2012) and Sutton (1991). Only in the last case is the historical approach considered.<sup>1</sup>

## **II. History as the Past**

Using historical markets in studying Industrial Organization broadens the pool of examples we have to choose from. If intellectual curiosity is the sole motivator in our studying markets then the past is as legitimate an object of inquiry as the present. Intellectual curiosity, although not necessarily practical, should not be sneered at. People are clearly interested in the past, as the demand for genealogy, historical dramas etc. show; “[e]ven if history were judged incapable of other uses, its entertainment value would remain in its favor”, wrote Bloch (1942); Teggart (1925) wrote that the truth of a story makes it interesting.<sup>2</sup> Among economic historians there are those, like McCloskey (1978, p.5, 1987), who see their work as economics in the service of history, using economics to study the past.<sup>3</sup> There are sufficient spillovers in this area to justify economic research into the past, as surely as the government-sponsored basic research that underlies certain industries. However, if it is the past for its own sake that interests us, there is little more to say on the matter. Henceforth, then, we will take the ultimate goal to be an understanding of contemporary economic markets.

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<sup>1</sup> In organizational economics, Baker and Gil (2013) consider the value of narrative.

<sup>2</sup> Historians are reluctant to claim that lessons can be learned from history, beyond disabusing us of erroneously held views (often put there by other historians, as in the case of nationalistic myths (Teggart, Evans (2001), Holbawarm).

<sup>3</sup> Like Kindleberger (1990), McCloskey (1976) sees history in the service of economics; the difference is clearly due to the audience McCloskey addresses in the two different articles.

To the extent that policy concerns drive our work, then, we need to explain why we ever look to the past for empirical evidence to inform policy. Why does the present not suffice?

The first answer is that there are not enough contemporary markets offering the data and the varying conditions needed for an empirical understanding of the current situation.<sup>4</sup> We look to the past because that is where we might find the data. This rather prosaic reason presumably explains Porter's (1983) choice of the Joint Executive Committee in the 1880s to examine collusion under unobserved demand shocks, Bresnahan's (1987) choice of the US automobile industry in the 1950s to demonstrate how price and demand information, coupled with the structure of firm ownership of products, could be used to infer the nature of firm competition and costs, and Mullin and Genesove's (1998) choice of the US sugar refining industry at the turn of the 19<sup>th</sup> to 20<sup>th</sup> centuries to verify the empirical conjectural variations technique of inferring conduct and costs from demand estimates and pricing behaviour.<sup>5</sup> In none of these cases was the historical context presented as inherently interesting. A more general question needed addressing and it was just the luck of the draw that the data could be found in markets of some several decades before.

In several ways, the drawing process favours the past. The higher costs of transportation and communication in the past typically resulted in more markets for a given physical good, thus offering the researcher both a larger number of examples to draw from as well as the opportunity to compare the same product in more than one market environment. Today, many goods are bought and sold in a single global market. There are still some goods whose markets are local, such as cement and cable television, and they are rewarded for that with a large number of studies, but there are fewer and fewer of them, an unfortunate development for the IO economist.

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<sup>4</sup> McCloskey (1976) also notes the sheer increase in data that history offers. For Nevo and Whinston (2010), the paucity of useful examples justifies structural estimation, i.e., the substitution of theory for variation in data.

<sup>5</sup> In at least one of these cases, as is often the case in empirical IO, the authors first located the data, and then formulated the question. But that does not affect the argument here.

### *Documentary Evidence*

Historical markets also offer documentary evidence: memos, minutes and correspondence, both intra- and inter-firm, which reveal strategies, perceptions of firms' own and rivals' costs, perceptions of rivals' strategies and reputations, calculations, intermediate objectives, and crucially, agreements and communications. These are likely to become available only after some time, while current material relevant to ongoing competition remains confidential (Tucker, 1977).<sup>6</sup>

A leading example of using archival documents in understanding market competition is Margaret Levenstein's (1997) examination of the bromine price wars over the turn of the last century. She uses both internal firm memoranda and letters among competitors to categorize price wars, to demonstrate that some were evidently the result of demand shocks, a la Green and Porter (1984), and others the aftermath of low-cost entry. That analysis is no less convincing than Porter's (1983) own statistical test of the theory. Documents can also reveal the management of collusive arrangements, as in Levenstein's analysis and especially Genesove and Mullin's (2001) analysis of weekly sugar cartel meetings. Both show that managing collusion goes beyond establishing the original agreement. Having the communications between firms (correspondence in the first case, verbal in the second) is in fact crucial to analyzing the 'explicit' part of explicit collusion, and understanding its advantage over implicit collusion. Explicit collusion means collusion by means of communications, so in studying it we ought to be studying inter-firm communication. As antitrust authorities do not generally pursue collusion cases without an explicit component (but see Kaplow (2013)), it is surprising that this subject is not studied more.

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<sup>6</sup> The exception is antitrust cases, for which the selection of documents that become public is likely to be extensively biased. At times, economists have access to non-public documents, but those writers have typically served as consultants or witnesses in the case, and so need to be considered biased.

Documents are also useful for studying vertical issues. Contracts between firms are rarely made public – presumably to make it harder for competitors to underbid one of the sides. With archival information, one has the opportunity to view those contracts. Documents can also reveal how firms manage incentive issues not an unchanging rule, but as learning process in which the firm “struggle[ed] to find the right balance”, as Kranton and Swamy (2008, p. 968) show for the East India Company, and Nicholas (1993) for the Hudson Bay Company.

There are other ways to use documents that have not yet been tried, to my knowledge. Firm forecasts and beliefs are two of those. Forecasts are especially important for much IO research where the narrow focus on a single industry at a particular point of time makes the rational expectations approach, in which the actual outcome proxies for the forecast, untenable. Using documented forecasts in empirical dynamic models in particular might lend them greater credibility. Beliefs about the nature of rivals who take out of equilibrium actions are the source of multiple equilibria in asymmetric information environments; the freedom of the theoretician to choose among these beliefs is a weakness of Bayesian equilibrium theory. Knowing firms’ actual (contingent) beliefs would discipline the empirical application of the theory.<sup>7</sup>

For that reason, the study of foreclosure would be revolutionized by direct evidence on beliefs. This area has attracted a fair bit of theoretical attention but little empirical consideration. In monopoly commitment models of foreclosure, outcomes depend crucially both on how much competitors know about their rivals’ contracts and how their beliefs about contracts offered to other firms change with the contracts they themselves are offered (Rey and Tirole, 2003).

