As a young economist in 1954, Arnold Harberger, who would later become a stalwart of the University of Chicago economics department, published an article entitled "Monopoly and Resource Allocation," which challenged the conventional thinking of economists. Harberger's article is best remembered for its striking conclusion: that markets are so efficient in allocating resources that any distortions created by monopoly were bound to be inconsequential -- at most 0.1 percent of the Gross National Product (Harberger 1954). In 1959, Harberger returned to the same idea, suggesting that removing distortions in Chile's economy would create a relatively insignificant improvement in economic performance (Harberger 1959).

This brilliant display of applied price theory inadvertently lent support to James Tobin in his effort to revive interest in Keynesian economics at a time when Keynesianism was increasingly falling out of favor. Harberger's encounter with Tobin throws an interesting light on the nature of applied welfare economics and its tenuous relationship with macroeconomic theory.

Harberger's article also prompted Harvey Leibenstein, a Harvard professor with an excess supply of research assistants, to investigate how monopoly might affect the productive capacity of the economy. Leibenstein proposed that firms often operate with a considerable degree of slack rather than maximizing their profits.
Because Leibenstein could not fit his proposition into a formal economic model, he called this variability of performance, "X-efficiency" -- an illusion to the Russian novelist, Leo Tolstoy's *War and Peace*, which contained the observation: "Two armies may be identical in every observable respect ..., yet one army, in possession of an intangible 'X-factor,' will soundly defeat the other" (Part XIV, II).

The notion of X-efficiency went far beyond challenging Harberger's article. After all, profit maximization is a core assumption of neoclassical economics. This larger threat did not escape the attention of the curmudgeonly George Stigler. As Claire Friedland, his close co-worker, observed in a *Journal of Political Economy* Stigler memorial issue: "Much of his work centered around saving the damsel in distress, neoclassicism, from her attackers" (Friedland 1993, p. 780).

Stigler held nothing in his efforts to protect neoclassicism. Thomas Sowell, an admiring student of Stigler's, used his contribution to the Stigler memorial issue to liken his mentor's style of debate to a "Demolition Derby" (Sowell 1993, p. 787). Symbolic of his combative nature, Stigler captioned a picture of John Stuart Mill, describing him as "perhaps the fairest economist who ever lived: He treated other people's theories at least as respectfully as his own, a mistake no other economist has repeated" (Stigler 1987, p. 99).

Stigler responded to Leibenstein with a not uncharacteristic no-holds-barred attack.

In the end, Harberger, based on his long experience as a development
economist, came around to lending support to Leibenstein's analysis, but without ever mentioning Leibenstein's work.

The exchange between Stigler and Leibenstein is interesting in its own right, but also it is revealing in terms of the protagonists' divergent understanding of empirical analysis.

Harberger's Triangles

Harberger struck a rebellious note at the very beginning of his article with a provocative statement, suggesting that he intended to upend conventional economic thinking about monopoly:

One of the first things we learn when we begin to study price theory is that the main effects of monopoly are to misallocate resources, to reduce aggregate welfare, and to redistribute income in favor of monopolists. In the light of this fact, it is a little curious that our empirical efforts at studying monopoly have so largely concentrated on other things. [Harberger 1954, p. 77]

Over and above the surprising findings of the article, one cannot help being struck by Harberger's ingenuity. Using tools that would be appropriate for an introductory class, Harberger produced a simple supply and demand diagram in which monopolistic power shifted the supply curve upward.

Harberger went on to show that the move to a new supply curve had only a slight impact on total welfare, measured by the triangular shaded area. Using rough estimates of the slopes of the relevant lines, he estimated the inconsequential dimensions of the welfare cost
of monopoly.

Harberger's article continues to be quite influential for good reason. Economists had used this kind of diagram for more than a century (see Hines 1999), but nobody before had ever tried to apply it empirically. Besides, the application of this diagram is a wonderful demonstration of the power of straightforward price theory to produce striking results. Even today, more than a half century later, practically any economist will immediately understand the meaning of the expression, Harberger triangle.

