*The case for increasing returns I:
'The Hicksian Getaway' and
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Frederic B. Jennings Jr.

Abstract: The case for increasing returns is accepted by most heterodox economists. Yet allegiance to decreasing returns in orthodox circles still endures directly and in the form of substitution assumptions. In forty short years from 1928 to 1968, beliefs shifted from Pigou calling rising cost 'inadmissible' to Alchian deeming decreasing returns 'a universally valid law' until Kaldor revived the case for increasing returns in the 1970s. How did these shifts of view occur? After Clapham opened the door and Pigou defined the orthodox stand, the 1930s debates swept through imperfect competition and many other issues into Keynesian disequilibrium theory. In 1939, 'The Hicksian Getaway' opened an Age of Denial leading to equilibrium theories based on substitution; then during the 1960s a second challenge to rising cost based on learning and technical change was defeated by 'The Hirshleifer Rescue' of decreasing returns and thus substitution in neoclassical theory. Why economists' substitution assumptions still hold sway is the focus of this study. First, the paper reviews 'The Hicksian Getaway' in its context and with respect to equilibrium models. Second, the paper analyzes and disproves 'The Hirshleifer Rescue' as an invalid argument based on a non-sequitur and thus simply asserted. Third, the case for increasing returns is developed into a theory of planning horizons supporting a generalized complementarity in economics. Some methodological implications are explored at the end.

Keywords: increasing returns, efficiency, cost, output, complementarity, equilibrium, planning horizons, production.

Introduction

Every organization – be it business, or social or ecological organization – has its formative moments, when patterns are set in place on which all its subsequent structures are built. Academic disciplines are certainly that way: intellectual frames of inquiry operate on assumptions selecting essentials, silent on everything else: no theoretical vantage sheds light on what it deems unimportant. These suppositions, so widely accepted, devolve into unconscious habit, defining the 'boxes' in which we all think. Casting aside these boxes – opening up to other assumptions – calls for reorganization of thought, or worse: a sort of 'scientific nervous breakdown' as Peter Earl (1983, p. 121) put it.

This is a part of what Clapham (1922) started in his seminal paper against the Pigovian classification of firms and industries into the 'empty boxes' of increasing vs. decreasing returns, which cannot be observed: there are no units of product, and economic gains from organization and due to invention are not kept distinct. Pigou (1922, pp. 134-35) responded that Clapham's 'boxes' are really 'valises' of smaller 'cases' which are what need to be filled, that help 'to expose the falsehoods of charlatanry...' Robertson (1924, pp. 144, 155-56) posed two causes of 'falling cost,' due to fixed inputs or invention/organization: 'The sole and sufficient explanation ... of increasing cost' was fixed inputs like land, though scarcity rents solve the problem (Knight 1924, Young 1913).

But two years later, Sraffa (1926, pp. 180-82, 194) explained that these 'laws' of increasing/decreasing return were related to aggregation and time, with increasing returns more relevant to narrow product domains and longer runs, supporting a view of firms in industry as 'competing monopolies.' At this point, Pigou (1927, pp. 193, 197; 1928, pp. 252-53, 256) came out with two papers on cost and supply, endorsing increasing returns and excluding the law of increasing supply price, saying that '... it is *impossible* for production anywhere to take place under conditions of increasing costs.' Consequently, Pigou concluded that '... *cases of increasing costs ... do not occur.*' A year later, reinforcing this view in even stronger terms, Pigou explained that:

> ... An increase in ... scale ... in general diminishes the average (and marginal) costs of the equilibrium firm... The law of decreasing supply price ... is not merely formally possible, but is likely... In actual life an industry ... cannot conform to the law of increasing supply price... Therefore ... the law of increasing supply price is excluded. From a cosmopolitan point of view it is excluded completely.

The implication for Pigou was that 'supply price cannot ... increase with increases of output. ... Only the laws of constant or decreasing supply price ... are admissible.' Pigou's sweeping dismissal of decreasing returns became the foundation of further discussions throughout the 1930s economic debates.

However, increasing returns suggested the need for industrial subsidy as a means to efficient pricing at marginal cost, according to Pigou (1912, 1920; also cf. Shove 1928 vs. Young 1913, Knight 1924, Robertson 1924). In a further analysis, Schumpeter (1928) raised stability issues, the inflexibility of fixed inputs in the short run, and the role of invention in any market process of growth. The well-known paper by Young (1928, pp. 230-33, 238) on increasing returns supported disequilibrium: '...the counterforces which are continually defeating the forces which make for economic equilibrium are more pervasive and more deeply rooted than we commonly realize. ... Thus change ... propagates itself in a cumulative way.' Furthermore, industry 'ought to be seen as an interrelated whole' in the analysis of economic growth.

The breakdown of Marshall's approach was speeded by a 1930 Symposium on 'Increasing Returns and the Representative Firm' with Robertson (1930) arguing in its defense against Sraffa (1930) and Shove (1930). A complex set of issues, subsuming the aggregation level of firms and industries, internal vs. external economies, equilibrium/disequilibrium models and the role of time were discussed at length. The upshot was, as Shove (1930, p. 116) concluded, 'that the economies of large-scale industry are ... positive and of considerable magnitude.' Harrod (1930) and Viner (1931) weighed in with attempts to rescue rising costs at least for short run equilibrium theory, where Viner offered a classification of various cost conditions. Harrod (1931; also cf. 1933) returned to the issue of falling cost, declaring that nonstandard products suggest that demand and supply are interdependent due to marketing efforts.

Joan Robinson (1932, 1933) added the notion of 'imperfect competition,' echoed by E.H. Chamberlin's (1927, 1933) writings on 'monopolistic competition,' showing an equilibrium outcome with declining cost and demand curves. Pigou (1933) responded approvingly with a formalization of Robinson's argument, while Shove (1933, esp. p. 121) objected to the notion that entry involved the movement of a short-run demand curve along a fixed short-run cost curve as illegitimate: '... the root of the difficulty lies ... in the fact that the costs of any given output both affect and are affected by the state of the demand.' The Robinson-Chamberlin books stemmed from Sraffa's and Young's work,[1] contending that these competitive forms show inefficiency as a result of 'excess capacity' as entry proceeds 'too far' for production at minimum average cost.

Lionel Robbins (1934, pp. 2-7) then turned to the fundamentals of cost theory, arguing that 'displaced alternatives' should be 'conceived ... in terms of *values* ... and not as technical *quantities...*' Defining 'costs of production' as 'a reflection of the strength of excluded demands' suggests that 'at the point of equilibrium, just as demand price will be decreasing, so will cost be increasing ... [due to] diminishing relative utility...' in accord with what he called 'Weiser's Law.' Assuming *ceteris paribus* assures diminishing returns, whereas allowing *mutatis mutandis* under a 'Theory of Variations' supports increasing and constant returns suppositions. Robbins said diminishing costs stem from external economies due to increased specialization through expanded division of labor and the presence of indivisible inputs, so placing them outside the realm of partial equilibrium models. Robbins (1934, pp. 15-18) closed with some comments on time, noting Marshallian short and long periods 'explain different *sections* of the same process,' which are related to expectations and 'the estimates of the future of the various producers concerned...'.

Kaldor (1933-34, 1934) wrote two papers on equilibrium and its surrounding conditions, suggesting the accuracy of foresight is central to its stability. His second paper addressed the 'entrepreneurial function' of firms, suggesting that 'the supply of "coordinating ability" *for the individual firm* is "fixed",' which makes the cost function dynamic and thus a feature of disequilibrium: 'the inherent tendency of the size of the firm to expand will be continuously defeated by the spontaneous "changes of data" which check it.' Kaldor (1934, pp. 70-72) found that '*long-period static equilibrium and perfect competition are incompatible assumptions*' and that 'the existing ... economic system ... is ... adapted to ... dynamic change and imperfect foresight'. Austin Robinson

(1934, pp. 256-57) developed this view, where firm sizes stem from a tradeoff between scale economies and managerial limits, supporting Marshall's framing of economics as a biological 'study of the adaptation of economic organizations to their environments.'

These discussions continued throughout the rest of the 1930s on imperfect/monopolistic competition and its implications. Keynes also published his *General Theory* in 1936, starting another related debate on macroeconomic phenomena and the process of cumulative change. The salient theme underlying the microeconomic debate was that increasing returns and declining costs, once accepted as true, were incompatible with efficiency, equilibrium and the competitive frame. Indeed the political implications should be acknowledged as well: [2] Lange (1938, p. 54) voiced them most clearly:

> The actual capitalist system is not one of perfect competition; it is one where oligopoly and monopolistic competition prevail. This adds a much more powerful argument to the economist's case for socialism. The wastes of monopolistic competition (he refers to the 'excess capacity' argument – FBJ) [3] have received so much attention in recent theoretical literature that there is no need to repeat the argument here. The capitalist system is far removed from the model of a competitive economy as elaborated by economic theory. ... Only a socialist economy can fully satisfy the claim made by many economists with regard to the achievements of free competition.