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<sup>7</sup> Documentation of intra-firm discourse can also be used to show the strategies that were considered – and so part of the ‘consideration set’ discussed below – but not chosen. Trachtenberg (1991) makes a similar argument in discussing nuclear deterrence, but even he claims that “[t]he test ... of how seriously such documents are to be taken is whether the sort of thinking they reflected had any real impact on policy on specific issues (p. 102).”

Relatedly, knowing what firms know would also help. The theoretical analysis of markets characterized by few firms on each side of the market (Horn and Wolinsky (1988)) depends crucially on what a firm knows about its rivals' negotiations with the other side of the market. Iozzi and Valletti's (2014) presumes that rivals' observability of negotiation breakdowns can be assessed from consumer observability of prices and product availability, but firms might know more, and earlier, than consumers; documents can tell us if that is so.

With the Economics discipline as a whole pivoting away from the full rationality assumption, and signs of at least the desire to do the same in the structural empirical analysis of dynamic behavior,<sup>8</sup> there is a need for some other model of firm decision-making. Documentary evidence of a firm's strategy offers an empirically based alternative for understanding the limits of rationality in firm decision-making. Single decision makers generally leave no such trails, but the need to instruct, to inform and to persuade within large organizations works to our advantage. In reading descriptions of a firm's strategy, IO economists should be particularly interested in how forward-looking the analysis is, and in its degree of 'perfectness'. For example, Alfred Sloan's justification of his product line plan, in which a new model is introduced to fill a gap in GM's current offerings high up in the quality dimension, some close substitutes are eliminated, but no model is introduced close to Ford's Model T, is not forward looking at all, with no discussion of how rivals might respond to the product line shakeup (Sloan, 1964). The goal in analyzing such strategy plans should be to inform our understanding of decision-making in all firms, and not just the particular firm, or industry.

Like any historian, the IO economist working with documents - whether internal firm communication, intra firm communication, court cases or retellings after the fact - must be on guard for biases: the bias in the selection of which documents make it to the archival source, and survive there

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<sup>8</sup> This is most obvious in the rise of Behavioural Economics. Closer to home, it is seen in comments by specialist in structural dynamic models (Rust, 2012) and, even closer, structural dynamic games (Benkard and Pakes, 2013).

(Schwarzkoepf 2013), and the bias in participants' communications, whether consciously self-serving or not. With regard to the latter, Friedman (1953) warns us off from considering intent, on the grounds that, like the pool player, the participants may not be able to articulate why they do what they do, and yet be successful at it nonetheless. He thus instructs us to consider outcomes only, and not expressions of intent. The alternative, however, is inferring firm objectives from quantifiable data such as prices and quantities alone. Declared intent has the advantage that it is not, or at least far less, in need of a model to interpret it. Indeed, so much of the optimality of strategies depends on what firms believe about other firms, future developments, etc. that we are limiting ourselves if we do not use that information, and we need all the help we can get.

For other purposes, the content of communication (what firms say to each other) may itself be the outcome of interest itself; understanding the communication mechanism can be of central concern in understanding both explicit collusion and firm management. The mere profusion of theory, and the limited opportunity to test it by 'outcomes', argues for using such information, as the mapping from theories to quantitative "observables" on their own is far from invertible. Discussions within the firm/organization reveal the constraints they face. Discussions can span a much greater space than actions can.

### *Historical Conditions*

Historical markets afford us the opportunity to analyze an industry operating under different conditions than contemporary ones. Finding data on an industry that can be described under two different sets of conditions permits comparative analysis. The centrality of comparative statics (or dynamics) in theoretical research would lead one to expect that comparing an industry across different environments would be a very common exercise. However, it is rarely done, presumably because the data requirements for the usual empirical IO paper of today, which typically involves demand estimation, are sufficiently demanding that fulfilling them for two different periods is typically

impossible. There are exceptions, of course, for example, Berry and Jia's (2010) analysis of the airline industry. However, comparative analysis for most industries clearly requires an approach that is less quantitative data demanding, as in Sutton (1991, 1998).

More commonly, the past is analyzed independently of the present. The various studies of cartels operating in antitrust environments that are at least perceived to be permissive do not include comparisons to the same industry under a stricter regime (Porter, 1983, Levenstein, 1995, 2002, Ellison, 1994, Genesove and Mullin, 2001). There is thus no opportunity to form an industry specific difference – to conclude, that, for example, the price-cost margin or the incidence of some behavior in a given industry differs across the two permissive environment. Although not explicitly done in these studies, weak conclusions can be drawn by comparing what is found in these studies to what we typically see in the contemporary environment. If some cartel behavior is observed both in the past and in contemporaneous cartels, than it seems reasonable to exclude the desire to elude antitrust scrutiny as a possible determinant. If the behaviour is present in most contemporaneous cartels, but not in the past, it is reasonable to suspect this same mechanism, although the innovation and learning of a collusive strategy is another possibility, as we will discuss later in the paper.

Continuing with the cartel example, the simplest models of collusion predict that for any reasonable discount rate, full collusion is feasible for large number of firms (Shapiro 1989, Farrell 2000). Going even further, if collusion is contractually enforceable, and bargaining among current and potential firms is efficient, then every industry would want to organize as a cartel. Knowing that courts upheld collusive agreements in countries such as the UK in the early 20<sup>th</sup> century (Freyer, 2006) and Finland (Hyytinen, Steen and Toivanen, 2012) Sweden, Norway and Austria up to the late 1980s perhaps suffices

to conclude that the fairly non-fully collusive nature of our economy is not primarily the result of anti-trust policy, but must be the result of more fundamental forces that make collusion difficult.<sup>9</sup>

Notice that no formal 'identification' is invoked in the comparisons, but where the past and present environments are radically different, and where the behaviour is pervasive, the conclusion is a reasonable one, and in the present; no one would argue that save for the antitrust regime, the two sets of economies are identical. Rather, the onus is on he who would argue for some confounding factor that makes the measured effect less than the true underlying one. The juxtaposition of the past and present environments, in which the comparison to the present may only be implicit, is a very powerful tool that serves to bound the effect of whatever mechanism is in question. This sort of analysis need not be done with great detail, and certainly does not require the researcher to commit to a specific model.