Harberger's results were not just striking; they were undoubtedly welcome. The article appeared in the wake of Joan Robinson's and Edwin Chamberlain's work on monopolistic competition. Their analysis offered a challenge to Chicago-style economics. After all, monopolistic industries behave in ways that do not conform to standard price theory. Worse yet, if monopolistic conditions were common, the relevance of price theory would be further diminished.

Two of Harberger's slightly older colleagues, Milton Friedman and Stigler, had attempted to downplay the extent of monopoly. In his famous essay, "The Methodology of Positive Economics," published just before Harberger's, Friedman signaled the relative importance of the question of monopoly by placing the question of monopolistic competition as a crescendo just before his concluding remarks (Friedman 1953). In Capitalism and Freedom, Friedman returned to the theme that monopoly is not a significant force in the economy (Friedman 1962, pp. 121-28).
A decade before, in the early 1940s, Stigler had already been criticizing the work of the New Deal's Temporary National Economic Committee's analysis of monopolistic conditions (Sigler 1942). Late in his career, Stigler chose to speak on the subject of "The Economists and the Problem of Monopoly" in his Richard T. Ely address. In his Five Lectures on Economic Problems, Stigler devoted two to the problem of monopolistic competition (Stigler 1949). In the first, after briefly dismissing Joan Robinson's work on the subject, he took on Chamberlain by describing the difficulty of defining an industry, implying that the monopolistic competition was incapable of providing the kind of formal analysis that Chicago offered. In the second article, Stigler offered some rough calculations about the extent of monopoly, based on some standard definitions of monopoly. Neither article was particularly succeeded in vanquishing the theory or proving the irrelevance of monopolistic competition, but the effort probably created enough confusion to allow some economists to continue their work untroubled by the challenges posed by Robinson and Chamberlain.

Stigler and Friedman were raising theoretical and empirical questions about the measurement of monopoly. Harberger took a much bolder approach, in effect, saying, "So what? Even if monopolistic practices are common, they are still harmless."

Harberger's defense of neoclassical price theory was doubly effective. Stigler and Friedman had to resort to complex methodological and empirical reasoning. Harberger, in contrast, used
straightforward price theory to raise his defense of price theory.

**General Response**

Harberger probably knew that his work would raise considerable controversy. He began the article by announcing the novelty of his project in a tone that suggest a spirited revolutionary challenge to the status quo rather than a defense of traditional price theory:

> One of the first things we learn when we begin to study price theory is that the main effects of monopoly are to misallocate resources, to reduce aggregate welfare, and to redistribute income in favor of monopolists. In the light of this fact, it is a little curious that our empirical efforts at studying monopoly have so largely concentrated on other things. [Harberger 1954, p. 77]

As might have been expected by such a brazen challenge, Harberger's article engaged or enraged different flavors of laissez-faire economics. After all, his attack on the effectiveness of antitrust regulation possibly raised more problems for laissez-faire economics than it solved.

Most obviously, if antitrust regulations that lower prices do not do much good, the same reasoning would suggest that other types of regulation that raise prices do little harm. For example, Harberger himself estimated that according to his method, the corporate income tax had a relatively trivial impact, even though it was still five times as great as the effect of monopolistic practices (see Hines 1999, p. 179).
Some conservatives were concerned about the implication that tampering with markets would do little harm. After all, if monopoly, or even the corporate tax rate, is not particularly destructive, would intrusive regulatory policies be equally benign? Along this line, James Hines hints that the literature on rent seeking might have been a delayed response to Harberger's article (Hines 1999, p. 183).

One broader question in particular comes to mind. If price distortions are so inconsequential, just what is it is that markets are supposed to do? Future Nobel laureate, Robert Mundell came closest to raising this point in 1962. Reflecting upon the insignificant dimensions of Harberger's triangles, Mundell worried that if distortions did so little damage, "someone inevitably will draw the conclusion that economics has ceased to be important!" (Mundell 1962, p. 622).