So these lofty economic conceptions seemed driven in part by political arguments about the optimal organization of social institutions, whether by markets or through command. A strong case was made tying increasing returns to inefficiency yielding a socialist claim that centralized setting of prices served better to mimic competitive firms than the free market did. Thus the case for increasing returns supported the socialist cause, setting the stage for an Age of Denial in economics due to 'The Hicksian Getaway.'

'The Hicksian Getaway'

After Pigou's (1927, 1928) two papers supporting increasing returns as a general phenomenon, deeming an upturning cost curve as simply inadmissible, the 1930s

discussions centered on how to incorporate this supposition into economic analysis, since static conceptions, stability, efficiency, equilibrium models and competition had all been declared incompatible with increasing returns. Socialism, market theory – all economic approaches – were embroiled in a debate about how economics should proceed and on what set of assumptions. Suddenly, into this situation, appeared a young economist with an economic conception allowing economists to escape from most of these unresolved dilemmas.

John Hicks, in *Value and Capital* (1939), walked away from the problem, making what must be considered one of the most outrageous statements in the whole history of economics. After reviewing the lessons of the 1930s debates on the conditions for rising and falling cost due to fixed (or rigid) factors – including entrepreneurial limits – Hicks (1939, pp. 83-85) said that: 'It seems to be agreed that this situation has to be met by sacrificing the assumption of perfect competition...' for some model of competing monopolies. Hicks continued:

> ... yet it has to be recognized that a general abandonment of the assumption of perfect competition ... must have very destructive consequences for economic theory. Under monopoly the stability conditions become indeterminate; and the basis on which economic laws can be constructed is therefore shorn away. ... It is, I believe, only possible to save anything from this wreck – and it must be remembered that the threatened wreckage is that of the greater part of general equilibrium theory – if we can assume that the markets confronting most of the firms with which we shall be dealing do not differ very greatly from perfectly competitive markets. If we can suppose ... that marginal costs do generally increase with output at the point of equilibrium (diminishing marginal costs being rare), then the laws of an economic system working under perfect competition will not be appreciably varied in a system which contains widespread elements of monopoly.

Then, with a refreshing honesty absent today in too much economics, Hicks sought to justify his 'getaway':

> At least, this get-away seems well worth trying. We must be aware, however, that we are taking a dangerous step, and probably limiting to a serious extent

the problems with which our subsequent analysis will be fitted to deal. Personally, however, I doubt if most of the problems we shall have to exclude for this reason are capable of much useful analysis by the methods of economic theory.

Period. End of section. The next starts with a sigh of relief for removing this annoyance so one can get to the work at hand: 'Let us, then, return to the case of perfect competition.' Thus Hicks simply dismissed the doubt that he himself had raised about diminishing returns as a limiting and 'dangerous step' for the ensuing analysis.

This was the end of discussion, tragically, due to the onset of World War II. After the war, Paul Samuelson (1947) founded his Ph.D. dissertation on the Hicksian frame, and shortly thereafter Arrow and Debreu (1954) extended general equilibrium theory on Samuelsonian grounds. That is how an established doctrine emerged from these shared assumptions and methods, supported by Friedman's (1953) unduly influential case against realism in economic constructions. Thus would orthodox economics enter an Age of Denial from which it has yet to fully emerge and embrace increasing returns. There was a moment during the 1960s when this emergence started, though it was all too soon derailed by 'The Hirshleifer Rescue.' The issue involved the role of time and learning in economics, and how this should be handled.

The intellectual context: Time and learning in economics

The apparent challenge to orthodox statics stemmed directly from a paper by Armen Alchian (1959) on a way to incorporate time in the theory of cost. Recall that time played a large role in the 1930s debates, and that other papers suggested how time might affect theories of dynamic cost. One of the first was by F.H. Knight (1921, pp. 186-87, 214-15), a year before Clapham's critique, clarifying Marshall's schema (e.g., cf. Frisch 1950):

> Great difficulties are met with in stating a clear and straightforward exposition of price theory because of the fact that the given conditions or data of the problem are so different according to the length of the time period which the explanation takes into account.

... The essential fact in economics is that different changes take place at different rates, that for certain time periods certain aspects of the situation may be assumed to remain unchanged, while for longer periods some of these will undergo change. The data or given conditions are different when different periods of time are under consideration.

After reviewing the difference of *ceteris paribus* from *mutatis mutandis*, Knight took up another 'phase of the problem' of short vs. long run theory in terms of issues of scale:

If it is true that a small output would naturally be produced by primitive methods while a larger one would justify a more elaborate organization with greater efficiency, it may well seem that the case is one of decreasing costs. ... A correct treatment of cost in relation to output should plot a complete cost curve for each method separately, extending from zero output up to one of indefinite magnitude... The significant part of the figure presents, therefore, not a curve of decreasing costs, but a series of curves of increasing costs at different levels. ... The substance of the matter is ... that if more efficient methods ... are available, the number of organizations in the industry will be reduced until all are on the most efficient scale.

Knight's 'main conclusion' in this paper anticipates Kaldor's (1934) point, '... that *decreasing cost with increasing output is a condition incompatible with stable competition...*' But the issue of dynamic cost was shelved during the 1930s debates in favor of static constructions.

In 1939, however, Stigler (1939, pp. 305-8, 310-12, 318-21) reopened this subject, with respect to fixed and variable costs, since

...one cannot uniquely define fixed and variable costs with reference only to time periods. At least two additional circumstances must be considered, the existing cost-price relationships and the anticipated movements of prices and outputs. ... This line of reasoning leads directly to the conclusion that time must be an implicit variable which affects the form of the production function. There is not a short run and a long run; rather there are continuous variations

in the marginal cost curve from very short periods to full, long-run equilibrium.

The foregoing list of considerations ... emphasizes the fact that short-run marginal cost curves form a rather extensive genus, each species of which is appropriate to a particular set of assumptions about technology and anticipations. ... Finally, once short-term alterations of plant are admitted, it is impossible to draw short-run cost curves with reference only to time periods. Each such cost curve is now subject to restrictions, not directly of time, but rather of a set of prices. The cost curves are defined for an interrelated range of prices...

It is possible to assume that such changes are continuous, and then a complete presentation (for any given set of price anticipations) involves a third axis, time, and the marginal and other cost functions become surfaces. ... but whether alterations of plant are continuous or discontinuous, it is no longer possible to handle the problem of the rate or extent of alteration by the use of plane geometry, since future prices are now important variables.

A year later, J.M. Clark (1940, pp. 241-43, 246-50) conveyed a parallel line of argument in his well-known paper endorsing a more realistic concept of 'workable' (over 'perfect') competition. In the course of this presentation, Clark considered the difference between short- and long-run curves:

I should like to point to certain ways in which long-run forces serve to mitigate the seriousness of the effects of imperfect competition. These considerations center largely in the proposition that long-run curves, both of cost and demand, are much flatter than short-run curves... ...In the field of imperfect competition ... these matters of degree are the essence of the problem. To develop the full importance of this it is necessary to take account of the time dimension of these curves...

One feature of this relationship might be expressed as a skewed surface or contour-map in which the vertical dimension measures price... What we may call the west-east dimension would represent the length of time during which a given price-relationship remains in effect; and the north-south dimension would represent physical volume of sales... The intersection of the surface with any vertical north-south plane would be a demand-schedule

representing the sales under various price differentials, each being assumed to remain in effect [over] a length of time represented by the distance of the plane from the west end of the diagram, where time equals zero. These curves would grow less steep from west to east. The horizontal contour-lines would be growth-curves (positive or negative) each representing the increasing effect with time of a given price-differential on volume of sales.

Such a surface would, however, represent only a beginning of analysis. Action by one producer would provoke responses by others... Changes in quality are ... not represented. ...The whole functional relationship is probably so complex as to defy mathematical plotting.

Fifteen years later, Clark (1955, p. 459) came back to the issue of adding a time-dimension to economists' cost and demand curves, suggesting it was still too simplistic:

A timeless two-dimensional demand curve of the conventional sort leaves out of account the fact that the effect of a given price, or price differential, on the volume of sales is a function, among other things, of the length of time during which it has been in effect. ... This time dimension ... means, among other things, that the effect of a given price on sales volume depends on the previous price or price situation, and that the curve is not fully reversible.

...The active variable is better described as a price policy than a price, and acts jointly with promotion... Similar comments apply to alterations of the product and moves in the area of sales promotion. This complex of variables would overload any possible system of graphic presentation. [4] A family of three-dimensional surfaces – the third dimension being time – with a different surface for each initial price or price situation, would still be a simplification.

Armen Alchian's 1959 paper appeared in this context, tracing nine propositions on cost as a means to incorporate time into production with a three-dimensional frame of the sort described by Stigler and Clark. Alchian tackled the problem as a relation of firms' 'equity cost' to the volume, output rate and period of a production run. He argued that equity cost – namely, the impact on company value – was a way to incorporate actual long-term factors into production decisions. Total equity cost (C) is a function of volume (V), output rate (X), production run length (m) and a planning interval (T): C

$= C(V,X,m,T)$. Alchian then transformed that cost relation to $\underline{A = A(V,X,T)}$, as V is the sum of X(t) over the interval $\underline{t = 0 \rightarrow m}$, allowing m to be dropped.