In none of the above is the fact that the market existed in the past relevant to the economic analysis. As such, the temporal dimension is comparable to the spatial one. In principle, these studies might have been about markets in different countries, rather than in an earlier period. Some one might have written a very interesting paper comparing the same industry in the UK, where cartels were legal, and US where they were not, or a comparison of concentration across a capitalistic country and mostly socialistic one. Pryor (1972) is a brief statistical analysis in that vein. In all of the examples we have given, it is only the market that is historical, not the nature of the analysis. That the market was in the past may make it easier to study, but the tools that we bring to the analysis are no different from what we would bring otherwise. Were the data available, Genesove and Mullin's (1998) methodological

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<sup>9</sup> This is not to say that anti-trust policy is not effective: Symeonidies (2002) shows that the beginning of an effective anti-trust regime in the UK, after a period in which cartel agreements were not illegal (although also not enforceable), is associated with a relative increase in concentration in those industries that were previously cartelized, as expected for exogenous sunk costs industries. The effect, however, is small - about a seven point increase in the CR4. Another example, more micro-oriented, is Sutton's argument that the high level of firm concentration that coincided in the UK with high brand concentration undermines Schmalensee's claim that strategic product proliferation explains industry concentration in ready-to-eat cereals in the US (p. 232).

verification exercise could have been conducted for 21<sup>st</sup> century sugar markets. Indeed, Kim and Knittel (2006) and Wolfram (1999) have conducted similar analyses for contemporary markets. There is thus nothing fundamentally historical about the analysis in these papers.

### *The Simplicity of the Past and How Far Back We Can Go*

The different conditions that prevailed in the past are arguably useful in and of themselves, not only for purposes of comparison. To begin, technologies were simpler in the past. The production functions of refining industries were particularly straightforward.<sup>10</sup> Many industries today are characterized by zero or near zero marginal cost, making it difficult to find a varying cost component to aid in demand estimation, and making the price-marginal cost gap, which is what the New Empirical Industrial Organization techniques are intended to identify, a less important component of the overall price-cost gap. Innovation was slower. Neither were there as many network and two sided market goods, which blur the distinction between demand and equilibrium solutions.

Firms were also simpler. Chandler (1964) states that before the 1920s, non-railroad firms had a single management layer and that remained the case for many leading firms into the early 1960s. This simplicity makes these firms a closer fit to our models, which generally conceive of firm decision making as that of a single rational agent.

That past markets fit current theory better than contemporary markets does not necessarily make the past an unambiguously better testing ground for determining if that same theory does a good job of capturing contemporary markets. If the past fits theory better than the present, than theory may not do a good job of fitting the present. This will not improve over time: since theory plays catch-up with reality, past markets will always fit present theory better than contemporary markets.<sup>11</sup>

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<sup>10</sup> See Genesove and Mullin (1998).

<sup>11</sup> McCloskey (1976) first denies that the past may be problematic for understanding the present (p. 19, 33), then, allowing for the possibility asserts that the appropriateness of the past has never been rejected statistically. The issue here, however, is not whether firms were profit maximizers in the 18<sup>th</sup> and 19<sup>th</sup> centuries, but whether parameters of strategic behaviour are stable over time, in a data environment in which we have limited

Nor is it useful to think of thinking of markets in any particular period as if they are covered by a special case of a theory that can be empirically identified. Consider basic price determination models, such as Cournot and Bertrand. Embedding these models in more extended structures in which they constitute the final stage has shown that firm strategic behaviour depends, *inter alia*, on hierarchical structure, managers' compensation schemes, and the observability of other firms' profits (e.g., Fershtman and Judd, 1987, and Miller and Pazgal, 2001). Likewise, the profitability of merger depends on the possibility of committing to delegate output decisions after the merger, which depends on firm hierarchical structure. All these underlying factors have changed over time. More generally, theory has identified a number of elements which might affect the nature of the interaction between firms, and which might be unstable over time. Using time periods where the reality is closer to the model makes for stronger tests only for one of the two types of errors, making model failures more damning, but successes less praiseworthy.

'How far back we can go' depends on what we mean to do, inferentially, with our findings from the past. They can be used to: (i) illustrate an empirical methodology (a sort of 'proof of concept')<sup>12</sup>, (ii) demonstrate that a phenomenon exists<sup>13</sup>, (iii) serve as a datum for averaging across markets, and (iv) bound an effect along some dimension, given the special conditions of the studied industry.

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opportunity to fully observe strategic determinants. Furthermore, in contrast to her statement, the opportunity to explain some phenomenon if the past is found to be fundamentally different from the present is no consolation if one was hoping to use the past to stand-in for contemporary markets.

<sup>12</sup> Nevo and Whinston (2010), who note that "[m]any structural papers in [IO] ... are focused on showed that an approach to answering a question is feasible" view that phenomenon as a division of labour between academic economists and those in government, the latter of which, they argue, are likely to be privy to more information in these matters. However, academics get to choose their markets, whereas those in governments respond to proposed mergers, or other firm behavior.

<sup>13</sup> This is the empirical analogue of 'exemplifying', in Fisher's (1989) disparaging characterization of game theory based IO theorizing. Shapiro's (1989) response emphasizes the heterogeneity of strategic interactions, which might suggest that there is little value in constructing an overall average effect.

For (i), calendar time is of no issue. In contrast, (ii) is vulnerable to the charge that the environment has changed substantially since (e.g., how imperfect capital markets are, in the case of predation). For (iii), the past is useless unless one can assert that the distribution of the effect one is measuring is stable over time – which one can not know. (iv) is useful if the industry maintains its position at an extreme point along the relevant dimension.

At this point, casual empiricism suggests that the first two uses are, unfortunately, the most common end of empirical research in IO.<sup>14</sup> In particular, previous findings are seldom used to form an overall assessment of some effect. Literature reviews might present some informal summary of results through citations, but little beyond that, with the emphasis placed on methodology instead. At best, we might expect a table of estimates in a textbook (e.g., Carlton and Perloff's (2005) table of Lerner Index estimates).