One wonders if Harberger, despite his almost cocky tone, had anticipated criticism of this sort. Obviously, he was prepared for some questions from the left, realizing that the elegance and simplicity of his model came at the price of ignoring many consequences of monopoly. For example, he anticipated future critics would be likely to raise questions about the way that the model measures total welfare without taking into account the redistribution of income from consumers to monopolists. Harberger preemptively dismissed potential critics who might bring up such matters with the cavalier comment:

I have not analyzed the redistributions of income that arise
when monopoly is present .... I leave [questions about income distribution] to my more metaphysically inclined colleagues to decide. [Harberger 1954, p. 87]

**Harberger as Development Economist**

Although Harberger may not have paid much attention to rent-seeking activities, he has long been associated with policy matters regarding economic development. Few economists ever have the opportunity to implement their vision as widely as Harberger. Either directly or through his students, Harberger was able to exert a great deal of influence on economic policy in countries around the world, especially in Latin America. David Levy offered a concise summary of this part of Harberger's career:

> Government leaders of more than 15 countries have called upon Arnold Harberger's expertise. With a reputation as a hands-on practitioner of economics, Harberger has also held consulting positions with global organizations, including the International Monetary Fund, the Asian Development Bank and the Organization of American States. Numbered among his students at the University of Chicago and the University of California Los Angeles are at least a dozen central bank presidents and two dozen foreign government ministers. [Levy 1999]

Judging by the geographic scope of his influence, Arnold Harberger became an extraordinarily successful economist.

One consistent line that Harberger and his students followed was a
strong hostility to the social welfare state. At the same time, Harberger was very friendly with undemocratic governments, which were installed by coups. Some of them openly used lethal action against people or groups who were not sufficiently market-friendly. These associations made Harberger controversial in some quarters.

Harberger himself was proud of what he had accomplished. Harberger published a "Letter to a Younger Generation," which can be read both as an exhortation to young economists and an overview of his career. There, he wrote that economists have "a great mission to help harness the concepts and ideas of our discipline so as to improve the economic lives of our fellow citizens." In this regard, he described himself as "a seriously dedicated missionary who has spent most of his professional efforts in the search for better economic policies and in trying to better prepare the profession itself for carrying out this search" (Harberger 1998b, pp. 1-2).

The explicitness of Harberger's discussion of his missionary zeal is relatively unusual, although most economists have a strong belief system -- what Joseph Schumpeter appropriately called a "pre-analytic vision" or what others call strong priors -- that drives much of what economist do. Unfortunately, like missionaries, economists can become wedded to a particular truth. Such a state of mind can prevent openness to other ways of thinking. Harberger even admitted that his experience at the University of Chicago helped to create "the vision, and if you like, the biases that I will be expressing here" (Harberger 1998, p, 5).
One must be careful about expecting consistency in two papers written nearly a half century apart, but Harberger's self-definition as a missionary makes an interesting contrast with his earlier denigration of "metaphysical" questions about the redistribution of income. However, his terminology is suggestive of the way many economists operate: "my theory, powered by my pre-analytic vision, gives me access to the truth, which is worthy of missionary zeal. Your reservations about my theoretical analysis are nothing more than metaphysical quibbling."

However, an idea, regardless of its intended purpose, can take on a life of its own, resulting in unanticipated consequences. Schumpeter's own appropriation of both Marx and quite a bit of Marx's method might be a case in point. Some Marxists' appreciation of Schumpeter might complete the circle.

The first unintended event was purely rhetorical.

Harberger Triangles vs. The Okun Gap

Although Harberger implored students to pay close attention to price theory, he saved any mention of his triangles to the end of his Letter. Ironically, at that point he said virtually nothing about the importance or the relevance of the work that launched his career. Instead, he confined himself to critiquing an off-hand comment by James Tobin.