Alchian's propositions state that the equity cost of production turns on how runs structure volume in generating output through time: more rapid output rates (X) for a given volume (V_o) increase its cost, whereas more time (m,T) reduces its total and marginal costs of production. The key to Alchian's explanation is that a faster X for a given V_o means shortening m (and vice versa).

Interestingly, Alchian's model was seen as a threat to orthodox statics, in its suggestion that increasing returns would derive from augmenting V (given X_o) by lengthening m. Whether the marginal impact on equity cost (of raising output rate X for a given m_o) would rise or fall was moot, due to Alchian's suppression of m in A(V,X,T). This was the question of increasing vs. decreasing returns, and whether the marginal cost curve for a unit of output – given m_o – would turn upwards.

A year after Alchian's paper, another angle of vantage on the nature of cost and demand curves with respect to time was proposed that seemed to presage where research was headed on these economic dynamics. The oddity is that its author appears not to see its significance, while the discussant missed the point altogether! Margolis (1960, pp. 531-32) addressed 'sequential decision making under ignorance' where 'actions taken today alter tomorrow's information and thereby tomorrow's decisions,' so the firm must be concerned not only with profit but also with learning effects. On this frame, Margolis traced the relation of prices to planning horizons:

> The greater the uncertainty of marketability, the shorter will be the planning horizons and the greater will be the allocated costs per year. Therefore the greater the uncertainty, the greater will be the variable costs because of a reluctance to commit the firm to best processes and the greater will be the fixed costs because of a shortening of the planning horizon.
> The implications of the above are that the greater the ignorance of the market the higher will be the estimate of the costs and the more inelastic the estimate of demand. What price should a firm charge if it has hopes of later expanding its market? The higher the price the greater the expected short-run profits and the greater the sacrifice of expected information about the mass

market. The lower the price the more information it gains about the future market possibilities.

The comment on this paper reveals the blinding effects of 'boxes.' Farrell (1960, pp. 560-64) defended 'traditional theory' as 'perhaps the most satisfactory analytical tool the economist has yet produced.' The 'considerable achievements' of 'perfect competition theory' offer a context '... within which economists should view this specialized and novel research ... as attempts to extend the theory of profit maximization...' The issue missed by Farrell is that profit maximization does not work with increasing returns; organization and 'complex systems' theories supplant this approach with more realistic knowledge assumptions such as Margolis invoked with planning horizons.

A related theme on entrepreneurial learning with respect to production had also emerged with Hirsch's (1952) paper on 'progress functions' in manufacturing, which was further developed by Arrow (1962, pp. 155-56) with a paper on 'learning by doing.' Arrow began '... with the obvious fact that knowledge is growing in time.' Posing a need to assume exogenous technical change in neoclassical growth theory and production functions (despite that this answer is 'hardly intellectually satisfactory'), Arrow identified two empirical generalizations on the issue (Ibid.):

> (1) Learning is the product of experience. Learning can only take place through the attempt to solve a problem and therefore only takes place during activity.
> (2) Learning associated with repetition... is subject to sharply diminishing returns.... To have steadily increasing performance, then, implies that the stimulus situations must themselves be steadily evolving rather than merely repeating.

These developments simply extended Alchian's schema of propositions, suggesting attention was shifting into learning and knowledge approaches, in part to understand technical change and dynamic complexity issues. All was setting a stage for new research on how to embrace such phenomena, until The Hirshleifer Rescue stopped a lot of this work in its tracks.

So this is the intellectual context of 'The Hirshleifer Rescue' to which we now turn. The belief was that time itself is simply insufficient to embrace the full complexity of

economic decisions, which are based on expectations of future prices and the entire array of anticipated contingencies implicit in all choice. Stigler and Clark called this schema impossible due to its simplification of variability in this process. Margolis shifted the emphasis from external into subjective time by introducing the planning horizon, in nice accord with Robbins' (1934, pp. 15-18) concern with producers' anticipations and their 'estimates of the future' (as quoted above). A year before, Alchian (1959) had offered a framework for this approach, which Hirshleifer saw as a challenge.

'The Hirshleifer Rescue'

Hirshleifer (1962, pp. 235-38, 246) read the Alchian article as a threat to orthodox science, since it appeared to reject The Hicksian Getaway and equilibrium theory in its suggested endorsement of the case for increasing returns. Stating his goal explicitly as one of 'rescuing the orthodox cost function,' to show that 'the classical analysis is consistent and correct,' Hirshleifer added the following:

> Alchian asserts quite broadly that nothing can be derived from his or any other accepted postulate about the shape of [the marginal cost curve]. If true, that would be unfortunate, since we have considerable empirical ground for confidence in the one crucial property of the classical marginal cost [curve] – that marginal cost eventually begins to rise with proportionate expansion of [rate and volume of output]. ... Happily, it can be shown that this property does indeed follow from the Alchian postulates (with a weak addition), so that we can justify the accepted shape of the marginal cost curve in the orthodox theory of the firm within Alchian's model.

This 'weak addition' that Hirshleifer posed is simply one of defining $\underline{V = m_0 X}$ such that output volume V shifts in proportion to output rate X by holding the run length m_0 constant. This simple restriction allows an apparently costless simplification of Alchian's unconstrained definition of V as the integral over m of output rate X(t), which streamlined the problem enough for Hirshleifer's claim that unit costs would indeed turn up eventually – under his cost function $\underline{H = H(V,X,T)}$ – to warrant 'the powerful logic of the law of diminishing returns.'

This argument then was picked up by Oi (1967, pp. 590, 594) in a paper on 'progress functions.' Starting with Arrow's call for 'technical progress' as an exogenous source of explanation for economic growth in order to fit the facts, Oi identified progress functions as a dynamic concept with '... no place ... in the static analysis of neoclassical theory,' except in '... the inter-temporal planning of production.' Oi considered two other writings: the Hicksian model of *Value and Capital* (1939) and the Alchian (1959) propositions as recast by Hirshleifer (1962). On this basis, Oi argued that learning and technical change could be ignored in neoclassical theory, as these phenomena were already contained in the Hicksian framework – confirmed by Alchian and Hirshleifer – as intertemporal production functions. Because 'the neoclassical concept of factor substitution is ... obliterated by turning to progress functions,' [5] Oi rejected this approach:

> In order to deduce these propositions, Alchian and Hirshleifer both appeal to learning, experience and economies which derive from not having to rush production plans. Notice, however, that if these writers had adopted a Hicksian intertemporal production function and [my] two theorems ... all nine propositions are seen to be logical consequences of my modified dynamic theory of production. ... To sum up, a dynamic theory of production along the lines of Hicks provides us with an essentially neoclassical explanation for progress functions. ... To attribute productivity gains to technical progress or learning is, I feel, to rob neoclassical theory of its just due.

Consequently, any explicit treatment of learning phenomena in economics is not needed; these processes are already embedded in the Hicksian frame. Planning horizons and other such concepts simply are redundant; the neoclassical theory already incorporates all of that.
Later, writing an essay on 'Cost' for the *International Encyclopedia of the Social Sciences*, even Alchian (1968, pp. 319-20) took the Hirshleifer argument as an established truth:

> A general and universally valid law is that for every volume of output there exists an output rate beyond which the marginal cost with respect to rate always increases. This is commonly called the law of diminishing marginal re-

> turns with respect to output. ... Joint proportional increases in both the rate and the volume (over the given interval of production) will of course raise total costs. The effect on the cost per unit of product is not predictable except for "high" rates of output. ... [This situation] involves an increase in the rate of output as well as in the volume. These two work in opposite directions... The rate effect will dominate as programs with higher rates are considered. ... Average cost per unit of volume can be decreasing for small outputs. But as larger outputs are considered, the average cost will, beyond some output rate, begin to rise persistently and with increasing rapidity...

Alchian's statement describes precisely what Hirshleifer claimed to have proven directly about production technology. Yet this finding conflicts with Pigou and other 1930s arguments for increasing returns before the appearance of The Hicksian Getaway. In 1968 – a mere 40 years after Pigou's second paper – economists simply were relieved to have a solution to the imbroglio over increasing returns at last. The Hirshleifer Rescue of equilibrium models showed decisively that static cost curves should turn upward (for rising volume and output rates on a fixed horizon), thus excusing economists from any further heed to increasing returns, learning, growth and technical change in our theories of cost and demand.