A careful review of how the environment has or has not changed in ways relevant to the conclusion can overcome that problem. That approach is unlikely to be followed if the point is to average over a large number of studies. However, if the study is to be used as a bound, there is a greater chance that the limitations of the past will be taken into account.

The issue can be exemplified by the phrase “it is ultimately an empirical question” that commonly closes down a discussion of competing predictions. Since all the models are based on rational agent, equilibrium game theory, the phrase is an admission that the empirical researcher is unable to condition on the factors that differ between the theories. This poses no problem if we are interested in a snapshot of a market at a given point of time; but ascribing the estimates from a past market to a current market requires that those unconditioned on factors remain unchanged over time.

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<sup>14</sup> I am currently collecting citations to a sample of empirical IO papers appearing in the *RAND Journal of Economics* and the *Journal of Industrial Economics* and classify them according to the four categories.

Finally, it should be clear that past and present is a gross divide that should be seen as a terminological convenience only. More precisely, as we move further back in time, the implicit conditions of our model change. We might say, then, that the past is where the implicit conditions underlying our model no longer hold. It is, however, more in keeping with our marginalist heritage to ask whether the benefit of the additional case got by going back an extra decade compensates for the loss of a less good fit to our economy. To respond that our models then are incomplete, and there is some larger model that incorporates both past and present misses the point that models must be simple enough to be usable, which in turn requires them to be particularized to a given time (and place).

### **III. History of Strategy**

Economic history as an analytic approach is relating a sequence of events in a way that imparts them with economic meaning. It is informed by economic theory but unencumbered by it, and leaves room for personalities and errors, and certain events may have overwhelming importance. Historical analysis is a natural complement to Industrial Organization, given the central role IO theory allots to the sequence of moves and such phenomena such as the first mover advantage.

A definite historical process is clearly discernable in strategy. In this section, I consider what a possible history of strategies might look like, as a test of the feasibility of taking a historical approach to Industrial Organization.

Strategies are unlikely to be available to all managers at all time. They are a type of knowledge, which grows over time. This does away with the conceit that economists only uncover strategies that are known to all managers.<sup>15</sup> We should, instead, be thinking of the 'consideration sets' (to borrow a phrase from Behavioural Economics) of strategies. That has another of implications.

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<sup>15</sup> It is an especially difficult conceit for those who teach strategies to MBA students, whether conceived as conveying 'best practices' or academic-originated ideas. The same issue arises in the discussion of military

First, strategy use will display hysteresis. If conditions change in some way that exposes a firm to a new strategy (perhaps having been given birth to by opportunity, when not by necessity) and then change back, the new strategy will remain in the firm's toolkit. This seems to be the case for collusion. Schroeter (1996) writes that banks and states having previously coordinated among firms, German "industrialists became more and more aware of cartelization being a tool of business policy which they could use without the direction of banks or government administrators. After a decade of learning - the 1880s - a decade of trial and error emerged." Alexander's (1994) calculated concentration threshold for collusive behaviour among US industries drops with the lax antitrust regime of the National Industrial Recovery Act, but remains there after the act is found unconstitutional.<sup>16</sup>

Second, when strategies initially develop, there will be a period during which time some firms will be aware of the strategy and others not. This can have a dramatic effect on market shares, amplifying differences when the firm that is privileged with knowledge of the strategy is the leading one, reducing differences otherwise. Whether competition is characterized by strategic substitutes or strategic complements will obviously affect the extent of this effect. If there is increasing dominance, then this will be the start of a process of divergence across firms. Periods of dramatically changing market shares should lead the researcher to consider the possibility that a new strategy has appeared.<sup>17</sup>

Third, the process of diffusion of strategies across firms becomes important. Managerial turnover and mergers and acquisitions are a large part of that diffusion. Tremblay and Horton Tremblay (2005), for example, associate the introduction in the brewing industry of both product proliferation and expansion to exploit learning by doing with the purchase of Miller by Phillip Morris, which had previously used both strategies in the cigarette industry. Although our focus is on market strategy, the

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strategy. Jomini thought it timeless, and that Napoleon was renowned for his military moves only because the development of strategic thought made their strategic elements more evident (Freedman (2013), citing Gat).

<sup>16</sup> Krepps (1999) presents evidence that her results are driven by changing samples over the years of her analysis.

<sup>17</sup> In certain cases, when there are large differences in the capabilities of firms, it might be more sensible to think of the overall balance of power, rather than the availability of a given strategy (Freedman, 2013).

literature on the growth of new management structures provides a number of examples along these lines, such as the administrative changes that took place in 1946 when several GM executives moved over to Ford, or in Nash Motors' merger with the refrigerator and other household appliances manufacturer Kelvinator (Chandler, 1964). An even more dramatic example is that the two firms that Chandler identifies as the earliest and most rationally directed innovators in the M-form of firm hierarchy, DuPont and General Motors, are linked in the persons of Pierre du Pont and Donaldson Brown, who moved to GM after its takeover by DuPont.<sup>18</sup> Of course, mere correlation between the strategy a manager undertakes at his new and old firm does not establish a diffusion process. Rather, one needs to distinguish between the 'push' story suggested here, and a 'pull' story, in which managers experienced in a given strategy are recruited (Boeker, 1997). Here, the documentary evidence is crucial.

Outside consultants also assist in strategy transfer across firms. Strategic behaviour is generally presented in Tirole's classic textbook (1988) ahistorically, as it is in all theoretical work. All the more astounding, therefore, is the one exception: a reference to a Boston Consulting Group (BCG) discussion paper on the learning curve. BCG aggressively pushed the idea that focusing on the learning curve would allow a firm to dominate its market, to the extent that the strategy was for many years essentially its only product (e.g., Keichel, 2010). It is almost the only strategy mentioned that its founder, Bruce Henderson, mentions in his book (Henderson, 1979).<sup>19</sup>

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<sup>18</sup> Chandler, at pains to argue that the four firms he studies arrived at their organizational innovations independently, denies any connection. He thus stresses that Pierre du Point and Brown supported Pierre's cousin Irene in initially rejecting the new hierarchical form at du Pont a month before taking their new positions at GM, where they did accept Sloan's plan (p. 114). However, opinions can change over a month and there may have been greater reluctance to make changes in the family, and eponymous, firm, where cousins still worked. Chandler does write that the administration structure in railroad firms later provided a "precedent" for industrial firms (p. 23). For his part, Sloan (1964), anxious that his role not be downplayed, note that he had outlined a reform plan a year before the du Ponts' arrival.