Writing after Keynes had already been declared dead, the Nobel Laureate, James Tobin, demonstrated why conservatives had good reason to be uneasy about insignificant dimensions of Harberger's triangles.
Tobin invoked Harberger to argue for policies that were antithetical to Harberger's own vision. Not without a touch of provocation, Tobin wrote:

Any economics student can expatiate on the inequities, distortions, and allocation of inefficiencies of controls or guideposts or tax rewards and penalties. But just consider the alternative. The microeconomic distortions of incomes policies would be trivial compared to the macroeconomic costs of prolonged under-employment of labor and capital. It takes the heap of Harberger triangles to fill an Okun Gap. [Tobin 1977, p. 468]

Tobin's rhetorical flourish was only intended to strengthen his case for directly stimulating employment. Nonetheless, Tobin must have stung Harberger to the quick. Harberger reserved the final section of his Letter to respond to Tobin, diverging from his earlier pattern of discussing how his own theory intertwined with what he considered to be appropriate policy lesson. Harberger began on a personal note:

Now when I first heard that remark, I issued a verbal challenge to Tobin, for him to try to express the Okun gap in the language of applied welfare economics. I have not seen any such effort, and I think the reason is not hard to find. [Harberger 1998b, p. 30]

The sarcasm dripping from that last sentence suggests Harberger's irritation, which must have been festering for more than a decade. Yet, Harberger's own application of applied welfare economics is
curious. Recall the introduction to his original study, where he listed the three great defects of monopoly: (1) the redistribution of income in favor of monopolists, (2) the reduction of aggregate welfare, and (3) the misallocation of resources.

Harberger, however, merely shrugged off the question of redistribution of income as metaphysical. It is true that welfare economics often supports policies that have harmful redistributive effects. That support, however, is conditional upon the possibility of making compensatory payments sufficient to make the injured parties better off than they would have been in the absence of such policies.

But Harberger could not make any claims of welfare improvements. His point is merely that the declines are not great.

Harberger's analysis might seem to be effective in refuting the second defect of monopoly -- (significantly) reducing welfare -- but with two caveats: the metaphysical question of redistribution has to be disregarded and first defect (misallocation) is inoperative.

Harberger's appeal to welfare economics was merely meant to accuse Tobin of failing to take into account the welfare costs of the leisure that workers sacrifice by becoming employed. Presumably effects of this lost leisure would offset significant amounts of the welfare gains from an expansion of employment. A cynic might suggest that Harberger could have gone further in pressing his case by claiming that the employment loss from monopoly might create sufficient leisure to actually make monopoly a welfare-improving arrangement.

Applied welfare analysis rarely concerns itself with the subjective
state of people, relying instead on observable transactions in the marketplace. Harberger had the good sense to dismiss wages as a measure of the value of a worker's lost leisure from employment, but he recognized that social conditions affect the reservation wage. He even acknowledged that unemployment affected the welfare of others besides an individual worker. In fact, the positive externalities of employment on friends, family, and the community at large might go a long way toward offsetting the welfare costs of lost leisure in many cases.

The externalities of unemployment go further. Most economists realize that substantial unemployment changes the social relations between labor and capital at the workplace. Metaphysical differences will determine whether one emphasizes either the improvements in productivity resulting from workers' increased fear of losing their jobs or alternatively the employed workers' anxiety and perhaps increased work demands.

Although Harberger did not take the last step in linking unemployment rates to social relations in the workplace, he certainly realized the complexity of where his argument was leading him. He concluded this discussion by asking:

How can we give sensible prescriptions for relieving the problem, if we operate out of such a state of ignorance? So, I submit, is a real challenge to us. [Harberger 1998b, p. 33]

The encounter between Harberger and Tobin hardly constituted a major
debate. Tobin made an offhand gibe about Harberger's decades-old article and Harberger answered Tobin more than a decade later. The exchange is interesting nonetheless because it seems to put Harberger's analysis in stark relief.