Turvey (1969, pp. 285-88) offered a useful summary of this series of papers in which, after reviewing the 'traditional analysis' of fixed and variable factors in a distinction of short- from long-run theory, he observed that the older approach '... attempts to deal with time – with the length of runs – without adequately incorporating a time dimension.' Ambiguities in the theory of cost '... are dealt with very elegantly by Walter Oi in his recent Hicksian extension of the traditional analysis to a multi-period production function.' On this basis, Oi '... asserts the existence of two kinds of economies: economies of later delivery and economies of integrated output programs,' standing on Alchian's nine propositions. As Turvey put it:

> The fact that some of the propositions can be based either on the intertemporal planning of production or on learning and experience does not, as Oi recognizes, involve any contradiction. No general statement can be made about their relative importance. What does emerge in general terms is the importance of the time dimension and the resulting multi-dimensionality of marginal-cost

concepts. ... When uncertainty concerning demand is coupled with uncertainty in production, cost minimization ceases to be a simple concept. ... This review of a number of contributions to cost theory makes it clear that the definition of marginal cost as the first derivative of cost with regard to output is too simple to be useful. Both cost and output have time dimensions, and both may be subject to uncertainty. ...A cost analysis which is to be useful in decision-making needs to be historical dynamics, not comparative statics.

The ultimate outcome of this series of papers was that The Hicksian Getaway and The Hirshleifer Rescue achieved the status of General Laws. As a result, economists simply accepted neoclassical theory: everyone knew (and taught or learned) that average and marginal cost turned up, for high enough volumes and output rates. Thus economists paid no further regard to increasing returns. But the interregnum didn't last long: the early 1970s saw a revival of ferocious attacks on American neoclassical theory, mostly by European economists.

The winter of discontent: 1970 to 1977

The decade took off with a bang from Martin Shubik's (1970, pp. 405, 413-14) *Curmudgeon's Guide to Microeconomics*, a '... frankly partisan and ... biased view' in which Shubik condemned the Hicks/Samuelson equilbrium model as suffering from 'a pervading sense of sterility' and 'an overpowering aura of specious generality.' He opined that: 'The very power and elegance of Hicks' analysis may have set the subject back as far as it set it forward.' However, as Shubik cast the point: 'An exploration of a dead end can be extremely useful if we realize that it is a dead end, and why it is so.' He expected 'that a new microeconomics is about to emerge' which he characterized as 'mathematical-institutional-political economy.'
The next year, Janos Kornai (1971) published *Anti-Equilibrium*, a frontal assault on mathematical economics and equilibrium, making an unsuccessful effort to introduce systems theory. E.H. Phelps Brown's (1972) Presidential Address before the Royal Economic Society on 'The Underdevelopment of Economics' started out decrying the paucity of economic contributions to 'the most pressing problems of the times,' and called for 'removal of the traditional boundary between ... economics and the other social sciences.' At the end of the year, the first of a series of papers by Nicholas Kaldor

(1972) appeared, called 'The Irrelevance of Equilibrium Economics.' Kaldor's attack was sweeping, calling for 'a major act of demolition ... destroying the basic conceptual framework' in order to 'make any real progress.' In this seminal paper, Kaldor endorsed increasing returns as a general case and tied it to complementarity in a Keynesian frame. A subsequent treatment by Kaldor (1975, pp. 347-48) reinforced that view with this statement:

> The theory of general equilibrium ... starts from the wrong kind of abstractions and therefore gives a misleading ... impression of the nature and the manner of operation of economic forces. ...Economic theory regards the essence of economic activities as an <u>allocation problem</u>... This means that attention is focused on what are subsidiary aspects, rather than the major aspects, of the forces in operation. The principle of substitution (as Marshall called it) ... is elevated to the central principle... This approach ignores the essential complementarity between different factors of production ... or different types of activities ... which is far more important for an understanding of the laws of change and development of the economy than the substitution aspect.

This is the real significance of the case for increasing returns, that Marshall's principle of substitution must give way to complementarity as the dominant form of interdependence in economic analysis (Jennings 2008a). So what responses came from mainstream economists to these attacks? Hahn (1973; also cf. 1981) wrote a review of Kornai's book called 'The Winter of our Discontent' that was skillful, learned and rather revealing. He offered a psychological explanation for all the disaffection, based on age, bitterness and disappointment with the narrow rigor of orthodox standards in economics. Calling Kornai's 'one of the few grammatical voices ... amidst [this] noise,' he seized 'the opportunity to proceed with a coherent discussion.' Hahn called for a sharper focus on 'whether General Equilibrium Theory (henceforth GE) is a dead end or not', defending this 'intellectual experiment' as 'of very great practical importance' for the following reason:

> When the claim is made – and the claim is as old as Adam Smith – that a myriad of self-seeking agents left to themselves will lead to a coherent and efficient disposition of economic resources, Arrow and Debreu show what the world would have to look like if the claim is to be true. In doing this they

provide the most potent avenue of falsification of the claims. ... Such work is of great practical significance...

        Kornai regards GE "as useless as a real science theory." Since throughout he adduces empirical evidence to refute this theory, I take it that he really means that it is <u>false</u> as a theory of what the world is like. But then it cannot but be a "real science" achievement to have formulated a two-hundred-year-old tradition so sharply as to enable such an unambiguous verdict...

This seems a very peculiar retreat from pushing equilibrium models as the apex of economics, deeming them now a 'useful' dead end, divulging by their utter unrealism 'what the world would have to look like if [GE were] true' as 'the most potent avenue of falsification...' Had that been the aim of the effort, though, none would truly object. The point is that these economists saw GE as the *only* acceptable way to frame economic phenomena, in the spirit of Alchian's 'general and universally valid law ... of diminishing marginal returns' from which any departure – at least in the University of Chicago's graduate program – is 'penalized as evincing failure to absorb training' (Reder 1982, p. 19; also cf. Leontief 1982, p. 105).

Arrow (1974, pp. 26-29) also offered an oblique reference to the issue in a discussion of social agreements and demands in a series of lectures on organization in the early 1970s, although he makes no explicit connection to increasing returns. He calls 'conscience' 'essential in the running of society,' but as 'we cannot know all the effects of our actions on all other people' (a horizonal argument for bounded attention):

> We must limit our sense of responsibility to others to have any effective action at all. ... One's social, one's political attitudes, for example, must always reflect a certain degree of compromise with one's individual point of view. ...No social action is possible at all without some element of cooperation and, in particular, agreement.

Then Arrow proceeds to express some thoughts on the problem of social agreements, which so well apply to the issues at hand that it is hard to interpret them elsewise:

> It may really be true that social agreements ultimately serve as obstacles to the achievement of desired values... The problem is that agreements are typically

harder to change than individual decisions. When you have committed not only yourself but many others to an enterprise, the difficulty of changing becomes considerable. ... What may be hardest of all to change are unconscious agreements, agreements whose very purpose is lost to our minds. Some commitments are to purposes which involve much sacrifice and a very great depth of involvement. ... Even if experience has shown the unexpectedly undesirable consequences of a commitment, the past may continue to rule the present. ...This thinking ... gives rise to the greatest tragedies of history, the sense of commitment to a past purpose which reinforces the original agreement precisely at a time when experience has shown that it must be reversed. ...We must always keep open the possibility of recognizing past errors and changing course. [6]

A few years later, after receiving a Nobel Prize in 1972 for his work on *Value and Capital*, Hicks (1977, pp. v-vii) said he accepted this honor 'with mixed feelings' as it was 'work which I myself ... have outgrown.'

What I now think of <u>Value and Capital</u> is the following. The 'static' part ... opened up ... exciting [vistas]; so it was difficult when writing not to exaggerate their importance. Thus it was that ... I so preposterously exaggerated the importance of the perfect competition assumption, declaring that its abandonment would involve the "wreckage ... of the greater part of economic theory." ... In spite of all that has since happened to that particular piece of theory – the further elaborations at the hands of Samuelson, of Debreu and of so many others... – the time came when I felt that I had done with it. But what I really regretted was that it had played so large a part as it did in the ... 'dynamic' part of <u>Value and Capital</u>. ... Where I ... went wrong was in my attempt to represent ... equilibrium ... [by treating decisions simultaneously with their effects], so that an equilibrium could be reached. That however was nonsense. ... It was this device, this indefensible trick, which ruined the 'dynamic' part of <u>Value and Capital</u>. It was this that led it back in a static, and so in a neoclassical, direction. Since then ... I have endeavoured to avoid the relapse into statics [and] to keep my thinking more securely in time, concerning myself with processes...

So here we have Hicks, suggesting that the whole case for The Hicksian Getaway was regrettable 'nonsense' based on an 'indefensible trick, which ruined the dynamic part of Value and Capital.' If Oi's reconfirmation of the orthodox story is founded on 'the dynamic part of Value and Capital,' where does this leave The Hirshleifer Rescue and the case for excluding technical change and learning from economics? Hicks simply asserted decreasing returns against twenty years of debates supporting increasing returns, and forty years later retracted this ploy. Yet The Hirshleifer Rescue has never been disproved or reversed. During my own dissertation research in 1978 (Jennings 1985, esp. ch. 5), I encountered The Hirshleifer Rescue as the only technical argument that I could find for decreasing returns, so I examined the claim very closely.