<sup>19</sup> The only other one is to be irrationally committed to the market, while forcing your rival to be rational about it. Keichel provides an example of unequal familiarity with learning by doing between a BCG toolmaker client and its rival. According to an executive of the client, "every time [the rival would] cost the thing, they'd come back and say "They're not making any money on this' ".

Another type of strategy where outside consultants play a central role is collusion. There is a long history of the promotion of collusion or merger by outside individuals or firms, from John Parks and William Moore<sup>20</sup> in the 1890s (e.g. Edgerton, 1897, and Lamoreaux, 1985) to the association management companies of today, such as AC Treuhand, identified as a facilitator of cartels by the EU (Marshall and Marx, 2012, and Marx and Mezetti, 2014). It seems likely that many of these outsiders did not only facilitate, but diffused methods of collusion and combination.

When the diffusion mechanism is a large, outside organization, diffusion can be controlled. According to Keichel, a conflict over whether to offer strategic advice on a (within industry) exclusive basis led Bill Bain and some colleagues to breakaway from BCG and form Bain and Consulting. The new firm worked not only on exclusive terms, but secretly, so rivals would be unaware that a new strategy was being used.

Firms might act the same irrespective of the availability of strategy consultants. To many economists, the advice that (other) consultants give seems un-nuanced, a one shape fits all bastardization or dumbing down of theory. But money is spent on consultants, which suggests that those within the firm who pay them see some value in their services. That service may not lie in the strategies they offer: Keichel quotes one consultant that most of the activity in value management was “teaching remedial math to middle managers” (p. 213-4). Perhaps consultants are used as fig leaves for difficult decisions, as Ghemawat suggests when he writes that “portfolio analysis gave executives a ready excuse to get rid of poorly performing business units while diverting most available funds to the “stars”” in the post-1973 recession, when credit was tight. Even if the advice is wrongheaded, that does not make them unimportant. Rather the opposite: the more mistaken, or biased, is the advice given

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<sup>20</sup> In testimony before the Industrial Commission, Moore stated: “There is the strongest kind of demand, all over the country, in almost every line of business, to organize, and the trouble is they do not know. It takes time, it takes education.” (*Hearings before the Industrial Commission on the Subject of Trusts and Industrial Combinations*, Washington, 1899)

and received, the greater the effect of consulting on an industry; the more that consultants' advice is correct and equally available to all, the more irrelevant it will be.

Who fills the role of consultant, and so what complementary skills and resources the consultant brings, matters. According to McKenna (2010), bankers filled the simultaneous role of consultants and information conduits among firms prior to Glass-Steagall. With passage of the Act, they could no longer fill that function, leaving the door open for accountants. Presumably, that changed the path of diffusion, decoupling it from the demands of optimal portfolio management. More recently, highly skilled theoretical and empirical IO economists have taken positions as in house economists for major Internet-based companies. The full consequences of that development have yet to be seen.

Another route of diffusion is the professionalization of strategy through MBA program. Dixit (). By providing a language, economists allow for articulation, which allows, in turn, for transmission that goes beyond teaching by example, and for abstraction, which allows for diffusion across industries.

One should also include its popularization through books such as Michael Porter's or, looking further back, Eddy's (1912) *The New Competition*, which promoted 'open prices' (i.e., no secret discounts). The reigning concept of the ideal firm structure, whether that be a conglomerate status or one focused on core competencies, will affect the diffusion of strategy, at the very least through its effect on the movement and interaction of management.

Fourth, since strategies diffuse across markets, what happens in the economy as a whole, and in the past, determines the set of strategies available to decision makers within a given market. The fundamental approach in Industrial Organization of considering each market in isolation of all others is thus, in this regard, wrong.

Finally, where strategies originate may be important. It is possible that Bruce Henderson was the first to think of using the learning curve strategically. No academic research seems to predate the BCG discussion paper: the earliest academic reference to this strategy in Tirole is Spence (1981), the

earliest reference in which is the same 1972 BCG paper, while Arrow (1964) does not discuss the strategic benefits of the curve.<sup>21</sup> That is, a consulting firm might not only have been the prime diffusor of the strategy, but its originator. Of course, strategies may also originate within firms, and the paths they subsequently take across the economy might plausibly depend on what kind of firms they originate in – small or big, new or old, entrants or incumbents?

Lest all this seems Quixotic, we should remember Chandler's successful analysis of the development of the M-form of organizational design. Chandler shows that the move to the M-form was inevitable but at the same time greatly facilitated by a rational approach to the design of the organizational form that would be suited to the new realities of firm structure. As noted earlier, the documentary evidence helps distinguish between the diffusion hypothesis, a 'push' story in which ultimate decision makers are educated in the availability of the new method by incoming managers, and a 'pull' story, in which managers who have experience in using the strategy are recruited for that purpose (Boeker, 1997). Business History's success in tracing the diffusion of other practices, such as in accounting or corporate law (Harris), should convince us of the feasibility of locating the origin of a strategy and tracing out its general path through the economy's firms and markets.

A more ambitious goal is to measure the incidence of the availability or at least use of a given strategy at different points of time. Such basic information is generally lacking. The set of strategies whose incidence we know most about is those imbedded in collusive agreements – uncovered illegal ones, such as Marshall and Marx (2012), Harrington (2006), and legal ones – research on National Recovery Administration such as Krepps (1999), Taylor (2007) and the series of publications by the NRA

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<sup>21</sup> Thompson (2012) cites a 1936 aeronautical paper as the first on the literature, but it is clearly not strategic. (There is a 1972 Rosen QJE paper on the subject.)

itself, Hyytinen, Steen and Toivanen (2012) on Finnish cartels, and current work of a team of researchers comparing among Austrian, Scandinavian and NRA cartels.<sup>22</sup>

A more typical strategy in this regard is predation. We have evidence of predation in individual markets, typically in historical ones – tobacco (Burns, 1986), shipping (Scott-Morton, 1997), telecommunications (Levin and Weiman and Levin, 1994), sugar refining (Genesove and Mullin, 2006) – but also in some contemporary ones, such as airlines (Goolsbee and Syverson, 2008), but no overall picture of its pervasiveness across the whole economy. Establishing that predation *can* occur, especially given the old Chicago school claims that it can not, is important, but that alone falls short of determining how common a phenomenon it is. For the most part, empirical research has not progressed beyond that ‘exemplifying’ stage that Fisher (1989) charged theoretical IO with exhibiting.