Or did it? Harberger inadvertently, offered substantial support for Tobin. In the earlier part of his Letter, Harberger emphasized that "real cost reductions are at the heart of the growth process." He went on to say: "Real cost increases in such cases often stem from output being driven down well below the levels appropriate for existing installed capacity" (Harberger 1998b, p. 14). In Harberger's original article, his analysis was static. Here in a more dynamic context, he was suggesting that increasing employment can cause real cost decreases, which can stimulate both investment and growth.

Harberger's focus in other parts of the Letter was more nuanced than Tobin's. Harberger recognized that aggregation can obscure important aspects of the economy. For example, in discussing the nature of movements in real costs, Harberger observed: "bad growth experiences often sit side by side with good experiences in the same industry, as successfully innovating firms in that industry thrive and expand, while their less fortunate competitors are driven to the wall" (Harberger 1998, p. 14).

Harberger's last observation is relevant to another critique that ignited an intense controversy. The resulting debate was quite different than his engagement with Tobin. Here, Harberger did not rise to his own defense -- instead, his colleagues did. In fact,
observations, such as the last one about productive diversity, seemed to lend further support to his critics.

**Leibenstein's X-Efficiency**

This more serious challenge came from Harvey Leibenstein, a respected economics professor from Harvard University (Leibenstein 1966). Like Harberger, Harvey Leibenstein was an unlikely rebel. Perhaps because of an aversion to conflict, he resigned from Berkeley after being repulsed by the campus turmoil of the 1960s (Perlman and Dean 1998, pp. 133-34). He gave no indication that he was intending to challenge conventional economics. Yet, his article set off a vigorous debate:

> Between 1969 and 1980, the article was the third most frequently cited in the Social Science Citation Index. However, ... much of this citation derived from attempts to explain X-efficiency theory away: it was under almost constant attack from much of the mainstream of the profession over that same dozen years. [Perlman and Dean 1998, p. 141]

Leibenstein argued that Harberger was only looking at the static price effect of monopolistic transactions. Granted, if a monopolist raised prices a little bit, the immediate effect might be small; however, by restricting himself to the marginal consequences of price changes, Harberger's model loses sight of the role of competitive pressures in promoting efficiency.

In contrast, Leibenstein directed attention to the productive side of the economy. According to Leibenstein, even if Harberger might be
correct that increased prices might do little harm, monopolies still reduce productive efficiency. In effect, Leibenstein's case rested upon one of the three conventional effects of monopoly, which Harberger dismissed; namely, that monopolies misallocate resources.

Leibenstein cited numerous studies of virtually identical plants producing quite dissimilar results. He postulated that economists needed to come to grips with the forces that account for the superior performance of some operations -- forces that conventional economic models do not capture.

Economists did not need Leibenstein to teach them about the harm done by monopoly. After all, a core concept of economics is that competitive pressures are the key to efficient production. This idea dates back as far as Adam Smith's dictum that "monopoly ... is a great enemy to good management" (Smith 1776, I.xi.b, p. 163). Alfred Marshall observed that business might not bother to maximize productivity until hard times come, when "manufacturers are put on their mettle and exert themselves to the utmost to invent improved methods and to avail themselves of the improvements made by others" (Marshall 1888, p. 92). The idea had become common among institutionalist economists that higher wages could "shock" managers to increase efficiency.

In this sense, the idea of X-inefficiency is not new. Precisely for this reason, Leibenstein's research assistants were able to compile numerous examples. One need only look at a textbook on industrial organization to find references to a host of studies that find
symptoms of X-inefficiency (see Scherer and Ross 1990, pp. 668-72). Herbert Simon, a proponent of the idea of organizational slack, devoted the final section of his Nobel Lecture to the subject. He credited Leibenstein along with Cyert and March (1992, pp. 41-44), who coined the term (Simon 1979, pp. 508-9). More specialized literature offers further confirmation. The ability of management to respond to the shock of new competition from imports provides additional evidence of prior X-inefficiency (see Scherer and Ross 1990, p. 669). Similarly, Borenstein's and Farrell's (2000) analysis of the rash of cost-cutting announcements in the petroleum industry in the wake of the 1986 oil price crash was symptomatic of an industry where X-inefficiency was endemic. On a more macroeconomic scale, Alex Field found evidence that the years of the Great Depression made up the most technologically advanced period in US history (Field 2003). Finally, the historical literature on large oligopolistic corporations went into great detail about managerial slack -- US Steel was a favorite example.