The Hirshleifer rescue rebutted

First, recall Alchian's argument, that total equity cost C is related to V (production volume), X (the rate of output), m (the production run length) and T (a planning interval), so $\underline{C = C(V,X,m,T)}$. Alchian's nine propositions state that cost depends on how production runs structure volume with respect to speed vs. time: higher output rates (X) for a given volume (V_o) increase its cost, whereas taking more time (m) reduces V's cost of production. The key to Alchian's story is that a more rapid X for a given V involves shortening m, inviting Hirshleifer's simplification of $\underline{V = m_0 X}$, so V and X shift in the same proportion over run length m_0. The key to The Hirshleifer Rescue is an argument that – within this frame – marginal cost with respect to output rate X turns upward, justifying 'the powerful logic of the law of diminishing returns' which Alchian later accepted.

Why might all of this matter? Arrow (1969, p. 495) perhaps said it the best, that a theory of monopolistic competition 'is forcibly needed in the presence of increasing returns, and is superfluous in its absence.' Simon (1976, pp. 140-41) called imperfect competition 'the permanent and ineradicable scandal of economic theory' while Nelson (1976, p. 732) said that: 'Industrial organization is a field ... in deep intellectual trouble... [whose] source ... is that old textbook theory that we all know so well.' Kaldor (1972, p. 1240; 1975, p. 348) had called for 'a major act of demolition ... destroying the basic conceptual framework' by abandoning the 'principle of substitution' in favor of 'complementarity ... which is far more important.'

The whole question of how we do economics stands on the outcome of the case for increasing returns. But the Age of Denial persists on this subject, exemplified by Waldrop's (1992, p. 18) reporting of the reaction at UC-Berkeley to Brian Arthur's academic research on increasing returns:

> So there they had all been, sitting down to sandwiches at the faculty club. Tom Rothenberg, one of his former professors, had asked the inevitable question: "So, Brian, what are you working on these days?" Arthur had given him the two-word answer just to get started: "Increasing returns." And the economics department chairman, Al Fishlow, ...stared at him with a kind of deadpan look.
> "But – we know increasing returns don't exist."
> "Besides," jumped in Rothenberg with a grin, "if they did, we'd have to outlaw them!"
> And then they'd laughed. Not unkindly. It was just an insider's joke. Arthur knew it was a joke. It was trivial. Yet that one sound had somehow shattered his whole bubble of anticipation. He'd sat there, struck speechless. Here were two of the economists he respected most, and they just – couldn't listen.

The entire justification for the accepted approach – the competitive frame based on decreasing returns, substitution and scarcity models – stands on The Hirshleifer Rescue. Understanding – and debunking – this argument discredits all equilibrium models as simply irrelevant (just as Kaldor opined) as explanations for anything other than transient short-term phenomena. One important issue is that of framing conceptual issues. As Arrow (1982, pp. 5-7) explained:

> A fundamental element of rationality, so elementary that we hardly notice it, is, in logicians' language, its extensionality. ... It is an axiom [of economic rationality] that [a] change in description leaves the decision unaltered. The cognitive psychologists deny that choice is in fact extensional; the framing of the question affects the answer.

In an orthodox world of perfect knowledge – with free attention and full information – the representation of economic constructions shall not affect the result. But in a world of incomplete knowledge – where theory involves selective focus and attention is scarce

– how we frame things shapes how we understand them and may blind us to relevant truths. Some meaningful implications stay invisible from one view and yet will be found transparently obvious from another angle of vantage. Selective focus is also restrictive blindness at the same time. The opportunity costs of what we do or how we think also remain unknown or at least unobserved. This is a case for open-mindedness and for pluralistic conceptions (Jennings 2007a) to avoid the 'tragedies of history' against which Arrow (1974, p. 29) warned.

So as one examines the Alchian-Hirshleifer frame – especially Alchian's (1968, p. 320) summary that 'an increase in ... rate ... [and] volume ... work in opposite directions' along with Hirshleifer's (1962, pp. 235-36) comment that V is a stock and X is a flow while joining them anyway in his H(V,X,T) – one is prompted to wonder why the two stocks (V and m) were not kept together in this formulation. This seems sensible on its face, so that is the way I addressed the problem in my Ph.D. dissertation (Jennings 1985, ch. 5). Instead of fixing $\underline{m = m_o}$ as in Hirshleifer's model, I use $\underline{X = V/m}$ to convert Alchian's $\underline{C = C(V,X,m,T)}$ into $\underline{J = J(V,m,T)}$, where V can now be increased through changes in m and/or X. Joining V to m and T not only makes all of the arguments stocks (with X – as the flow – removed), but also makes time more explicit: $\underline{H_t = m+T}$ is the agent's time horizon. Hirshleifer's suppression of time (in accord with his static approach) effectively screens the impact of run duration on the conclusions so wrought. The introduction of $\underline{J = J(V,m,T)}$ allows a much clearer view of how Alchian's argument (that $\underline{A_{xx} > 0}$) rests solely on temporal length.

What Hirshleifer claimed to show was that dH/dX for any m_o (where X shifts in proportion to V) [7] eventually has to rise, since $\underline{d^2H/dX^2 = H_{xx} + 2mH_{xv} + m^2H_{vv}}$ is dominated by $\underline{H_{xx} > 0}$. This is because the other two negative terms are bounded above by zero (with H_x and H_v positive) when V and X rise together. Hirshleifer bases the positivity of H_{xx} on Alchian's $\underline{A_{xx} > 0}$. What is shown below is that Hirshleifer's d^2H/dX^2 does not limit to positive values as X is increased without limit, under Alchian's propositions.

Thinking of $\underline{V = mX}$ suggests another related advantage of framing with m and not X. A rise in X for a given V_o – Alchian's version – reduces m. But the functional form with V and m is open to increasing V via m and/or X (so dm/dV can appear anywhere on the interval $\underline{0 \leq dm/dV \leq 1/X}$). [8] Raising X for any V_o occurs by shortening m,

which is obscured in the Hirshleifer reformulation. Time should be at the center of focus and not placed out of view! As Arrow said, the representation of economic constructions shall affect the conclusions seen.

Hirshleifer's claim – that d^2H/dX^2 will limit to positive values as V and X rise in proportion for a given production run m_o – requires that J_{vv} turn upward, as $\underline{d^2H/dX^2 = m^2J_{vv}}$ if $\underline{V = m_oX}$. Hirshleifer bases this argument on the dominant role of H_{xx}, since the negative terms (H_{xv} and H_{vv}) will limit to zero in his expression for d^2H/dX^2 as V and X rise together. Reframing this claim with $\underline{J = J(V,m,T)}$ is very enlightening.

Transforming Alchian's $\underline{A_{xx} > 0}$ into the Hirshleifer format yields $\underline{H_{xx} = (m/X^2)(mJ_{mm} + 2J_m) > 0}$ which holds at all levels of V and X (with $\underline{J_{mm} > 0}$ and $\underline{J_m < 0}$). But this means that H_{xx} for a given V_o gets smaller as X increases since m is shrinking as well, implying that $\underline{J_m < 0}$ may come to dominate as $\underline{H_{xx} \rightarrow 0}$. In any event, this shows that dH/dX cannot increase without limit for rising X and V in proportion.

Hirshleifer argues that d^2H/dX^2 will limit to positive values as X and V rise in proportion, due to the influence of H_{xx}. But at all levels of X, $\underline{H_{vv} < 0}$, where $\underline{H_{vv} = J_{vv} + 2J_{mv}/X + J_{mm}/X^2}$. The higher X gets, the less will be the influence of the last two terms ($\underline{J_{mv} < 0}$ and $\underline{J_{mm} > 0}$), which forces J_{vv} to turn *negative* as output rate (X) increases. If so, then $\underline{dH/dX = mJ_v}$ cannot 'begin to rise persistently and with increasing rapidity' at high levels of X and V, as Alchian (1968, pp. 319-20) said was 'a general and universally valid law.' As $\underline{d^2H/dX^2 = m^2J_{vv}}$, neither can Hirshleifer's marginal cost turn upward: the limit (as X and V rise together) of d^2H/dX^2 has to be less than zero, implying a case for increasing returns. [9]

In sum, the higher the output rate X, the greater the dominance of J_{vv} over H_{vv}, which must be negative at all levels and combinations of V, m and X. In the limit, as X rises, $\underline{J_{vv} < 0}$. Since $\underline{d^2H/dX^2 = m^2J_{vv}}$, this makes Hirshleifer's proof false. Marginal cost for rising output on a given horizon cannot increase (even 'eventually'); unit cost falls, as A.C. Pigou taught us so long ago. Production volume may be enlarged through higher output rates and/or with longer production runs: this is why dm/dV can range between 0 and 1/X (from $\underline{m=m_o}$ to $\underline{X=X_o}$). The suppression of time in the Alchian-Hirshleifer model obscures its role. With the functional form of cost transformed to $\underline{J = J(V,m,T)}$, the impact of m on H_{xx} can be readily seen. So why was this specious argument not disproved a long time ago? My doctoral thesis (Jennings 1985, pp. 99-101), after

reviewing a much more complex and comprehensive version of this proof, finished the argument thus:

> [We have shown] nothing less than the following fact: that Hirshleifer's 'rescue' does not really follow from Alchian's statements at all! ... Hirshleifer's constant m will make $\underline{dm/dX = 0}$. Holding dm/dX strictly negative makes $\underline{J_{vv} < 0}$ a <u>necessary and sufficient</u> condition for there to be some constant dV/dX between 0 and m for which $\underline{d^2H/dX^2 < m^2J_{vv} < 0}$. Since Alchian's version leaves room for $\underline{C_{xx} < 0}$, a <u>stronger</u> requirement than needed for $\underline{J_{vv} < 0}$, Hirshleifer's argument is a <u>non sequitur</u>, even without the LeChatelier limit on A_{xx} once $\underline{V = mX}$ is assumed. Its status reduces to simple assertion, which flies in the face of an evident fact: unbounded increasing returns...
>
> A brief review is in order. Alchian's original goal was to offer a dynamic concept of cost. In his paper he makes an ordinal contrast of rate vs. time of production. He neither intended nor offered a statement on absolute changes in marginal cost; $\underline{A_{xx} > 0}$ for any fixed volume of output because it is cheaper to add a unit after the learning is done (through m) than by increasing output rate (widening X) while entrepreneurial skills are still growing! Nothing at all is implied about whether the latter will lead 'eventually' to an upturn in marginal or average costs. The strangest thing is that Alchian also accepted Hirshleifer's proof.
>
> The upshot of this grievous mistake is that any incorporation of learning by doing and technical change into cost and price theory has been deferred. The point lies in fifty long years during which we have painted a 'well-behaved' world, forestalling development of our conceptions in the direction of proper behavioral science. 'Hicksian getaways,' even redeemed, were supplanted by sanctions of rate over volume as justification for upturning cost. The limits of Hirshleifer's central contention could not have been checked very closely. The carelessness thereby implied is appalling, with how much we rest on this claim. After all, the error is not well-concealed to any skeptical eye. Its impact stretches well beyond sight, if his proof has diverted attention from learning. We cannot doubt that it has.

A methodological lesson

Arrow (1982, p. 7) said modern philosophers no longer believe in extensionality; descriptions mold decisions so the 'framing' of questions 'affects the answer.' As a result, 'the implications of information may change with alternative frames of reference.' How we think can matter a lot in determining what we do. The very first step in the argument, that $\underline{H_x = -(m/X)J_m > 0}$ which shows that the positivity of H_x stems totally from the decrease in m (because $\underline{J_m < 0}$ for any given V_o), implies that the role of m is important. This suggests, in turn, that this alternative form – that should have been checked – was not even considered.

There is a methodological lesson here in need of attention. Popper (1959, pp. 278-79; 1963) argued that the integrity of academic science stems from the self-policing character of 'conjecture and refutation.' He saw an open process of scientific inquiry as self-correcting, entailing a disciplinary endeavor to refute what is known:

> Science is not a system of certain, or well-established statements... We do not know: we can only guess. ... But these marvelously imaginative and bold conjectures or "anticipations" of ours are carefully and soberly controlled by systematic tests. Once put forward, none of our "anticipations" are dogmatically upheld. Our method of research is not to defend them, in order to prove how right we were. On the contrary, we try to overthrow them. Using all the weapons of our logical, mathematical and technical armoury, we try to prove that our anticipations were false – in order to put forward, in their stead, new unjustified and unjustifiable anticipations, new "rash and premature prejudices," as Bacon derisively called them.

If correct, this stirring image shall guard our inquiries against denial; such could only arise in a setting committed to pushing one's own ideas against all opposition. Alas, a recurrent theme behind the statements cited already is that economists are resistant to 'refutation' – to a point of denial – offering neither adequate justification nor reply to attack. As Mueller (1984, p. 160) said, 'neoclassical economics reigns supreme, not because it refutes challenges to it, but because it ignores them.' To claim that there are 'no credible rivals' (Hahn 1973, p. 129) or 'no satisfactory alternative to neoclassical theory' (Hart 1984, p. 189) is unacceptable on its face. Simon (1979, p. 510), in his Nobel lecture, responded to these spurious claims: 'There is an alternative. If anything,

there is an embarrassing richness of alternatives.' Earl (1983, p. 121) referred to Kuhn's view that senior scientists must die off for any successful reform as an 'entirely reasonable' attitude, despite that it conflicts with Popper's self-policing image of science.

Popper's benign depiction ought to be overthrown or replaced. The real facts show errors and intolerance in control, with orthodox science often denying credit to worthy alternative views; if so, one must dismiss his story as little more than a fanciful dream. Instead, we have to revamp Popper's naïve version of science into Polanyi's (1966, pp. 78-79) less fantastic conception of how research is conducted:

> I have spoken of the excitement of problems, of an obsession with hunches and visions that are indispensable spurs and pointers to discovery. But science is supposed to be dispassionate. There is indeed an idealization of this current today, which deems the scientist not only indifferent to the outcome of his surmises, but actually seeking their refutation. This is not only contrary to experience, but logically inconceivable. The surmises of a working scientist are born of the imagination seeking discovery. Such effort risks defeat but never seeks it; it is in fact his craving for success that makes the scientist take the risk of failure. There is no other way. Courts of law employ two separate lawyers to argue opposite pleas, because it is only by a passionate commitment to a particular view that the imagination can discover the evidence that supports it.

This seems to be a more valid description of how research proceeds. It also explains how wrong ideas can gain currency among advocates seeking to guard their human capital against any challenge from other constructs (whether right or unproven). Indeed, the rivalry of academic culture reflects a part of the problem. Maintaining that The Old Guard must die off for any reform is tantamount to admitting that academics is not about learning but about defending an orthodoxy against innovative vitality. Indeed, the prime directive of organization – of institutions or intellect – is self-preservation (Selznick 1948, pp. 268-70; Katz and Kahn 1966, p. 97). In any rivalrous setting, opposition is the game and total victory is the goal. This is in part the manifestation of substitution assumptions applied to improper realms where they have no place, since learning is a *complementary* process (e.g., cf. Boulding 1962, pp. 133-34;

Jennings 2008a). The final step in our revision of The Hicksian Getaway and The Hirshleifer Rescue will be a conversion of $J(V,m,T)$ into $M(Q,H)$.

Toward a horizonal model of cost

The Alchian/Hirshleifer refutation is only a step on the way to reconstructing cost and price theory along 'horizonal' lines. A proper model of pricing under conditions of increasing returns is 'forcibly needed' and not 'superfluous' (Arrow 1969, p. 495). Simply expressing $J(V,m,T)$ in horizonal terms shall open new avenues for economic analysis. The conversion is based on $\underline{Q = V/H_t}$ with $\underline{H_t = m+T}$, [10] where (since $\underline{dm/dT = -1}$ for a given horizon H_o) marginal cost $\underline{M(Q,H) = dJ/dV = J_V(V,m,T)}$. The first partials of $M(Q,H)$ are $\underline{M_Q = HJ_{VV}}$ and $\underline{M_H = QJ_{VV} + J_{Vm} < 0}$, where $Q(P)$ is a function of H^* (the agent's planning horizon). Here, J_{VV} can exceed zero but only if J_{VV} is less than $\underline{-(1/Q)J_{Vm} > 0}$, implying the upper bound of J_{VV} as Q rises for any H_o is zero in conflict with Hirshleifer's claim. If rising costs are 'excluded completely' (Pigou 1928, p. 253) then increasing returns prevail, with $\underline{M_Q < 0}$. The slope of the marginal cost curve (M_Q) is strictly bounded above by $\underline{-(H/Q)J_{Vm} > 0}$. [11]

What we have, from this conversion, is a marginal cost function $M(Q,H)$ that may increase – but will likely fall – with rising Q, and that declines with horizontal lengthening caused by 'horizon effects.' Here the time horizon H_t is distinguished from the planning horizon H^*, where the time dimension of foresight is subsumed by awareness in all dimensions (social, physical and ecological) leading to greater conscience, or what Arrow (1974, p. 27) defined as 'a feeling of responsibility for the effect of one's actions on others' (also cf. Jennings 2007b). As Simon (1983, p. 107) put it so well at the close of his lectures on *Reason in Human Affairs*:

> Reason ... is instrumental. It can't select our final goals... All reason can do is help us reach agreed-on goals more efficiently. ... It would be quite enough to keep open for our descendants as wide a range of alternatives as our ancestors left for us... In accomplishing [this] more limited goal, will an appeal to enlightened self-interest suffice? ... Success depends on our ability to broaden human horizons so that people will take into account, in deciding what is to their interest, a wider range of consequences. It depends on whether all of us

come to recognize that our fate is bound up with the fate of the whole world, that there is no enlightened or even viable self-interest that does not look to our living in a harmonious way with our total environment.