## **V. A History of Industrial Organization**

As typically practiced today, empirical Industrial Organization is a micro-oriented discipline, with any given paper limited to a single market, usually over a short period of time. This approach has been very fruitful, but has left us lacking a good accounting of how competition, firm structure and the like change over time, even for an individual market but especially in the aggregate. IO economists lack a grand historical narrative of their subject matter, something that would be analogous to the basic times series properties of the main macroeconomic variables, with which every macroeconomist is familiar, and which every macroeconomic theory must fit.

In this section, I ask whether an economy-wide research agenda might be constructed. Is there a broad History of Industrial Organization that describes the changing nature of competition and firm structure, also at the level of the entire economy? That would be so, if there were common changes to

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<sup>22</sup> Blom et al’s large scale surveys of management strategies suggest that something similar could be done for business strategies. However, it is likely that firms would be more reluctant to be forthcoming about the latter, and in any case, this could be useful only for going forward in time.

the environment, whether exogenous or emergent, in which firms and industries operate that make it worthwhile to study IO issues at the level of the economy and trace them over time. I would like to show plausible candidates for such determinants, some of which others have been suggested before, some of which might be new. It is useful to organize them according to the three components of any industry level analysis: consumer behaviour, costs and conduct.

Several important elements of consumer behaviour have changed dramatically over time and common across industries. The exposure of consumers to advertising has gone through several different media, each new one encroaching on the previous ones, from newspapers through magazines, films, radio, television and the Internet. The introduction of television led to large scale effects of advertising, while the Internet itself has made both targeting of ads and consumer initiated search more precise. Firms' knowledge of demand has also grown more precise over time, especially recently with both the ability to trace individual users' purchases and 'clicks' and the use of 'big data' to uncover correlations at the population level.

Another common demand transformation is market integration, whether through the Internet again, globalization, the one market policy in the EU, or the railroad in various countries in the 19<sup>th</sup> century. Levenstein (1995) argues that the last led to the pool. A changing distribution of wealth can also lead to systematic shifts in demand across consumer goods. Eizenberg and Salvo (2015) have identified one such in the new middle class that is expanding in emerging economies such as Brazil; their analysis is restricted to the soft drinks market but the basic argument should apply more generally across many goods.

Aggregate demand may be relevant. Ghemawat (2002) claims that firms were not particularly interested in strategic thinking in the post-World War II period. He ascribes this lack of interest to excess demand, consequent upon the destruction of much of the capacity of Western European nations. Ghemawat does not explain the mechanism, but one can readily think of two: capacity constrained

firms will behave competitively (the Bertrand equilibrium price is the competitive price for sufficiently low capacity relative to demand); and the gains from capacity building were sufficiently great that issues of strategy were not important enough to deserve managers' limited time. The notion that executives' limited managerial capacity for strategic thought means that strategic changes will not take place in tumultuous times, or when there are large opportunities available even without such changes echoes Chandler's (1964, p. 36) explanation of US firms' disinterest in administration at the turn of the 20<sup>th</sup> century. According to Chandler, the opportunity of expansion, as well as obtaining resources left managers with "little time" to address administrative issues.<sup>23</sup> Once again, the study of management structure provides a model for IO.

The structure of costs has changed uniformly over time for large sets of industries. In *The Visible Hand*, Chandler (1990) considers the increases in economies of speed and scope in many industries at the end of the 19<sup>th</sup> century, which he views as the leading to vertical integration. IO economists have also recognized overall technological change as an instigator to change in the competitive environment. Milgrom and Roberts (1990) focus on the complementarities between information technology cost reductions and various organizational and productive changes, whereas Baker and Hubbard (2004) consider the effects on firm structure.

Finally, conduct has also changed in a unified fashion, with anti-trust policy, intellectual movements (the Open Price movement), the contractual environment, conglomeration and then diversification, and the strategic sophistication of managers, as discussed above.

Changes in anti-trust policy will have a common effect across industries. Here some work has been done, such as Symeonidis (2002) and the various papers on the National Industrial Recovery Act (some, like Cole and Ohanian (2004), even considering economy-wide effects) , as well as the

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<sup>23</sup> Chandler also ascribes the limited administrative changes during World War II to managers' preoccupation with "the problems of conversion to military and then reconversion" (p. 44).

examination of the claim that anti-cartel enforcement after the Sherman Act led to the first US merger wave (e.g., Bittlingmayer, 1985). Jovanovic () and Jovanovic and Rousseau's (2002) work on reallocation through mergers might provide a theoretical basis for such work.

Macro-economic factors also impact upon firms in ways relevant to IO. Strategic actions that are essentially investments, i.e., involve giving up on current profits for higher future profits, like predation or collusion, are sensitive to credit conditions. So should the nature of strategic interactions, via firms' debt positions (Brander and Lewis, 1986), although the sign of the effect depends on whether strategies are substitutes or complements. Financial innovation has been seen to abet merger formation (Nelson, 1959). Lamoreaux (1985) emphasizes the timing of economic turndowns relative to the industry life cycle: new industries lack the mutual knowledge and trust to maintain collusive discipline in the face of negative demand shocks, and thus when the Panic of 1893, firms in such industries cut their prices. She furthermore argues that this experience led these same industries, and not more established ones, to later participate in the Great Merger Movement of the turn of the century. Lamoreaux, however, is perhaps alone among recent economists in tying Industrial Organization issues so explicitly to macroeconomic events among empirical economists.

To these should be added the innovative environment – intellectual property rights regimes, but also the life cycle stage of a general purpose technology (GPT), and the contractual makeup of the leading firm(s) providing the GPT.