**Stigler to the Rescue**

While economists were unlikely to welcome Mundell's nightmare of their discipline sinking into irrelevancy, Leibenstein's assertion of dissimilar efficiencies had an even more uncomfortable implication. Virtually all schools of economics, ranging from monetarist to Keynesian, build their models and theories upon the key assumption that both individuals and businesses are maximizers. As a result, identical firms should produce identical outputs. Leibenstein,
however, claimed that the widespread existence of dissimilar productive efficiencies showed that many firms -- not just monopolies or oligopolies -- do not maximize.

Leibenstein's article unleashed a volley of criticism from George Stigler, who opened the debate by challenging Leibenstein with a caustically titled article, "The Xistence of X-Efficiency." Stigler began by acknowledging that Leibenstein's article was influential, by bemoaning the "awful name" that Leibenstein had chosen, and by promising to make the case that what Leibenstein called X-inefficiency "can usefully be assimilated into the traditional theory of allocative inefficiency" (Stigler 1976, p. 213).

The last promise is curious because he never explained the "traditional theory of allocative inefficiency." In fact, Stigler concluded his article by suggesting that such a theory would be virtually impossible.

The remark about the awfulness of the name suggested the stakes of the debate for Stigler: "[to] assume that monopolists do not maximize profits ... is an abandonment of formal theory, and one which we shall naturally refuse to accept until we are given a better theory" (Stigler 1976). Leibenstein, of course, used an X to point to something that could not be reduced to a formal theory.

On the surface, however, Stigler's vehemence in attacking X-efficiency might seem puzzling. After all, Leibenstein's article lent support to the proposition that barriers to competition -- barriers that most economists abhor, such as monopolistic practices or
regulation -- can be wasteful. Stigler himself was a constant critic of regulation. For Stigler, however, monopoly was a creature of government protection and regulation, rather than a natural result of market processes.

But then, Stigler had already opposed the idea behind X-efficiency long before Leibenstein proposed the concept. Almost three decades earlier, Stigler launched an epic debate about minimum wage laws (see Prasch 2007). Richard Lester found evidence that increasing minimum wages would not decrease employment. Lester, who had extensive experience in industry after having recently served as chair of the Southern Textile Commission of the National War Labor Board, used government data, as well as surveys of industry leaders, to make his case, which was at odds with the assumptions of mainstream economic theory (Lester 1944). Stigler first responded to Lester with an article attacking the theoretical underpinning of the Lester's work. For example, he dismissed Lester's appeal to the shock theory by insisting that shock effects were impossible because competition necessarily made low-wage businesses, such as textiles, efficient -- in effect denying the possibility of X-inefficiencies (Stigler 1946, p. 359).

In 1939, Stigler laid the foundation for another part of his attack on X-efficiency when he showed that flexibility needed to be taken into consideration in understanding efficiency (Stigler 1939). Without knowledge of the context, a static observation of a plant might give rise to a misleading interpretation of efficiency.
Although small, high-cost plants may seem inefficient when they sit idle, they may represent the least expensive approach to meeting demand during periods of peak capacity.

By the time that Stigler challenged Leibenstein, he added that firms, which seem to be in the same industry, were not necessarily producing the same goods, such as different quality tomatoes or a product that requires less shipping because of advantageous location. Ultimately, the notion of an industry depends upon an arbitrary classification. Cross product competition adds a further complication to an analysis of industrial organization. As a result, comparisons of the performance of an individual firm with other firms or in terms of some industry standard are meaningless.