The question of where all this points – in terms of a reconstruction of economics in a 'horizonal' frame – is still open. Beginning with my dissertation (Jennings 1985), then at more length in recent years (cf. Jennings 2003, 2005, 2006, 2007abc, 2008abcde), I have attempted to outline some new directions in horizonal theory. The Alchian-Hirshleifer refutation, completed 25 years ago, appeared in my Stanford dissertation without prompting a murmur of interest. With orthodox science so defensive, fighting 'dissent' from alternative views (thereby excluding innovative vantages from attention), 'the underdevelopment of economics,' as Phelps Brown (1972) described it in his Presidential lament, should be no surprise. The problem is well described in Reder's (1982, pp. 17-19) study of Chicago's graduate program:

> Especially repugnant ... is the suggestion that price and marginal cost ... may vary independently ... [as under increasing returns – FBJ] ... Whatever their merits, such suggestions undermine the authority of neo-classical price theory... Let me elaborate: initiation to the Chicago sub-culture is through a rigorous training program in which failure is to many a distinct possibility, and placement in a well defined pecking order a concern of all. Success is achieved by mastery and application of certain tools and concepts to obtain correct answers... Correct answers must conform to definite criteria ... answers that violate any maintained hypothesis of the paradigm are penalized as evincing failure to absorb training.

One Nobel Laureate (Leontief 1982, p. 105), reacting to Reder's description, said this:

> How long will researchers working in adjoining fields ... abstain from expressing serious concern ... about the splendid isolation in which academic economics now finds itself? That state is likely to be maintained as long as tenured members of leading economics departments continue to exercise tight control over the training, promotion and research activities of their younger faculty members and, by means of peer review, of the senior members as well. The methods used to maintain intellectual discipline in this country's most

influential economics departments can occasionally remind one of those employed by the Marines to maintain discipline on Parris Island.

The sorry state that economics finds itself in today arises from a belief in decreasing returns supported by The Hicksian Getaway and The Hirshleifer Rescue. Once these costly obfuscations are replaced by increasing returns – as is already the case in much economics, especially outside America – then inquiries should reopen to a diversity of new approaches. Some of these are worth exploring in the conclusion to follow.

Conclusion

The 1930s were a formative time in economics, when a post-Marshallian synthesis started due to Clapham's (1922) seminal paper *On Empty Economic Boxes*. Marshall died in 1924, and shortly thereafter Pigou (1927, 1928), his protégée, set the stage for the 1930s debates by endorsing increasing returns as a universal truth. The ensuing exchanges show a time of ferment among economists, as a wide range of issues were raised and discussed throughout the discipline. Some have quoted Wordsworth (1805) on what those years were like for economists: 'Bliss was it in that dawn to be alive / But to be young was very heaven...' The field was open to new ideas, searching for resolution of fundamental lacunae in a flowering process of learning and growth. The brief review offered above identifies only a few of the many insights sown at that time.

Microeconomics emerged from Marshall, looking for resolution by analyzing economists' 'empty boxes,' and the 'valises' therein containing 'cases' in need of filling. Assumptions were raised, debated in terms of their ramifications and demarcations, then accepted or rejected diversely in ongoing conversations sweeping through every aspect of economics and social science. Setting acceptable limits and defining the boundaries of the field truly absorbed the attention of economists through these years, at least until 1939 and the brutal onset of World War II. It is unfortunate that the interruption occurred at that very moment; it ended discussion and drew attention away from what had been learned. The open issues surrounding increasing returns and its spreading implications simply faded from view. Once the war was over, economists sought to move ahead, trying to get beyond the irresolution of that turbulent time.

The Hicksian Getaway offered the opportunity to escape from all these unresolved dilemmas: Samuelson (1947) placed his *Foundations of Economic Analysis* on this frame, and then Arrow, Debreu and others simply followed his lead into equilibrium models showing competitive markets work, if under rather restrictive conditions. A great many honors were bestowed upon economists for advances in equilibrium theory and static conceptions structured thereon. The opportunity cost of this selective focal attention takes the expressive form of frustration with the narrowness of the economic questions posed and the answers sought. Too many economists have expressed similar views to those cited here.

But opportunity costs stay unseen, as the worth of what we forego. One can never return to opportunities spurned through choice; these are Roads Not Taken, to be forever lost. The new theoretical insights that might have flowed from more realistic constructions stay unknown. The 'alternative fields' of institutional, ecological, social and cultural economics have flourished, depicting how work could have proceeded under increasing returns suppositions. Additional work by Schultz (1993) and Arthur (1994) and a few edited volumes (e.g., cf. Buchanan and Yoon 1994; Heal 1999) suggest a resurgence of academic interest in increasing returns, but its full methodological and horizonal implications still remain mostly undeveloped.

The impact of The Hicksian Getaway and The Hirshleifer Rescue was to imbue rigid dogma with a false scientific credential, at the expense of open-minded debates such as occurred in the 1930s. The fragmentation of economics as a formerly integrated discipline is a result. The reason is a direct consequence of competitive frames, standing on substitution assumptions, scarcity and decreasing returns.
Instead of embracing complementarity, increasing returns and network models, static constructions still litter the field. Instead of addressing chaotic complexity in all its unfolding cumulation, narrow equilibrium models and partial analyses steer economics away from pressing concerns. Instead of forcing economists to admit that choice is not determinate, the positivist definition of 'science' says testability is the only acceptable justification for inferential claims. Instead of framing conscious awareness into all models of human decision, many economists still abstract away from bounded attention into rational expectations, perfect knowledge and full information assumptions. Such suppositions state the conditions under which our results apply: any unrealism in assumptions will limit the applicability of findings so derived (Jennings 1968, ch. 1).

The world is one of increasing returns save for short-term, partial analyses. So any economic construction built on decreasing returns should not be used to guide decisions, since it will squander resources somehow. We need to fit our analyses to the prevailing conditions at hand to rely on the outcomes so revealed. Otherwise, we run a risk of 'knowing not what we do.' A good example was already mentioned.

If education is suffused with complementary interdependencies, substitution does not apply: incentives structured on a competitive frame must fail in this setting. Cooperation is the efficient institutional form in the presence of positive feedback systems such as suggested by complementarity. If embracing increasing returns will make 'complementarity ... far more important' than substitution in economics (cf. Kaldor 1975, p. 348), so will the case for competition collapse in favor of cooperation as the optimal organizational form for human society.

This shows a core reason for the failures suggested here: rivalry in academics is simply counterproductive in the presence of dominating complementarities in education. The failure reaches from theory into our institutional frames. The opportunity costs of what we do remain unseen, except through a theoretical lens. Suppositions – set apart from the facts – lead us astray.

All this is so, even without any mention of planning horizons (H*). As one of the aims of this paper is to point the way to a novel approach, it is appropriate to address some 'horizonal' implications. As noted above, with marginal cost defined as M(Q,H) [12] – where $\underline{M_H < 0}$, and $\underline{M_Q > 0}$ *only for short horizons* which, extending, open the cost curve until $\underline{M_Q < 0}$ – the case for increasing returns and complementarity is strengthened. But there is another aspect of horizonal interdependence – still largely unknown and underdeveloped – that reinforces the case against substitution in favor of complementarity.

Your planning horizons interact directly with mine. Horizon effects spread contagiously across social space. If so, when I become more intelligent and predictable in my decisions, you can plan better too: I thus stabilize (as a disturbance term) in the causal projections on which you base your actions. Equally, if I 'lose my cool' and my confidence in what I know – or if my decision environment grows less certain – my

collapse of planning horizons will likely influence you. Horizons shift together, radiating outward to others. Such phenomena – i.e., the interdependence of horizon effects – show 'interhorizonal complementarity,' and they appear in many arenas unexplored by economists. These are but a few indications of many new research opportunities stemming from planning horizons and their economic effects.

Substitution, decreasing returns – The Hicksian Getaway and The Hirshleifer Rescue – offer reasons why new approaches have not emerged. An economics standing on increasing returns will differ in many ways still undiscovered. There is work to be done to repair the damage of false suppositions on economic constructions and to revise 'habits of thought' (Veblen 1898) along with our many unconscious assumptions. So once again, by instigating conversation among economists – with an open mind and a thirst to learn and thus to increase understanding – could there be another formative moment in economics to come? One can only hope...

Endotes

[1] Allyn Young was E.H. Chamberlin's principal dissertation adviser at Harvard in 1927.

[2] Also cf. Mises (1920), Lange (1934, 1936), Lerner (1934, 1939, 1977) and responses by Hayek (1935, 1940).