Such economy-wide assessments of the determinants of market structure provide more than a simple summary of these forces. Even for one interested only in the individual market, understanding the average state of the relevant parameters remains important, as there are likely to be feedback mechanisms between the aggregate economy and a specific market. One such mechanism is the diffusion of strategies across industries described in the previous section. If strategies are more likely to

emerge when they are profitable and be forgotten when not used,<sup>24</sup> and if common wisdom about the usefulness of a given strategy emerges among managers, then the more useful a strategy has been on average in the economy, the more likely it is to be subsequently used in a given market.

Another feedback mechanism is the political economy of competition, since the regulation of competition is likely to respond to the average (weighted according to interest group power) competitive situation. A US example is how inter-firm cooperation in the war effort in World War I provided the ideological framework for the pro-cartel National Industrial Recovery Act. Building an optimal anti-trust policy that recognizes the limited usefulness of rules that condition on information about the industry that is costly or impossible to gather requires a picture of the average state of competition.

A broad history of Industrial Organization should also prove useful in guiding us in our use of historical markets to inform policy decisions for contemporary markets. Absent specific information on each of the underlying conditions that various theoretical models have shown to determine the nature of competition. We may not know everything we might want to know about delegation, compensation, credit conditions and strategy consideration sets for the particular market that we are drawing on, but if we have a general picture of what those conditions were at any point in the past, we can make an informed decision of how well it fits the present, and how important any deviation may be for our analysis.<sup>25</sup> We may, for example, wish to discount the findings of predation in past markets if we think that capital markets in the past were more imperfect than they are today.

## VI. Errors

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<sup>24</sup> Choosing delegates to US national party conventions with loyalties to other than the primary winner, would appear to be a strategy that was forgotten by some after forty years of uncontested conventions.

<sup>25</sup> Using the typical to impute the particular can be useful, but the dangers of inappropriate inference are clear. See Evans (2001) on Zemon Davis' imputation of a typical French woman peasant's *mentalité* to Bertrande de Rols, Martin Guerre's wife.

A close, case-based analysis of firm errors, especially large, continuing and then dramatically reversed ones, offers a perspective on economic behaviour very different from the standard, regression based econometric studies. Tracing firms' responses to their own errors, once recognized, often reveals the constraints they operate under. Such learning embodies a sequence of events that makes an historical-narrative approach appropriate. Tracing their response to others' errors can show how equilibrium – what happens in a continuing, error-free state – is sustained.

Advertising provides a number of examples of egregious firm errors. Anheuser-Busch spent 20% of its advertising budget on billboard ads before its president decided to find out whether they were effective, and found that they were not (Ackoff and Emshoff (1975). that. More recently, Blake, Nosko and Tadelis (2014) convincing show that companies overspend on Internet search ads. Sutton (1991) offers a couple of other examples. He cites (p. 219) Coca-Cola's ads that compare the leading firm to its rival Pepsi – the cardinal mistake for an incumbent to make.

Firm product choice provides additional examples. The UK prepared soups incumbent Heinz responded to the entry by the US Campbell Soups of condensed soups by introducing its own condensed soup, the main effect of which was to make consumers more comfortable with consuming that new product, thus facilitating Campbell's entry (Sutton, 1991). Henry Ford's refusal to introduce a higher quality car in addition to, or in place of, the Model T, ignored both the benefits of economies of scope instead of scale and the growing competing supply of used cars (Sloan, 1964). Hortascu and Puller document firm errors in pricing.

What do we gain from identifying errors? The first, and simplest, benefit is correct inference. The standard inference from input demand in the beer industry would be that billboard ads were effective, rather than just that the firm thought them to be. Allowing for a structural break at the time of Anheuser-Busch's reconsideration of its advertising strategy would lead one to interpret the fall in billboard advertising as a sudden decline in the productivity of billboard ads. Most applied economists

would start ruminating about the effect of changing media (the growth of cable? sports shows?); the more imaginative might think of a change in product offering and a correlation of consumer tastes with media exposure. Few would consider that perhaps the firm(s) had been making a mistake for many years, before recognizing it and stopping the wasteful advertising.<sup>26</sup> Awareness of such errors save us not only from erroneous conclusion about the particular industry, but alerts us to the possibility of such errors occurring in other industries.<sup>27</sup>

Two counter-arguments are likely to be raised. The first is that firms' mistakes are random, and will average out in any statistical analysis. But both the Anheuser-Busch and the Ford stories shows that a given firm can consistently make the same error year after year. Nor are errors likely to average out to zero at the industry level or in a panel of firms, for the simple reason that IO is the study of competition among the few. There being few, a single firm will be a large part of the industry aggregate, so industry averages are not immune to a single firm's systematic error. Furthermore, other firms' behavior will amplify the error if there is strategic complementarity or learning across firms. Furthermore, Blake, Nosko and Tadelis' (2014) finding indicates that firms' errors are correlated, and in their case, apparently economy-wide.

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<sup>26</sup> Yet another counter argument is that excessive advertising can be simply understood as effectively a lower cost of advertising. The firm acts as if advertising is cheaper than it is. That is fine for a positive analysis of firm behaviour, but the feedback into firm profits would be misinterpreted. And of course, the realization that billboard advertising is not actually profitable is not predictable and the econometrician would still interpret the realization as an exogenous shock.

<sup>27</sup> Knowing that a firm changes its strategy because it has recognized that he has made a mistake may possibly assist in identification.

The second counter-argument is that errors can not be long lasting because of the ‘survivor principle’. A firm that consistently makes mistakes will be forced out of the market (or forced to switch managers or owners). This argument fails on both narrow empirical grounds and on more conceptual grounds. Empirically, we see from some of the above examples that errors can be long-lasting, much larger than the median six years of the period examined in a typical IO industry study.<sup>28</sup>

Conceptually, the ‘survivor principle’ is less discriminating than one might hope. This is especially so in markets with few competitors, that is, those markets that are at the centre of the study of Industrial Organization. The survivor principle fails for a number of reasons:

It can take a long time to operate in industries with long lived assets, when either those assets are firm specific or there are high transaction costs to transferring them. Most of what we will see then is errors and their corrections.

It ignores the integer problem and efficiency rents.