Besides, firms are probably producing other non-marketed outputs, "including leisure and health" (Stigler 1976, p. 213). Stigler did not mean the leisure and health of the workers. Instead, he was referring to the well-being of employers, perhaps alluding to John R. Hicks's observation: "... the best of all monopoly profits is a quiet life" (Hicks 1935a, p. 8; 1935b, p. 265). In effect, Stigler was merely redefining what Leibenstein might see as X-inefficiency as something that conformed to neo-classical theory. In this way, he saw himself assimilating Leibenstein's observations into traditional economic theory.

Rescuing the Damsel of Maximization

In Stigler's indirect defense of Harberger, a single question of economic theory was central: do firms maximize? Yet, Stigler focused
Stigler demonstrated what Melvin Reder proposed as the central feature of Chicago economics: a stubborn adherence to its "tight priors" (Reder 1982). Stigler defended of his tight priors by demanding some sort of scientific delineation of industries as well as a multidimensional measure of output -- neither of which would ever be capable of precise measurement.

Stigler, in turn, argued for consideration of management's utility of work as a component of output. In doing so, Stigler violated the precepts of neoclassical economics in using the utility of work as part of his defense. After all, neoclassical economics is supposed to treat subjectivity only in so far as it can be inferred from transactions in the marketplace.

Donald McCloskey described Stigler reprimanding someone who strayed from this practice by "declaring loudly that all that mattered were the observable implications" (McCloskey 1994, p. 14).

Stigler was undoubtedly correct that CEOs do often take actions that trade off firm profitability for their own personal utility. For example, more recent research has shown that corporations underperform when their CEOs excel in golf and that the cost of corporate jets is higher when CEOs belong to country clubs far from their headquarters (Perelman 2007, p. 9).

In terms of efficiency, however, the effects of actions that raise managerial utility at the expense of the firm do not qualitatively differ from embezzlement. Certainly, diverting resources to personal
use does not constitute profit maximization -- the assumption that Stigler set out to rescue.

Such quibbles might be dismissed as irrelevant. After all, Stigler was out to rescue the concept of individual maximization. To do so, he was willing to contaminate the "purity" of neo-classical theory, just as an expert chess master sacrifices a pawn to save the queen.

Even so, Stigler's ultimate critique may be less than the sum of its parts. It came down to the proposition that nobody can scientifically define an industry or compare two different firms. Consequently, Stigler precluded the possibility of any legitimate discovery of X-inefficiency.

Any observation purporting to be evidence of X-inefficiency could then be dismissed nothing more than an anecdote. As Stigler is often credited with saying, "The plural of anecdote is not data."

In effect, Stigler was attempting to paint economists into a corner, such as Supreme Court Justice Potter Stewart found himself in his concurring opinion in Jacobellis v. Ohio (1964), where the court was ruling on the question of obscenity. Steward famously wrote: "I shall not today attempt further to define the kinds of material I understand to be embraced within that shorthand description; and perhaps I could never succeed in intelligibly doing so. But I know it when I see it."

Stigler tried to slam the door shut on anyone who might be inclined to follow Justice Stewart's example:

Unless one is prepared to take the mighty methodological leap into the unknown that a nonmaximizing theory requires,
waste is not a useful economic concept. Waste is error within the framework of modern economic analysis, and it will not become a useful concept until we have a theory of error. [Stigler 1976, p. 216]

In short, Stigler declared that unless economists can wrestle waste or inefficiency into a simple mathematical box, economists must not take such a "mighty methodological leap." So much for Stigler's "traditional theory of allocative inefficiency."