[3] Some words about the 'excess capacity' argument on which Lange's case stood appear appropriate here. The 'excess capacity' claim – associated with imperfect and monopolistic competition – derived from three errors in need of further attention. The first was that entry is sensitive to profits and not to prices (such as with 'limit pricing' phenomena), although market prices are more observable than firms' internal profits (Bain 1965; Jennings 1968, ch. 3). Second, the argument does something quite illegitimate as Shove opined above: it tells a long-run story about demand (by moving a short-run demand curve) while leaving the short-run cost curve fixed! Tangency with a long-run cost curve finds no inefficiency along its envelope of lowest-cost outputs. Furthermore, a third problem is that the 'excess capacity' argument also ignores the defining characteristic of this market, namely a preference for product diversity over

greater standardization. Only with wholly uniform products will minimum average cost be efficient: the very meaning of product diversity is that consumers are willing to pay a bit more in exchange for product variety (as a means to a better fit of features to individual needs). If so, entry yields more variety, in accord with consumer demand, for which we are willing to pay: the 'welfare ideal' in this situation no longer occurs at minimum cost but (arguably) at the tangency point, due to brand loyalty issues (also cf. Dixit and Stiglitz 1977). In a later revisit to his theory, Chamberlin (1951, pp. 56-57) said that the 'excess capacity' argument

> ...indicates nothing so much as a complete misunderstanding of the problem. ...When the product is recognized to be heterogeneous ... the general system of consumers' preferences now embodies sloping demand curves for the individual firms as an expression of the general desire for diversity which now must be recognized as coordinate with efficiency in production in defining the welfare ideal.

[4] Clark commits an epistemological error in this statement: he cannot deem a solution impossible just because none has emerged.

[5] Stigler (1951), pp. 140-44, for example, represents the process of firms' growth with respect to factor substitution by simply asserting separability and substitution over complementarity of productive functions in the following manner:

> For our purpose it is better to view the firm as engaging in a series of distinct operations. ... The costs of these individual functions will be related by technology. ... Certain processes are subject to increasing returns ... other[s] ... to diminishing returns...
> Our ... assumption, that ... the functions are independent, is ... important. Actually, many processes will be rival ... Other processes will be complementary... If, on balance, the functions are rival, then usually the firm will increase its rate of output of the final product when it abandons a function; and I think that this is generally the case.

Alternatively, Nelson (1981, pp. 1053-55) explores the growth implications of factor complementarity in this way:

> If factors are complements, growth is superadditive... The growth of one input augments the marginal contribution of others. Where complementarity is important, it makes little sense to try to divide up the credit for growth, treating the factors as if they were not complements. . . . [It is like] dividing up the credit for a good cake to various inputs. ... In short, there are not neatly separable sources of growth, but rather a package of elements all of which need to be there.

[6] Especially cf. the beautifully sensitive and revealing comments by Tannenbaum and Hanna (1985, pp. 99-103, pp. 118-21) on the psychology of 'hanging on' and 'letting go,' as quoted in Jennings (2008b, pp. 14-15, notes 20 and 21).

[7] The difference between Alchian's use of V as the sum of X(t) over m, and Hirshleifer's $V = m_oX$ is specified by the LeChatelier Principle, which says that $A(V,X,T) \leq H(V,X,T)$ as the latter relation is subject to another constraint not on the former, all other things equal. If we keep this in mind, then the difference between the two formulations is indeed trivial and immaterial.

[8] When $dX/dV = 0$, any increase in V is achieved by increasing m (such that $dm/dV = 1/X$). When $dX/dV = 1/m$, an increase in V is wholly through X, with $dm/dV = 0$ (which is Hirshleifer's case).

[9] We simply compare four cost formulations. The one Alchian offers to us is $\underline{C = C(V,X,m,T)}$. On the assumption that $\underline{V = mX}$, we can translate Alchian's formulation into: $\underline{A = A(V,X,T)}$. Hirshleifer's version shall be expressed as $\underline{H = H(V,X,T)}$ with $\underline{V = m_oX}$, and my reformulation is: $\underline{J = J(V,m,T)}$. The first thing to show is how Alchian's proposition that $\underline{A_{xx} > 0}$ (with $\underline{V_o = mX}$) can be restated thus:

$$d^2A/dX^2 = A_{xx} = m^2/X^2 \, J_{mm} = C_{xx} - 2m/X \, C_{xm} + m^2/X^2 \, C_{mm} + m/X^2 \, C_m > 0 . \qquad (1a)$$

Note the formulation of $\underline{A_{xx} = m^2/X^2 \, J_{mm}}$ gives strong indications that the positivity of A_{xx} stems from a decline in m and has little or nothing to do with extensions of X. This is also implied by the fact that $\underline{J_m < 0}$ where $\underline{A_x = - m/X \, J_m > 0}$. It is noteworthy that $\underline{C_{xx}}$

> 0 is not required by $\underline{A_{xx} > 0}$. As long as $\underline{2m/X\ C_{xm} - m^2/X^2\ C_{mm} < m/X^2\ C_m < 0}$, then $\underline{C_{xx} < 0}$ is possible as long as:

$$\underline{2m/X\ C_{xm} - m^2/X^2\ C_{mm} - m/X^2\ C_m < C_{xx} < 0}, \qquad (1b)$$

while $\underline{d^2A/dX^2 = A_{xx} > 0}$. It is worth noting that $\underline{C_{xx} < 0}$ is a sufficient condition for increasing returns (where $\underline{d^2H/dX^2 = m^2 J_{vv} < 0}$). Now we look at Hirshleifer's statement that $\underline{d^2H/dX^2 = H_{xx} + 2mH_{xv} + m^2H_{vv}}$ limits to H_{xx} as V and X grow large in proportion. Hirshleifer's version of d^2H/dX^2 can also be expressed in the form of J and C thus:

$$d^2H/dX^2\ =\ H_{xx} + 2m\ H_{xv} + m^2\ H_{vv}\ =\ m^2\ J_{vv}\ =\ C_{xx} + 2m\ C_{xv} + m^2\ C_{vv}\ , \qquad (2)$$

assuming that $\underline{V = m_0 X}$. The reason that $\underline{H_{xv} + m\ H_{vv} < 0}$ as X and V rise in proportion is that the wider the X, the less is m reduced per unit of X for a given $\underline{V = m_0 X}$. This is why $\underline{H_{xv} + m\ H_{vv}}$ limits to zero as X increases for given m_0, and it has nothing to do with upturning cost for $\underline{V = m_0 X}$! The positive sign of J_{vv} is simply asserted, not proven. Now we show how increasing returns ($\underline{J_{vv} < 0}$) does not conflict with Alchian's statements as reflected in line (1) above, for any and every value of V and X. By placing Hirshleifer's frame into Alchian's setting, i.e., where $\underline{V_0 = mX}$ so $\underline{dm/dX = -m/X}$, we find that:

$$d^2H/dX^2\ =\ H_{xx}\ =\ A_{xx}\ =\ m^2/X^2\ J_{mm}\ =\ C_{xx} - 2m/X\ C_{xm} + m^2/X^2\ C_{mm} + m/X^2\ C_m\ >\ 0. \qquad (3)$$

But this expression has nothing to do with the sign of J_{vv}. The positivity of H_{xx} and A_{xx} is from $\underline{J_{mm} > 0}$, from cutting m when $\underline{V = V_0}$; it does not commit to rising cost (from any new V for a given m_0), which is set by the sign of J_{vv} (which will likely be less than zero).

[10] I am indebted to Robert G. Wolf, a former colleague at Tufts, for suggesting this conversion to me after one of my presentations.

[11] Jennings (1985, pp. 101-6). Here is a brief sketch of the technical argument. First, I assume the partition of H between planning (T) and production (m) periods is made to minimize cost such that: $\underline{J_T = J_m}$ with $\underline{dm/dH = 1 - dT/dH}$ and $\underline{dm/dT = -1}$ (for any

given horizon H_o). Then marginal cost $M(Q,H)$ is the first derivative of $\underline{J = J(V,m,T)}$ with respect to V for any H_o, i.e.: $\underline{M(Q,H) \equiv J_v(V,m,T)}$. The first partials of $M(Q,H)$ can then be stated thus:

$$M_Q = H\, J_{vv} \quad \text{and} \quad M_H = Q\, J_{vv} + J_{vm} < 0 \ . \qquad (4)$$

J_{vv} can be greater or less than zero, reflecting rising or falling marginal costs of producing Q or V for a given H_0. However, if $\underline{J_{vv} > 0}$, it has to be less than $\underline{- 1/Q\, J_{vm} > 0}$ as well. This suggests that the limit of J_{vv} as Q gets large cannot be far above zero, in contradistinction to Hirshleifer's claim. Although the case for rising cost per unit, $\underline{M_Q > 0}$, rests solely upon unfounded assertions – at least on purely technical grounds (as opposed to horizonal long- vs. short-run issues already outlined) – the signs of M_Q and J_{vv} remain undetermined functions of H. More relevant is that the slope of $M(Q,H)$ with respect to Q is a function of both Q and H, where $\underline{dM/dQ = M_Q(Q,H)}$ for a given horizon. The impact of horizon effects on $M(Q,H)$ is a part of this story, where (for a given $\underline{V = V_o}$):

$$dM/dH = M_H + M_Q\, dQ/dH = M_H - Q/H\, M_Q < 0 \ . \qquad (5)$$

The marginal cost curve may turn upward (despite the absence of evidence and theoretical ground for the view), although – if true – rising cost is attributed to the effect of short horizons and not to production technology. And rising cost – when occurring at all – is tightly constrained, due to the negativity of M_H: M_Q must be less than $\underline{- H/Q\, J_{vm} > 0}$, so as Q rises for some H_o, M_Q will likely decline.

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