It ignores problems in the market for corporate control. That mechanism only works, if it works at all, at the very highest level of the corporate hierarchy, whereas the difficulties may be further down the line, which even the best manager may find impossible to fix.

It ignores cross subsidization within firms. A firm may be profitable in one industry but not in another. The error may be precisely the continued presence in the second market.

It ignores commitment problems – non profit-maximizing behaviour may benefit the firm in some circumstances.

Thus although selection will winnow out some errors and some error-prone managers, it will not be fully effective, it will take time and errors will abound. Indeed, a large part of managing an organization, whether it be a firm, a collusive agreement, or anything else, consists of recognizing mistakes and taking steps to avoid them in the future. This is learning and adaptation.

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<sup>28</sup> Based on the universe of empirical papers in the RAND Journal of Economics, between 1984 and 2002.

In fact, a rendition of errors and responses to those errors is an alternative rendering of firm behaviour: alternative to our most basic paradigm in empirical work, which is that behaviour can be summarized by a common rule that depends on environmental parameters, and by observing how behaviour differs across different, stable environments, we can uncover that rule. In contrast, Genesove and Mullin's (2001) reading of the Sugar Institute shows the development of a rule in an unstable environment, in following how that combination struggled over time to correct the errors in its collusive agreement.

The analysis of errors can also illuminate the structure of equilibrium. Collusion offers a number of examples. A cartel arrangement that fails, fails in a particular way; how it fails shows why it could not be sustained. Late 19<sup>th</sup> century regional salt pools set too high a price that invited entry (Levenstein (1995)). The US macaroni cartel failed to accommodate its low cost firms, who cheated, and fell apart when National Recovery Administration codes were unexpectedly not enforced (Alexander, 1997). The Sugar Institute's information sharing unraveled when the largest firm, and then the next largest and so on, proved unwilling to share (Genesove and Mullin, 1999). Firms in the Brazilian coffee cartel misconceived the state of their competitors' incentive compatibility constraints (Bates, 1998). In all of these cases, the firms fail to anticipate the problem, rather than the environment changing and making a previously sustainable situation unsustainable. How spectacular a failure is sheds light on how distant the cartel was from satisfying the necessary equilibrium conditions: the failures among the marginal cartels are likely to sputter out. There is no parallel insight where a cartel succeeds, as we do not see what was critical to its success.

Failed entry also reveals what underpins equilibrium. The massive incumbent advertising that greeted the early 1970s failed entry by large, non-cereal food companies into the granola cereal market is viewed by Sutton (1991, p. 238-9) as a crucial confirmation of his theory of advertising as an endogenous sunk cost. Interestingly, Sutton's argument here differs from how he himself describes (p.

309) his use of history. According to Sutton, the role of history is to examine whether movement away from a stable situation in the wake of an exogenous change corresponds to the profitable deviation identified by theory that makes the previously stable situation no longer stable.<sup>29</sup> For him, this is an additional test that theory must pass, beyond the usual first order and long run conditions. There is no exogenous change in this example, however, but rather the entrants' mistaken appraisal of rivals' responses.<sup>30</sup>

IO economists of all stripes are of course aware that firms make errors. However, tales of errors tend to be reserved for purposes of motivating anecdotes; the analysis of \*\*\* assumes no systematic errors. This disconnect is most obvious in the hold-up and regulatory commitment literatures. Williamson's 'hold-up' thesis is buttressed by his famous story of the cable company in Oakland, California. Newbery (2002) delivers a number of examples of regulatory exploitation of firms, such the expropriation of private tram lines in the UK in 1870 and Jamaica's 1962 announced renegotiation of its contract with the telephone monopoly, which led to a drastic fall in investment.<sup>31</sup> Somebody erred there. At the same time, both Williamson and Newbery predict that governance mechanisms will be used to avoid these occurrences, and, especially in the first case, cross-sectional regression analysis has been used to check, and generally uphold, that prediction. Squaring the "when things go wrong" and "optimal governance structure choice" stories would take an historical approach in which firms and regulatory agencies learn over time. Learning is continual, and things continue to go wrong, since new technologies appear and demand changes.

## VIII. Conclusion

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<sup>29</sup> Note that the deviation – identifying a disequilibrium - interests Sutton, not the new equilibrium, i.e., the comparative static analysis. Sutton (1998) adds two additional uses of history, but they correspond closely to the usual econometric exercise: \*\*\*.

<sup>30</sup> Newbery's (2002) examples of regulatory exploitation of firms are similarly informative of the equilibrium regulator-regulated relationship.

<sup>31</sup> Iacobucci, Trebilcock and Winter report a similar story for electricity in Ontario in 2002.

Recognition that there are historical processes that underlie the particulars of competition in individual markets implies that one can not treat the past as the present, and import, without correction, our estimates of parameters of strategic interactions from the study of markets in the past into our models of contemporary markets. At the same time, having uncovered those same historical processes, we should be able to adjust those same estimates, or at the very least determine the direction in which they bound the relevant parameters for today's market.<sup>32</sup> This is obviously a simultaneous process in which we refine our understanding of the historical process at the same time that we fill in our missing understanding of today's markets.

After considering banishing from the City not poets, as Plato suggests, but critics, George Steiner (1989), himself a critic-philosopher, concludes that criticism would continue unabated without the critics, for art responds to art in critical fashion. So does science to a large degree, so that one might think that a scientific discipline might likewise progress equally well without methodological essays. Empirical Industrial Organization is so fragmented, however, with both a paucity of data on a given market and its current micro-orientation that its literature generally avoids raising the larger issues of what can be learned and, especially given that micro-orientation, how, and if, its findings can be aggregated into an overall picture of the economy. This exploratory essay is meant to bring these questions to the fore, especially with regard to the appropriateness of historical markets as a base of inference about contemporary markets, and the value of analyzing markets with a greater historical eye to what has transpired over time. The essay has suggested some answers, but they are likely to be far from definitive.

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<sup>32</sup> Does anyone do this? An obvious place to look is in surveys by historically oriented writers, such as Lafontaine and Suslow. Authors need to differentiate their work from that of predecessors leads them to note differences in the underlying institutional environment, but rarely from an historical perspective. (So read that survey, Bresnahan, finance and innovation, etc.)



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