**Who Are You Going to Believe, Me or Your Lying Eyes?**

Marx's (not Karl) famous question brings to mind Chicago's notorious pattern of refusing to accept data that is inconsistent with its theoretical preconceptions. A former Chicago faculty member recounted how people who used data that called into question the prevailing theory would "be met by choruses of 'I can't believe it' or 'It doesn't make sense'." Milton Friedman's own Money Workshop at Chicago in the late 1960s and the early 1970s was a case in point" (McCloskey 1985, p. 140). One of Stigler's own collaborators described the Chicago attitude toward empirical analysis:

... if someone holds a view it cannot be dislodged by any conceivable empirical data. Evidence from a data system doesn't convince them. These people have made their decisions already. They've become true believers and no amount of empirical evidence will ever convince them by definition. [Kindahl 1997]

Pushed to extremes, data of virtually all sorts can be thrown into
question. For example, an article in the British Medical Journal humorously questioned whether parachutes really helped the "gravitationally challenged," calling for a double blind study (Smith and Pell 2003).

Of course, the leaders of the Chicago school were more than capable of effectively mustering data to support its own position, suggesting that data is used more as a rhetorical device than evidence.

In the case of X-inefficiency, this denial of the existence of differential inefficiencies seems to defy common sense. X-inefficiencies seem to be commonplace. They may defy theoretical analysis or even quantification, but examples seem to be everywhere.

Even, Harberger, based on his long career as a development economist, weighed in on the subject. Recall how he alluded to differential productivities in his 1998 Letter. In the same year, he devoted his presidential address to the American Economic Association to the subject of economic growth. He began, in effect, by downplaying the marginal perspective, which had been the centerpiece of his 1954 article:

Many, maybe even most, economists expected that increments of output would be explained by increments of inputs, but when we took our best shot we found that traditional inputs typically fell far short of explaining the observed output growth. [Harberger 1998a, p. 1]

Harberger gave numerous examples of the sort of productive improvements that fall through the usual net of economic analysis --
many based on his experience in Latin America. In his most telling case, he wrote:

I recall going through a clothing plant in Central America, where the owner informed me of a 20-percent reduction in real costs, following upon his installation of background music that played as the seamstresses worked. [Harberger 1998a, p. 3]

Harberger clearly that conventional economics is not particularly useful in finding the kind of music that might make the seamstresses work harder. Both Harberger and Leibenstein accepted the existence of intangibles that cannot be fitted into some abstract theory.

Similarly, in the world of team sports, people commonly speak of players' intangibles in a way that resemble the idea of X-efficiency. Despite unimpressive outward appearances and statistical records, some athletes have inexplicable intangibles that defy quantification.

The statistics-obsessed world of baseball offers an intriguing example. Shortly before Stigler's foray into X-efficiency appeared, Leo Durocher, a famed manager who had been guiding the Chicago Cubs about 11 miles away from the University of Chicago. Durocher later explained to a reporter why Eddie Stankey was his favorite player: "He can't hit; he can't run; he can't field; he can't throw. He can't do a goddam thing, Frank, -- but beat you" (Durocher 1975, p. 13). Other players, who are outwardly very impressive, are described as poison, meaning that their effect on others is destructive.

Perhaps economists could learn from Leo Durocher. Perhaps there were
even some Cubs fans in the Chicago economics department at the time.

**Fleas, Rabbits, and Elephants**

All the protagonists mentioned here -- Tobin, Leibenstein, Harberger, and Stigler -- had something important to contribute. Harberger created a model that supported Chicago-style antitrust theory. Tobin used the small dimension of the triangles to make the case that concerns about the level of employment were important. Leibenstein showed the limits of Harberger's approach by making the case for X-inefficiency. Stigler responded by warning that following Leibenstein's lead would complicate price theory.

The contributions of Tobin and Leibenstein -- that unemployment is costly from a macroeconomic perspective and that organizational slack common -- would probably strike any non-theoretical economist as self-evident. Jaroslav Vanek even pictured the respective dimensions of allocational, Keynesian, and X-inefficiencies as "fleas, rabbits and elephants" (Vanek 1989, p. 93).

Harberger, as a more policy oriented economist, (inadvertantly?) lent support to the ideas of both Tobin and Leibenstein. Stigler, in contrast, stubbornly refused to concede any legitimacy whatsoever to Leibenstein. Readers are free to decide, however, if demolition derbies are the proper venue for a search for truth.

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